Three essays on economics and information shocks

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Abstract

A person living in an industrialized society has almost no choice but to receive information daily with negative implications for himself or others. His attention will often be drawn to the ups and downs of economic indicators or the alleged misdeeds of leaders and organizations. Reacting to new information is central to economics, but economics typically ignores the affective aspect of the response, for example, of stress or anger. These essays present the results of considering how the affective aspect of the response can influence economic outcomes.

The first chapter presents an experiment in which individuals were presented with information about various non-profit organizations and allowed to take actions that rewarded or punished those organizations. When social interaction was introduced into this environment an asymmetry between rewarding and punishing appeared. The net effects of punishment became greater and more variable, whereas the effects of reward were unchanged. The individuals were more strongly influenced by negative social information and used that information to target unpopular organizations. These behaviors contributed to an increase in inequality among the outcomes of the organizations.

The second and third chapters present empirical studies of reactions to negative information about local economic conditions. Economic factors are among the most prevalent stressors, and stress is known to have numerous negative effects on health. These chapters document localized, transient effects of the announcement of information about large-scale job losses. News of mass layoffs and shut downs of large military bases are found to decrease birth weights and gestational ages among babies born in the affected regions. The effect magnitudes are close to those estimated in similar studies of disasters.

Contents

List of Tables

List of Figures

Overview

Discussion of the motivation

Each of these three papers reveals a new relationship between variables of long-standing economic interest. Two of them show links between mass job losses and government policy, on the one hand, and health at birth, on the other. The first paper shows how the introduction of a new media technology influences third-party punishment and the relative success of different non-profit organizations. The existence of these relationships was in each case suggested by considering how new pieces of economic information (information shocks) could generate strong emotional reactions. In one case the health and psychology literature said news of job losses should generate stress in the people near the site of job losses, which should in turn decrease birth weights and gestational age. In the other case, the research on punishment, anger, gossip, and communication suggested that the deployment of social media technology could increase punishment. It should be noted that in none of the studies is the emotion itself the direct object of study. First, data on the emotions themselves is valuable but often not available. Second, that data is not always necessary to make a connection of economic interest. The approach of using the literature on emotion to make a connection between non-emotion data is the one used profitably in these papers.[1]

[1] Other economics papers use a similar approach. One example is the study of clock games by Kang et al. (2010), in which the basic features of anxiety are used to predict how a game's form will affect behavior in the game. Standard economic theory predicts no effect, but a clear prediction about the direction of the effect can be obtained by considering anxiety to be a psychological flow cost that is incurred as long as some salient economic uncertainty remains unresolved. That prediction can be obtained whether or not some otherwise valuable, emotion-related measurement is obtained during the experiment. Studies comparing the strategy method and direct-response method rely on similar thinking (see, for example, Brosig et al. 2003).

A second example is the model of surprise and suspense developed by Ely et al. (2013). They suppose that an agent has preferences over his own experiences of the emotions of surprise and suspense. However, the authors suggest testing this model by examining the relationship between the design of a surprise-suspense good—a book, movie, or sport—and the consumption (audience) of the good.

Economists have studied emotion before[2], but, as noted by Loewenstein (2000), they have focused on the anticipation of emotions to be experienced in the future. Loewenstein (2000) advises economists to pay more attention to what he calls immediate emotions (or visceral factors), especially negative ones. Such emotions include those considered in these papers.

Loewenstein (2000) argues that the dominance of visceral factors in human behavior is one of the most important reasons that economists are so *reluctant* to consider them. Visceral factors can override deliberation to generate behavior that is contrary to long-run self-interest. He prescribes models with state-dependent preferences. This scheme allows, for example, an otherwise money-interested person to become angry over another's unfairness and prefer to throw away his own money simply to hurt the offender. However, even in situations where no economic model exists, we can still propose and test important economic relationships or develop simple empirical models by considering emotional reactions.

Emotional reactions and economic news fit together in ways that give the researcher several advantages. These advantages add up to tell the researcher when and where to look for some interesting behavior, what that behavior should be, and that, with some luck, there should be a lot of it.

First, the emotional reaction occurs at a time that can be identified with a useful degree of precision. That is, the reaction occurs just after the information arrives because emotional reactions occur quickly and without effort or volition.[3] Emotional reactions are also transient, so they can be reasonably assumed to begin and end at some point. This advantage is exploited in the two chapters on health and job losses.

Second, the emotional reaction to any given type of situation or information is fairly regular (Frijda 1988). For example, being cheated out of a desired goal almost invariably gives rise to anger. Therefore, the type of emotion experienced by the subjects of research may be understood by generalizing findings from related research and, to some extent, by introspection or theory of mind. In addition, those people who experience an emotion often express it openly. The third chapter makes use of data on lobbying messages sent by community members to policy makers, which provides information about the intensity and valence of the emotional reaction to news about

[2] In particular, the early economists Jeremy Bentham and Adam Smith devoted substantial attention to emotion.

[3] However, there is always the possibility that some people may be privy to important information before it becomes publicly known.

a local change in economic policy.

Third, emotions have well-studied and regular behavioral and physiological effects (Frijda 1988). For example, anger gives rise to the desire to hurt the agent whose behavior triggered the anger. This regularity, which effectively characterizes emotions, is referred to as an "action tendency" or "action readiness." The experience of stress is accompanied by an extensively-studied physiological reaction discussed in chapters 2 and 3. The advantage of the regularity in both the input and output sides of emotion is that the researcher can consider a helpfully stable mapping between the information shock and observable outcomes (behavior or otherwise).

Fourth, the combination of the first three advantages gives the researcher some justification for suspecting that the reaction will be present in a detectable amount. Due to the speed and input-regularity of emotions, the same form of emotional reaction will often be experienced by a large group of people at the same time. In addition, the reactors often tend to have the same interests and characteristics (common environment and homophily). For example, everyone in a town has an interest in the general economic conditions of the town. Alternatively, some people may express their reaction and persuade others to agree with them (social influence). These factors tend to concentrate the effects of the emotional reaction, or even amplify the effects, which may make the effects easier to detect and study. This last advantage plays a key role in all three chapters.

These advantages serve as a counterargument to the view that emotions are unimportant because they are transient. One piece of the counterargument, already articulated by Loewenstein (2000), is that visceral factors can influence people to take "extreme actions" (p. 429). But, potentially more importantly, emotional reactions may also influence many people to all take an extreme action at the same time. The regular input-output mapping involved in behavior, along with correlated individual characteristics, means that any emotionally-charged broadcast to a group of people is likely to generate highly correlated behaviors. In one such example, Deaton (2012) suggests that during the recent financial crisis, the stock market indices may have been an especially salient form of information about economic conditions, which then generated a strong correlation between market movements and self-reported, subjective well-being as measured by daily surveys. Social interactions may also serve to amplify and prolong the reaction. Finally, the chapters on birth outcomes suggest that even transient responses may have permanent effects on the next generation. These points show that emotional reactions may be especially important when an authority considers how it will release (broadcast) information to a large audience.

The remainder of the section discusses some additional and more domain-specific ways in which incorporating emotional reactions contributed to the papers.

The second and third chapters were first inspired by an observation made by Matthew Rabin in his short course at Caltech in 2011. He commented that in economic models the loss of a job has almost no effect on the worker's the present discounted value of lifetime earnings yet in reality many people are terrified of job loss. This same puzzle is tackled in an extensive study by Davis and von Wachter (2011). They show that even very recent, sophisticated models of the labor market generate earnings losses far less than those seen in actual data. Using representative survey data, they also document that workers' worries about their economic futures are highly attuned to economic conditions. Davis and von Wachter's (2011) take-away is a great disconnect between the standard models and the attention that unemployment receives: Unemployment is of little consequence to a worker in a model, but it is among the most concerning issues in politics and economics. Along the same lines, the strength of the emotional reaction to unemployment should be a warning to anyone who would discount the costs of job loss, even if the emotion is regarded as an inconsequential by-product—where there's smoke, there's fire.

The notion that something about the costs of job loss failed to add up is not new. In his short paper "The Private and Social Costs of Unemployment," Feldstein (1978) worked through a simple numerical example in which he shows that, due to tax policies and benefits, unemployment would have only a small negative effect on the *net* income of a worker. Regarding this "low private cost of unemployment" (p. 156) as proven, he speculated that unemployment would be higher if not for some puzzling force (possibly, social norms) dissuading people from taking transfer payments.

However, at that time research on the links between economic conditions and physical and mental health was well under way. The short review "Health and Social Costs of Unemployment" (Liem and Rayman 1982) covers the research in the 1970s and early 1980s with particular attention on stressful life events and mental health. This line of research grew into a very large literature with contributions from researchers in economics, health, and psychology. A recent meta-analysis included over 300 studies on unemployment and mental health outcomes such as distress, depression, anxiety, and subjective well-being (Paul and Moser 2009). Stress features prominently in Sullivan and von Wachter's (2009) report of enormous effects of job loss on mortality. Thus, aside from the debate over the magnitude of earnings losses, this research indicates that there are substantial psychological and physiological costs to job loss.

Finally, we turn to the chapter on punishment and social media. Being a relatively new technology, social media is the subject of only a nascent literature. A variety of "real-life" events suggested the importance of punishment. Appendix Table 1.10 (on page 56 of the main matter) describes 48 selected cases of punishment and public shaming that substantially involved the Internet. These examples show social media being used to pressure prominent leaders, demand changes in business practices, and to publicly shame individuals for small transgression. Executives have been forced to resign, and companies have been shut down. These examples are not isolated incidents but rather pieces of a pattern to which businesses are now responding. For example, the online review site Yelp has adapted its moderating operations and scans for potentially viral news events in order to mitigate the problem of attacks against specific restaurants, which have frequently overwhelmed legitimate reviews (McKeever 2015). Yelp's business model depends on voluntary contributions to a public good, which forces the company to handle a double-edged sword: cooperative individuals are also highly punitive against those who violate norms (Falk et al. 2005).

These events are characterized by very intense, rapidly-forming, punitive responses by a large number of individuals. The responses often arise from relatively inconsequential or localized violations. It is difficult to reconcile this behavior with theories based entirely on stable, self-interested preferences. However, the speed, intensity, and sociality of the responses strongly suggest that emotion is critical.

The literature on punishment, even in economics, often attributes the behavior to anger or a similar emotion, for example, indignation. These findings appear in self-reported data (Fehr and Gächter 2002) and in experimental manipulations (Grimm and Mengel 2011). However, without some theory of what makes people angry, it is difficult to make predictions based on the relationship between anger and punishment. The first chapter instead makes use of research showing that communication and social media are also linked to anger. Anger spreads especially well through social media (Fan et al. 2014; Berger and Milkman 2012; Berger 2011). Moreover, individuals who see an antisocial behavior tend to experience negative emotions and can relieve those emotions by sharing information about the antisocial behavior with a third-party (Feinberg et al. 2012b).

Putting these findings together suggests that introducing social media will have some facilitating effect on punishment. The experiment found support for this prediction by exposing individuals to new information about organizations (information shocks) either with or without social media. The apparent asymmetry between punishing and rewarding behavior seems especially unlikely to have been predicted without reflecting on the research about emotional motivations. Like the other two chapters, a substantive economic relationship between two variables was found without having to measure emotions. Nevertheless, better measurements of emotion-related variables would add greatly to any similar studies conducted in the future.

Discussion of what was learned

The previous section lists several reasons why studying emotion-mediated effects is promising. However, despite those reasons and the fact that economy-related stress may be the top stressor among Americans, the effects reported in chapters 2 and 3 are fairly modest. The announcement of a very large job loss event is associated with a decrease in the mean birth weight of less than 1 percent. These effects show little sign of extending beyond the county of the employer. In one sense the effects are large and in another small. The effects from announcements of job losses appear large when we note that they are similar in size to effects estimated in studies of natural and man-made disasters (see Figure 2.3). This finding adds another piece of evidence that the costs of job loss and economic change are greater than once thought.

In contrast, all of these seemingly extreme shocks appear to have smaller effects than several more mundane factors, for example, maternal smoking and black race (also in Figure 2.3). However, it should not be concluded that the effects of shocks are unimportant. First, the largest effects on birth weight (all losses > 100 grams) are associated with exposures measured at the individual level, for example, the mother smoking, the mother being black, or birth after the father has lost employment. The average effects in the area-wide shock studies probably mask large degrees of heterogeneity.

Second, the effects on birth outcomes are just one piece of the (population-wide) stress response. The fact that many papers in economics look at birth outcomes reflects the easy availability of high quality birth data and economists' long-standing interest in intergenerational mobility and human capital.[4] A more complete accounting of the costs of any shock or stressor requires considering more outcomes. Furthermore, these other outcomes or costs of a shocks may not be highly correlated with the effects on birth outcomes. Figure 2.3 shows effects from a wide variety of shocking events yet the birth weight losses are surprisingly similar. A very recent working paper reports a few grams of lost birth weight in association Super Bowl wins—likely due to increased tobacco and alcohol consumption (Duncan et al. 2015). Thus, one might conclude from the literature that almost any emotionally charged event with widespread attention will arrive along with a small (a few grams or low double-digit grams) decrease in average birth weight within the affected area regardless of the wider material and social consequences of the shock. This apparent invariance is itself a puzzle to be explained.

An additional problem with studying *average* effects on birth outcomes is that these effects are small relative to differences between different locations—even within the same country—and unexplained time-trends (Donahue et al. 2010). Separating interesting effects from these large and murky sources of variation can be statistically difficult. Finally, estimating these average effects is of limited practical use. It is impractical to advise people to avoid disasters. Health care workers might be usefully warned of an increase in the risk of poor birth outcomes in the aftermath of a shock. But, it would be far more useful to provide information on which individuals are at the greatest risk of a bad outcome and how they might avoid one.

These issues could be addressed with finer data on individuals' responses. The traditional way of collecting such data is a survey. However, when the event of interest is a surprise, a prospective survey is almost impossible. A retrospective survey will be subject to recall errors. Either case will suffer from the typical problems with self-reported data, which may be exacerbated by stressful situations. Surveys are also expensive, and large sample sizes may be needed to find the individuals that have strong responses to the stressor. One promising approach to understand these responses—and the inputs to fetal development more broadly—is to collect relatively cheap, high resolution data through mobile devices. Methods are being developed to allow inference of emotional states from smartphone usage, for example, typing rates and shaking movements

[4]The enormous number of papers written based on birth data from the U.S. and elsewhere demonstrates the value of systematically collecting data that covers every instance of some important phenomenon (in this case births). The coverage of the data allows it to be merged with any other data source without worrying about selection. This has allowed researchers document a great variety of different factors that influence birth outcomes.

(Graham-Rowe 2012). Instrumented mobile devices may also allow researchers to understand how pollution and other environmental exposures interact with stress and economic circumstances.[5] Birth data may eventually be linked with large amounts of high-resolution data on activity, location, noise levels, pollution, and other personal variables, but privacy concerns will certainly emerge.

One take-away from the last two chapters is that the effects of large-scale shocks on average measurements of health may be smaller than expected. We see something similar in the chapter on social media and punishment. One might predict that introducing social interaction would lead to an enormous increase in punishing (or rewarding) behavior. However, the social condition only had about 21 percent more instances of individuals inflicting punishment and 12 percent more instances of reward, and neither difference is statistically significant. However, on average, the *net* losses generated by punishment almost doubled. This increase depended on the emergence of unpopular organizations that, due to social information, received concentrated punishment and relatively few rewards. The social condition showed some evidence of an increase in the net effects of rewards. Together these effects generated large increases in inequality across the organizations.

The substantial effects on the distribution of points but modest effect on the overall propensity to punish or reward gives away the fact that attention is the main mechanism. The social effects on rewarding behavior appear to result almost entirely from the attention-manipulating algorithm used to dynamically construct the web page. Punishing behavior has some social influence that goes beyond the algorithm, but these effects appear to relate largely to helping potential punishers seek out unpopular organizations. The social condition also had no significant effect on the average degree of self-reported anger. It is—at least in this setting—much easier to manipulate people's attention and where they direct their efforts than to persuade them to do more or less of some action. Attention-related choices, to the extent that they can be called choices, are made under weak incentives. The value of any potential object of attention cannot even be determined until some attention has been directed to it. Without any information to distinguish the various objects on a screen, attention will be determined by very weak influences, for example, small differences in effort costs related to manipulating the window. Since potentially important choices follow from where attention is directed, the ultimate consequences of small influences on attention may

[5] Small, inexpensive mobile devices for measuring pollution exposure are in development (Handwerk 2015), which could help understand the effects of pre-natal exposure to air pollution (Currie et al. 2009) and the interaction of maternal pollution exposure and poverty (Vishnevetsky et al. 2015).

be disproportionate.

The role of attention also suggests an interpretation of two common tropes about the Internet and society that seem incompatible: One is that social media has caused people to spend an increasing amount of time looking at viral trivia like baby videos and humorous listicles. The other is that social media encourages unending outrage and abuse (Thompson 2014). The results of this experiment suggest instead that social media may function mostly to concentrate attention and thereby increase the intensity and salience of any reaction, be it favorable or unfavorable. A more subtle conclusion is that Internet use involves numerous choices made rapidly with little reflection and little directly at stake, which makes factors like search costs—even miniscule ones—and visual presentation ("display effects") relatively important and gives substantial influence to those who design online interfaces and algorithms. The distribution of behavior will be shaped by the incentives of the designers, the incentives and psychological characteristics of the users, and how these variables interact to determine the design of online mechanisms. But, the importance of unpredictable social events and interactions means the realizations of behavior may still be highly variable and difficult for a designer to control.[6]

An important methodological lesson from the social media experiment is that it is possible to create an experimental model of a complex media environment that is surprisingly realistic in several ways. A couple of technical features stand out. First, the click rates of items on the page show a pattern that is very similar to real Facebook data reported by Bakshy et al. (2015). Second, the social treatment increased the total amount of time spent on the news page and increased the probability that a participant would visit more than one news story. The business case for social media is that it should increase the engagement of users. A failure to find this effect in the experiment would raise doubts about whether the participants experienced experimental social media in the same way they do real social media. Third, the participants' behavior is broadly consistent with related research. More educated participants viewed more news stories, which is a pattern that also exists in the general population. College-educated participants were also more likely to punish, a result also reported by Carpenter and Seki (2011) and Carpenter et al. (2004) (based on Japanese fishermen and Thai and Vietnamese slum dwellers, respectively). Overall, about 50

[6]For example, in the setting of the experiment here, a designer wishing to increase average page views would implement the social media condition rather than the independent condition. This choice would, as a "side effect," increase the mean and the variance of the distribution of net punishment.

percent of participants in the present study punished at all during the experiment, which compares well to the results from Fehr and Fischbacher (2004a), where about 45–60 percent of a sample of Zurich college students punished.[7] As discussed above, anger is thought to be an important cause of punishment, and the participants in this experiment who reported experiencing more anger were also more likely to have punished during the experiment.[8] Finally, the participants who reported engaging in some kind of protest activity before were more likely to inflict punishment in the experiment.

This success is important because algorithms that infer preferences and guide choices are ubiquitous and a topic of growing research (Kramer et al. 2014, for example,). However, few researchers have sufficient access to social media data to conduct research, and the algorithms themselves are often secret (Lazer 2015). Chapter 1 demonstrates that simple tools and participants recruited inexpensively can be used to create a useful model of these situations. The behavior of the model agrees with important aspects of behavior in social media, in lab experiments, and among the general population.

[7] However, see Guala (2012) for extensive discussion about the rate of punishers.

[8] Anthropological evidence shows a positive relation between market integration and the propensity to punish (Henrich et al. 2010). An analogue to their market integration variable in industrialized societies is not obvious. However, the present experiment's results do show a suggestive (but not statistically significant) association between propensity to punish and owning a smartphone, which might be thought of as a tool allowing much easier access to many economic services.

Chapter 1

Punishing and rewarding in an experimental media environment

Abstract

The economic and political outcomes of organizations and leaders can be affected positively or negatively by media coverage. The existing economics literature on media production and bias treats media firms as producers and the public as consumers. However, social media technology incorporates consumer behavior directly into the production of news coverage. The interactions and preferences of numerous individuals can attract attention to specific events and frame the coverage in a way that is helpful or harmful to the subject of coverage. This study experimentally investigates individual choices to monetarily reward or punish non-profit organizations in response to media coverage and how those choices influence the aggregate outcomes of the organizations. In the control condition all participants are independent and receive news content from a static, centralized source. The manipulation introduces a stylized form of social media in which (1) the most attention-getting news content is made even more salient and (2) individuals' reward-or-punish behavior is made public. The introduction of social media is found to increase inequality across the organizations along with the net effect and variability of punishment. These effects are strongly driven by the attention-manipulating effects of the page construction algorithm which tend to concentrate positive and negative actions. However, social information about punishment generates additional effects by allowing participants to effectively target unpopular organizations.

1.1. Introduction

Leaders, companies, and other organizations can be adversely affected by negative media coverage. Exposure of corrupt politicians can damage their electoral prospects (Ferraz and Finan 2008). Hermitage Capital Management successfully pursues a "shaming" strategy to pressure corporations to correct governance violations (Dyck et al. 2008). The Catholic church's sexual abuse scandal caused substantial numbers of members to leave (Hungerman 2013). Media firms' behavior reflects an understanding of these effects. For example, media firms bias their coverage in favor of the advertisers on which they depend for revenue (Di Tella and Franceschelli 2011; Gurun and Butler 2012). Thus, to understand the prospects of influential leaders and organizations, we must understand the production of media coverage. In particular, we should know how media processes cause a particular organization to (1) receive more or less attention from the public and (2) be depicted in a more positive or negative way. Social media is a new technology with great capacity to draw attention to events and shape discussion about them. This study experimentally investigates how introducing a stylized form of social media into a news environment affects the organizations which are subject to coverage. The topic of technological development in the media market is first on the list of promising research agendas in Prat and Strömberg's (2011) survey of the political economy of media. Their survey notes specifically the lack of research on news media and the Internet.

Research on media and political economy predominantly takes the approach of treating media outlets as producers and members of the public as consumers. Often, in addition to choosing media consumption, each consumer has some other decision to make related to policy outcomes, e.g., voting. A common concern is bias, that is, selection by a media outlet of what issues are covered, what aspects of the issues are covered, how facts are framed, and how to comment on issues (Prat and Strömberg 2011). This framework gives the decision about bias to the firm, although it will be subject to equilibrium effects.

However, the standard framework is challenged by the Internet media market with social media. Decisions about bias are no longer solely the domain of the media outlet. Consumers and large portals, e.g., aggregators and social networks, are integral parts of the Internet media market, and they influence all aspects of bias listed above. In this context, we can identify two important features that are missing from the standard framework.

First, consumers' behavior directly enters into the production function of web news. That is, consumers contribute to media products by adding commentary to content on news websites or to links posted on social networks. These comments typically add a positive or negative perspective. Some sites provide features designed to allow users to display their opinion, for example, "Likes" on Facebook or voting on Reddit. In addition, many media sites publicly display aggregate measures of their users' behavior, e.g., by publishing "trending" topics or listing the most-viewed or most-shared stories. Audience data is also used to dynamically change particular pieces of content or their salience on the site. For example, a popular news story may be made more salient by increasing its size and changing its position on the site. Features which draw attention to certain items and make them easy to encounter are especially important because the amount of content produced is far greater than anyone can consume. When numerous options are available, the visual accessibility of the options is known to influence attention and choice (display effects) (Reutskaja et al. 2011).

Second, media consumers often access media content suggested by third parties, which can influence the relative attention given to different events or perspectives. News sites receive about 20 percent of their traffic from links on Facebook, which are posted by users or displayed by a "personalized" recommender system (Somaiya 2014). This dynamic reduces the news site's ability to influence the audience's attention. Instead, the audience's attention will be influenced by social sharing of links and complex, opaque algorithms. The sharing behavior depends on millions of interacting individuals, making it potentially much more complicated than bias produced by a single firm.

These facts mean that to understand the political and economic roles of the media, we must understand how social media technology and individual behavior interact. The relevant consumer behaviors are often made subject to unusual types of motivations that are difficult to model or predict. That is, the choice to share or consume a piece of news content is subject to emotions, such as surprise, anger, or boredom and also concerns for social image, political preferences, and social norms (Berger and Milkman 2012; Berger 2011; Barasch and Berger 2014). In particular, negative news coverage of leaders and organizations often generates consumer responses that resemble third-party punishment as studied in the social preferences literature, which further involves emotional and social motivations (Carpenter and Matthews 2012; Fehr and Fischbacher 2004a; Coffman 2011).

This study aims to shed light on the role of social media technology using model societies that are simple enough to remain tractable yet still incorporate several important features of the actual media market. First, each model society is composed of numerous individuals with access to media content. Second, each society includes prominent organizations which are subject to coverage in the media. Third, in addition to consuming media content, each consumer can take actions that have positive or negative effects on the organizations. The experimental manipulation introduces a stylized form of social media with two features: (1) the behavior of users determines the relative salience of the pieces of news content, and (2) the users' positive and negative actions towards each organization are made public. In addition, the study runs multiple instances of each society with identical initial conditions but a different random sample of participants. This method, dubbed "multiple worlds" by Salganik et al. (2006), allows examination of the distribution of aggregate outcomes. By experimenting on these model societies, the study addresses several questions. First, how does the technology used to provide media content influence the outcomes of the organizations? Second, how does the technology influence the behavior of the individual consumers? Third, how does individual behavior interact with technology? In particular, the study helps us understand differences between positive and negative behaviors towards the organizations.

The results reveal that the addition of social information into the media environment has two main effects. First, the organizations covered by the news content become more unequal in their outcomes. This effect occurs mainly because the social condition concentrates positive and negative actions. Organizations that have been affected by positive or negative behavior are more likely to be the subject of further actions. At the individual level this herding-like behavior is mostly driven by the page-construction algorithm, which sorts the most viewed stories to the top of the page where they receive more attention. The social effects of punishment and reward are almost entirely eliminated once the effect of the sorting algorithm is accounted for. Second, under the social condition the net effects of punishment in a given world tend to be greater on average but also more variable. At the individual level two behaviors contribute to the increase in net punishment. Social information allows individuals to seek out and target relatively unpopular organizations even when they are rare in comparison to popular organizations. In addition, negative social information has some strong effects that go beyond the sorting algorithm. Participants that view any given story are significantly more likely to punish and less like to reward if that story was punished by previous participants. A similar social effect of reward is not found, which suggests that negative actions can have special social effects.

1.2. Background

1.2.1. Mechanisms of media influence

Media may influence behavior through several mechanisms. The most obvious is a simple information and learning effect. Media coverage provides new information to an individual who then updates their beliefs and behaves differently. Another similar mechanism, which can be difficult to distinguish, is persuasion. Persuasion can be modeled with rational, Bayesian agents (Kamenica and Gentzkow 2011), which is not distinct from an information and learning mechanism. Persuasion as a distinct mechanism is sometimes considered to be learning that is erroneous in some way, for example, by failing to correctly account for the incentives or bias of a persuader (Cain et al. 2005; DellaVigna and Kaplan 2007). Persuasion can also be conceptualized as a change in preferences (see, for example, Yanagizawa-Drott 2014). Another mechanism is limited attention. Individuals may have a limited capacity to be influenced by new information, and the media may bias attention towards certain pieces of information and away from others.

Rational learning and persuasion are two candidate mechanisms for media influence, particularly in the political economy literature. However, definitively distinguishing the two mechanisms is difficult. DellaVigna and Kaplan (2007) exploit geographic variation in the rollout of Fox News to show that access before the year 2000 resulted in greater vote share for Republican candidates. They attribute this effect chiefly to ideological persuasion. In a small field experiment Gerber et al. (2009) document an ideologically leftward shift when the voter is offered a free newspaper subscription. Their evidence is more consistent with a learning effect than persuasion. Pure information or learning mechanisms also appear in research on financial markets and the timing of local media coverage (Engelberg and Parsons 2011). Research in finance has also looked at the "sentiment" of media content—the positive or negative orientation of the text—which may operate in a similar way to learning and persuasion (see, for example, Tetlock 2007). Finally, Strömberg (2004) show that U.S. counties with greater radio broadcast audiences received more federal funding under the New Deal. This effect may reflect self-interested policy makers perceiving radio listeners as being more informed.

The attentional mechanism is also supported by numerous studies. In an event-based design, Eisensee and Strömberg (2007) study the effects of competition between newsworthy events for public attention. Using two empirical designs, they show that disasters receive less aid from the United States when they face greater competition for television airtime. Gentzkow (2006) argues that the introduction of television, "crowded out" consumption of local radio and newspapers, which in turn decreased turnout in local elections. The nationwide marketing of the *New York Times* may have had similar effects (George and Waldfogel 2006). Olkean (2009) reports that improved television access decreased social capital and community activities in Javan villages. These effects may be viewed as manifestations of limited attention. Research in finance documents similar effects. Even highly incentivized and experienced investors have limited attention. Market behavior is sensitive to the timing of important announcements made at publicly known times (Dellavigna and Pollet 2009; Hirshleifer et al. 2009). Shifting the attention of investors appears to be one of the important roles of media coverage in financial markets (Barber and Odean 2008). Huberman and Regev (2001) document a stark case in which a news article drew attention to previously published cancer research and thereby generated enormous effects in biotech trading. Unlike the present experiment, these studies do not consider whether positive and negative news might have different effects on attention.

1.2.2. Negative publicity

Research indicates that negative media coverage can generate substantial effects across a variety of political and economic domains, including elected and autocratic leaders, corporations, and non-profit organizations. Negative publicity is often interpreted in terms of ethics or morality. However, media coverage may also simply convey negative information about the quality of a product. The literature offers limited evidence about whether negative publicity functions by attracting attention, persuading individuals to take action, or some other mechanism.

Ferraz and Finan (2008) provide evidence on the effects of publicizing corruption by exploiting a government policy that published randomized audits of elected officials. Officials revealed as corrupt are significantly and substantially less likely to be re-elected. These effects are amplified by the presence of local radio broadcasters. The authors attribute this effect to the voters' new information about the officials, which leads them to punish corrupt officials by voting against them. The study cannot say whether the voters were motivated by an interest in the quality of government (potentially purely self-interested) or retribution (social preferences). Several studies examine the effects of "name and shame" campaigns against human rights abuses by governments. These campaigns are reported to (1) decrease violence and abuse (Krain 2012; Murdie and Davis 2012), (2) decrease foreign direct investment in the target countries (Barry et al. 2013), (3) increase the probability of humanitarian intervention (Murdie and Peksen 2014), and (4) increase the probability of economic sanctions (Murdie and Peksen 2013; Peksen et al. 2014). It should, however, be noted that these studies do not have exogenous variation on shaming campaigns. Human rights organizations and media are unlikely to launch randomized shaming campaigns but rather behave in a strategic manner (Wright and Escribà-Folch 2009; DeMeritt 2012). Media can also serve to increase levels of violence. Yanagizawa-Drott's (2014) study of the Rwandan Genocide reports that villages with better radio reception engaged in greater levels of militia violence, which is attributed to (1) a propaganda-fueled boost in preference for ethnic violence or (2) information about the (low) likelihood of punishment by authorities. Dellavigna et al. (2014) also report that radio broadcasts can trigger ethnic hatred.

Dyck et al. (2008) analyze Hermitage Capital Management's "shaming" attacks against Russian corporations. They report that Hermitage's actions drew media attention to corporate governance violations, which then prompted action by the corporation or the intervention of a regulator. The mechanism depended on international media coverage, as opposed to domestic, which suggests that the relevant leaders aim to protect their reputations with international lenders or business partners. These results are robust to using Hermitage's portfolio as an instrument for shaming attacks. Another study from the finance literature uses content analysis of *Wall Street Journal* analysis to show a link between pessimistic sentiment and decreases in the Dow Jones index (Tetlock 2007).

Hungerman (2013) shows that scandals can also have substantial effects on non-profit organizations. They report that membership in the Catholic church stopped growing following widespread allegations of child sexual abuse. Surprisingly, the members shifted predominantly to Baptist churches. The scandal may have also decreased the number of Catholic schools and enrollment in them (Dills and Hernández-Julián 2012).

Research on consumer goods provides evidence that negative publicity can decrease demand. Although product reviews generally do not have the ethical or moral component involved in scandals, these results still show that consumers are responsive to negative information (Brown et al. 2012; Chevalier and Mayzlin 2006; Basuroy et al. 2003; Reinstein and Snyder 2005). However, Berger et al. (2010) argue that negative publicity can sometimes benefit a company if the effect of added publicity outweighs the negativity.

1.2.3. Motivations for sharing and punishment

The topic negative publicity is especially important when considering the possibility that information transmitted over social media could spur individuals to action. Both social information transmission and punishment behavior are motivated by negative emotions. Media content with emotional charge, especially anger, is more likely to be shared (Berger and Milkman 2012), and anger is especially contagious between social media users (Fan et al. 2014). Generally, emotion and physical arousal increase sharing of media content (Stieglitz and Dang-Xuan 2013; Berger 2011).

Similarly, research from several angles shows close links between punishment and negative emotion. In economics experiments, numerous researchers have linked punishment behavior to negative emotions, especially anger, spite, and indignation (Fehr and Fischbacher 2004b; Bosman et al. 2001; Bosman and Van Winden 2002; Carpenter and Matthews 2012; Frank 1988; Sanfey et al. 2003). The strongest evidence of a causal effect comes from adding “cooling off” periods to punishment games, which reduces the propensity to punish (Grimm and Mengel 2011; Neo et al. 2013; Wang et al. 2011). Individuals also make trade-offs between costly punishment and “costless” verbal punishment, indicating that the underlying motivation may be to express negative emotions about a norm violation or inflict psychological pain on the transgressor (Xiao and Houser

2005). Dictators (in the dictator game) are also willing to make larger offers in order to avoid being subjected to verbal punishment by the receiver (Xiao and Houser 2009).

Research in marketing links punitive consumer behaviors, for example, boycotts, to "consumer outrage," a reaction involving moral emotions and perceived violations of norms (Lindenmeier et al. 2012).

Finally, sharing negative information about a particular individual (or organization) can serve as a form of punishment against that individual. Sharing can be seen simply as increasing the audience and effectiveness of negative publicity. This interpretation fits well within the literature on norm enforcement, where gossip can be considered a form of low-cost punishment (Feinberg et al. 2012a). However, an individual may inflict punishment by spreading negative publicity even without the intention of actually punishing. Sharing may be motivated by a desire for attention or to provide useful information to friends. This situation is similar to a news company that publishes a damaging exposé or Hermitage Capital Management, both of which are ultimately motivated by profit. Thus, we might view negative publicity targeting a corrupt leader as a form of collective action. The collective action problem can be solved as a side effect of news outlets or individuals responding to private incentives (Olson 1965).

1.3. Experimental design

1.3.1. Motivation

The experiment is designed to capture individuals' positive and negative behavior toward non-profit organizations. The environment is tractable but rich enough to involve both attention and social influence, which are the two main mechanisms identified in the political economic literature on media. First, limited attention is relevant because the number of participants and stories is fairly large. Reutskaja et al. (2011) find substantial attention-related effects with 16 options whereas the present experiment has 24 stories. Second, persuasion is potentially operative because participants can send positive and negative signals. In addition, the stories were selected to be controversial and unusual, which leaves room for the participants to be swayed. However, it must be noted that the experiment is not designed to disentangle the mechanisms involved in social influence and attention. That goal would require a set of much narrower and more tightly controlled experiments. Instead the design is intended to test how the introduction of social media affects economic outcomes when attention and social influence are at work.

The wealth (in points) of the organizations is the main economic outcome. The presence of these organizations also constitutes an important contribution over studies that examine only the behavior of media consumers or social media users. The individuals are only allowed to influence points by punishing or rewarding (by 10 points) or doing nothing. This design is the simplest one that captures variation in the motivation to take action at all and the polarity of the action. The points correspond to real monetary payoffs so that the participants are incentivized under other-regarding preferences. The addition and subtraction of points is conceptualized as a highly stylized form of reward and punishment. Corresponding naturally-occurring behaviors include fund-raising campaigns, public praise, politically-motivated patronization of businesses, boycotts, vigilante activities, harassment, shaming, posting negative reviews, and protesting.

1.3.2. Overview

Participants in the experiment were recruited from Amazon Mechanical Turk. Each participant was informed that completion of the experiment consisted of: (1) viewing a news web page with headlines of 24 real stories about non-profit organizations, (2) visiting 1–9 of the 24 stories,[1] and (3) completing a brief questionnaire. Subjects could optionally make choices to affect the payoffs of the 24 distinct non-profit organizations discussed in the stories. However, each subject that completed the study, which takes about 3–10 minutes, received only a fixed payment of exactly $0.75.

1.3.3. Payoffs to non-profits

Each of the 24 non-profit organizations was endowed with 300 points. At the end of the experiment a single organization was selected uniformly at random. That organization's points were converted to money at the rate of 10 points = $0.30 and sent to the organization as a donation. The participants

[1] The most important part of this requirement is that the participant visit at least one story. The upper bound of nine was intended to prevent any one participant from having a large influence on a given world. Based on pilot testing, the upper bound was expected to bind for very few participants.

could change each organization's amount of points without incurring a cost (other than a very small amount of time and effort). Specifically, each news story contained a menu which allowed the reader to optionally add or subtract exactly 10 points from the organization described in the story. Since each participant was allowed to read up to nine stories, a participant could affect, at most, the expected payoff of nine organizations. The parameters were designed such that within any independent session (world) no organization could go below 0 points or above 600 points.

1.3.4. Design of the news website

The stories used in the website were taken from a variety of popular news websites. A preliminary sample of recent stories about non-profit organizations was collected. These stories were rated by a separate group on Mechanical Turk on various dimensions, for example, the extent to which the subject experienced anger and interestingness. These ratings were used to construct the final set of stories posted on the website. The stories were selected so that interestingness was moderate to high and within-story anger ratings had moderate to high variance. High within-story variance in anger ratings is interpreted as an indicator of controversy. Since choices from a visual menu are strongly influenced by the position of the items, the initial order of the stories was randomized.

Just as at a normal website, subjects could view a news story by clicking the headline. The full-text of each story would then be displayed.[2] Unlike a typical news site, each story was accompanied by a sidebar with the name of the non-profit organization and neutral description of the organization, which was usually adapted from wikipedia.org. Below the description the subject could click a button to add 10 points or a button to subtract 10 points from the named organization. Subjects could also cancel or reverse this decision.

The website's menu bar contained a drop-down display where the participant could check how many stories he had viewed and how many points he had added and subtracted. After viewing at least one story, a "Survey" button would become available. This button allowed the participant to go to the final step of the experiment, which was to complete a questionnaire.

The news website was created using Meteor (`https://www.meteor.com/`), which is a cutting-edge framework for developing web applications, and hosted on a DigitalOcean server.

[2] Some stories were truncated so that the typical length was about 500 words. Truncation was done so as to avoid changing interpretation of the story.

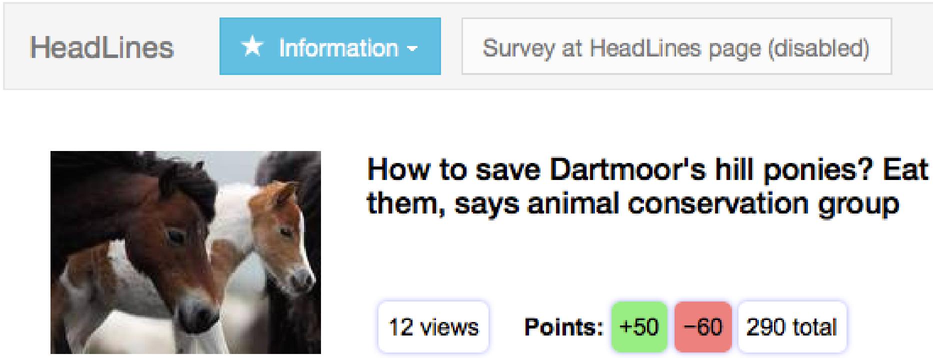


FIGURE 1.1: Screenshot of the top of the news website in the social condition.

This approach made it relatively simple to give the site a modern design and high performance. Users tend to become frustrated and leave websites that have poor design or functionality. Therefore, good design is important to minimize participant attrition and selection bias. A natural-looking site is also important for encouraging the participants to behave as they would at a real news site.

1.3.5. Multiple-worlds structure and experimental conditions

The experiment uses the multiple-worlds structure proposed by Salganik et al. (2006). This structure puts participants in groups or worlds, but each world is independent of all others. This design generates independent realizations of random group-level outcomes even when individuals are allowed to interact. Each world has the same initial conditions (other than the identities of the participants). In this case, the conditions are (1) the stories and non-profit organizations on the news site, (2) the order of the stories on the page, and (3) the endowments of the organizations. Each world is composed of 30 participants who participant sequentially. There is no temporal overlap among participants in a given world. If a participant did not complete the study, then their data would not be incorporated into the world. Effectively they would never have existed as far as other participants were concerned.[3]

The experiment contained a *social condition* and an *independent (control) condition*. In the social condition, a participant could see several aspects of the behavior of all previous participants (within his own world only). Each headline on the website displayed (1) the number of previous views, (2) the current total points of the organization in the story, (3) the number of points added,

[3]Nevertheless, the attrition rate was low due to ease, interestingness, and high earnings per unit time.

and (4) the number of points subtracted. Each subject was also informed about how many participants had gone before them. In addition, during the social condition the position of the headlines on the page was determined by the number of views. The headlines were arranged in *decreasing* order, so that the most-viewed story was at the top. Ties were broken according to the initial random ordering. *The organizations' points had no role in ordering the headlines*.

In the independent condition no participant received any information about the behavior of other participants. Each participant was presented with an identical version of the webpage. The study is based on 12 social worlds and 4 independent worlds.

1.3.6. Similar studies

The design of this experiment draws on that of Salganik et al. (2006). They created a website where individuals could download music without charge. Their experiment involved a *social condition* in which (1) each participant could see how many times each song had been downloaded and (2) the songs were sorted so that the most popular songs appeared at the top of the screen. In the *independent condition* the participants had no information about downloads by other users, and the order of the songs was randomized for each participant. Multiple instances, or "worlds," of the website under both conditions were run independently. The main result is that in the social condition the popularity of songs was more unequal and unpredictable than in the independent condition.

The present study provides an extension and addresses a confound in Salganik et al. (2006). The extension is to allow for both positive and negative behavior (rewarding and punishing) instead of just a positive behavior (downloading a song). In addition, Salganik et al. (2006) link popularity to the positive behavior, whereas the present study uses page views as a measure of popularity. My design is therefore symmetric with respect to the positive and negative behaviors. The confounding issue in Salganik et al. (2006) is that the visual ordering of the songs within a given world was far more stable under the social condition than under the independent condition. This occurs because the social condition sorted songs by popularity whereas the independent condition sorted songs randomly across subjects. In a social world, most subjects towards the later part of the run saw the songs in a very similar order. The stability itself can greatly increase inequality across songs because a few songs will tend to remain at the top of the page where they will receive more attention. The present design fixes the news story order across all subjects in the independent condition. In that way, the design is actually somewhat biased against finding greater inequality in social condition. Nevertheless, that result still appears.

My design also draws on common features in the experimental economics literature. In typical experiments on punishment the participants are given an endowment by the experimenter. Punishment corresponds to a participant destroying the endowment of another (see, for example, Fehr and Fischbacher 2004a), which is precisely how punishment occurs in this design. Finally, in my design some players are organizations which have no choice to make. This feature is borrowed from the "representative dictator game" of Carpenter et al. (2008), in which participants play as a dictator with a non-profit organization.

1.3.7. Instructions to the participants

Participants were informed that they would view the news site as part of a "group" of 30 individuals. That is, they were told that their points over choices would be summed with those of the 29 others in the group to determine the final total points for each organization. They were fully informed about the payoff structure for the non-profit organizations.

An important difference from many lab experiments is that the instruction page was identical for both experimental conditions. In a web experiment the risk of attrition is higher than in a lab, and participants might select out of the experiment due to the content or length of the instructions.[4] To mitigate this problem, all participants saw the same instructions page, which informed them of (1) the basic nature of the news page, (2) the payoff structure of organizations, (3) the fact that they were in a group of 30, and (4) the necessary and sufficient conditions to complete the study, that is, visit 1–9 stories and then complete the questionnaire. Additional information about the social condition was provided through tooltips attached to the objects that displayed the organizations points. The social conditional also included a small, collapsible banner explaining succinctly how the headlines were sorted.

Some participants on Mechanical Turk select out of tasks because they worry that their per-

[4] A total of 549 users entered the experiment. Of those, 506 reached the news headline page, and 480 subsequently finished. Therefore, the attrition rate upon reaching the headline page was 5.1 percent.

formance and pay may be low. This behavior could introduce selection on confidence or risk preferences. Therefore, the instructions also emphasized that the experiment does not have “right” or “wrong” answers and that each participant would be paid so long as they completed the basic requirements. Each subject had to also answer three questions about the instructions to demonstrate understanding of the payoffs and conditions of completion. Upon proceeding to the news headline page, a pop-up window reiterated that the subject had only to visit 1–9 stories and that it was entirely optional to add or subtract points.

1.3.8. Technical aspects of recruitment

Several aspects of the study, including recruitment, were designed so that subjects could not repeatedly participate in the study nor begin the study and then complete it at a later time. The purpose of this design is to make the study resemble a situation where many individuals participate a single time, each within one brief interval of time.

Several methods were used to control the recruitment process. Amazon’s built-in filter was used to allow only participants located in the United States. However, some subjects outside the United States are able to deceive Amazon’s system. Therefore, the server hosting the experiment also geocoded each participant’s IP address using two different geocoding services. Any visitor with a foreign IP address was blocked from participating. In addition, most Turkers from outside the U.S. are based in India. So the study was run only during the day in the U.S., when people in India were sleeping. Pilot studies and monitoring of the server indicated that these measures were very effective at preventing illegitimate participation. Inspection of the open-ended responses on the survey revealed little sign of anyone who was not a native speaker of American English.

Each potential participant at Mechanical Turk was given a one-time use link to the study’s website. They were advised that if they followed the link, they would not be able to make any other attempt on the study and would not be able to leave the site and return later. Any subject that followed the link would have their Amazon Worker ID and IP address logged. Any future participant with a matching ID or IP address would be prevented from participating.

1.4. Models and statistical procedures

1.4.1. Model of individual choices

The analysis of choices is conducted using pooled logit models. For expositional simplicity, consider that the dataset generated by a single world is structured as a participant-by-story panel with $N_w \times K$ data points, where $N_w = 30$ is the number of participants *in a single world* and $K = 24$ is the number of stories. Note that in the analysis the worlds will be pooled. Let $Y_{ik} \in \{0, 1\}$ be a binary outcome variable for the i-th participant in the world. For example, we may have that Y_{ik} indicates if participant i viewed story k. At the time that participant i visits the news headline page, each headline item k can be characterized by a vector $\mathbf{x}_{ik}$ potentially consisting of: (1) the position of the headline on the page, (2) the number of previous views of the corresponding story, (3) the number of points added, and (4) the number of points subtracted. Each participant i is then modelled as making 24 binary choices with the characteristics $\mathbf{x}_i = (\mathbf{x}_{ik})_{k=1,\ldots,24}$. We specify the model

$$\mathbf{P}(Y_{ik} = 1|\mathbf{x}_{ik}) = \Lambda(s_k + \mathbf{x}'_{ik}\boldsymbol{\beta}), \tag{1.1}$$

where s_k is a story-specific parameter (estimated by including dummy variables in the regression).

A typical component of $\mathbf{x}_{ik}$ will be the position $x_{i,k,1}$ of headline k on the news page when viewed by participant i. The case $x_{i,k,1} = 1$ means that story k was at the top of the page when viewed by i, while $x_{i,k,1} = 24$ means the story was at the bottom of the page. Note that the series $\{x_{i,k,1} : i = 1, \ldots, 30\}$ represents the position of a particular story k on the web page over time, which will potentially change according the the viewing choices of the 30 participants. The coefficient on $x_{i,k,1}$ represents the influence of the headline's visual position. We expect that headlines at the bottom of the page are less likely to be viewed, so the coefficient should be negative. In the social condition, the positions of the headlines can change from subject to subject depending on the number of views for each story. This feature allows the effect of position to be identified even when the story-specific parameters $\{s_k : k = 1, \ldots, 24\}$ are included in the model. However, in independent condition the stories do not vary in position. Therefore, the position effect cannot be estimated using only data from the independent condition. This limitation is operative in several of the models used but does not cause any serious problems.

The vector of story characteristics $\mathbf{x}_{ik}$ will also include some components that represent, in aggregate, the points-related choices of all the participants in the world prior to i. For example, $x_{i,k,2}$ might represent the sum of all rewards to story k. That is

$$x_{i,k,2} := \sum_{j=1}^{i-1} r_{jk}, \tag{1.2}$$

where $r_{jk} = 10$ if participant j rewarded the organization in story k (added 10 points).[5] A similar measure can be defined based on the variable p_{jk}, which equals 10 if and only if participant j punished the organization in story k (subtracted 10 points). The coefficients on these variables are designed to represent social influences. For example, we would like a positive coefficient on $x_{i,k,2}$ to reflect that previous decisions to reward k will (causally) influence a subsequent participant to also reward k. However, in order to have this interpretation, we must include the story-specific effects s_k in the model. If the statistical model omitted these effects, the estimates of the coefficient on $x_{i,k,2}$ (or any other points-related effect) might merely reflect the story-specific effect. This bias would be equivalent to claiming to identify an effect of social influence when in fact the effect is caused by a common environment. In this context, the "environment" is the news story itself.

One concern is that the model requires that each participant i makes independent choices $(Y_{i,1}, \ldots, Y_{i,24})$. However, in this application we might suspect that a participant may sample a variety of choices. In addition, the requirement that the participant read at least one story may induce dependence between the choices. To mitigate this concern, we estimate standard errors which are robust to within-subject clustering. The analysis will show that social influences can be "diluted," that is, the social effect of punishment/reward (on any particular story) decreases when other stories have also been punished or rewarded. To account for this effect many of the models will include terms that allow for a decay of social effects over the course of the experiment. That approach can be thought of as modeling the contextual effects on each subject's choices.

Another concern regards inference when participants interact in groups, which creates dependencies between the participants. The typical solution is to use standard errors clustered by the group. However, in this design we have only 4, 12, or 16 groups, which does not meet conven-

[5] In some models we will also use an indicator variable like $x_{i,k,2} := I\{\sum_{j=1}^{i-1} r_{jk} > 0\}$, which is simply a dummy variable that indicates if *any* previous participant rewarded story k.

tional levels for the quality of the asymptotic approximations. The alternative approach is again to model the dependencies by incorporating variables accounting for the interactions and then assuming independence conditional on the covariates. Appendix Tables 1.18 and 1.19 compare standard errors that are clustered by participant and by world. The models are the same ones displayed in Table 1.4 below. These results show that the standard errors clustered by world can be larger or smaller than those clustered by participant. In general, standard errors clustered by world are somewhat larger but similar in magnitude. The second table (1.19) includes page position controls, which should further control for subject inter-dependencies. That modification makes the two sets of standard errors even closer, supporting the argument that modeling the dependencies can improve inference. The analysis will use clustering by participant because the two approaches give similar results and the number of worlds is small.

1.4.2. Statistical procedures for the world-level analysis

1.4.2.1. Motivation of the metrics

We aim to define a few simple metrics that characterize the overall state of the organizations in a given world. These metrics can be calculated after each participant ("time period") of a world or at the end of the world as a final summary outcome. This passage provides some motivation and intuition for the metrics used in the study. The next sub-section provides mathematical definitions for the metrics.

An important characteristic of a world is the inequality of the organizations' points. Salganik et al. (2006) report a large, positive effect of social information on the inequality of downloads across songs within a given instance of a market. Their design is similar to this study. The songs correspond to organizations, and the downloads correspond to points. This suggests that the social condition should increase inequality. Figure 1.2 shows the distribution of points using a realistic but stylized data set. The data can be taken to represent (1) the final points after all 30 participants in the world or (2) an intermediate "snapshot" of the data before the world has been completed. The case of zero inequality corresponds to a perfectly flat, black line, which indicates that all organizations have the same number of points. As the black line diverges from zero, the inequality increases. *The measure of inequality used is the Theil index as described below.*

However, Salganik et al.'s (2006) study included only one type of behavior, downloading a song. The present study has two types of behavior, punishing and rewarding, which can each contribute to inequality. Therefore, we would like to have measures of the overall effects of punishing and rewarding which relate to inequality. *The metrics net punishment and net reward quantify the contributions of punishment and reward to changes in wealth levels and, therefore, to inequality.*[6]

Another interpretation of the net metrics is that they reflect clustering of behavior with respect to stories. For example, suppose that reward choices are uniformly distributed over the 24 stories. If many participants "coordinated" to punish a particular story k, then the net punishment against k would probably be large because the total points subtracted from k would be larger than the points added to k. However, if the participants distributed the same number of punishment choices over many different stories, then it is likely that in many cases the subtractions will be "canceled out" by the additions. Then the net punishment could be zero.

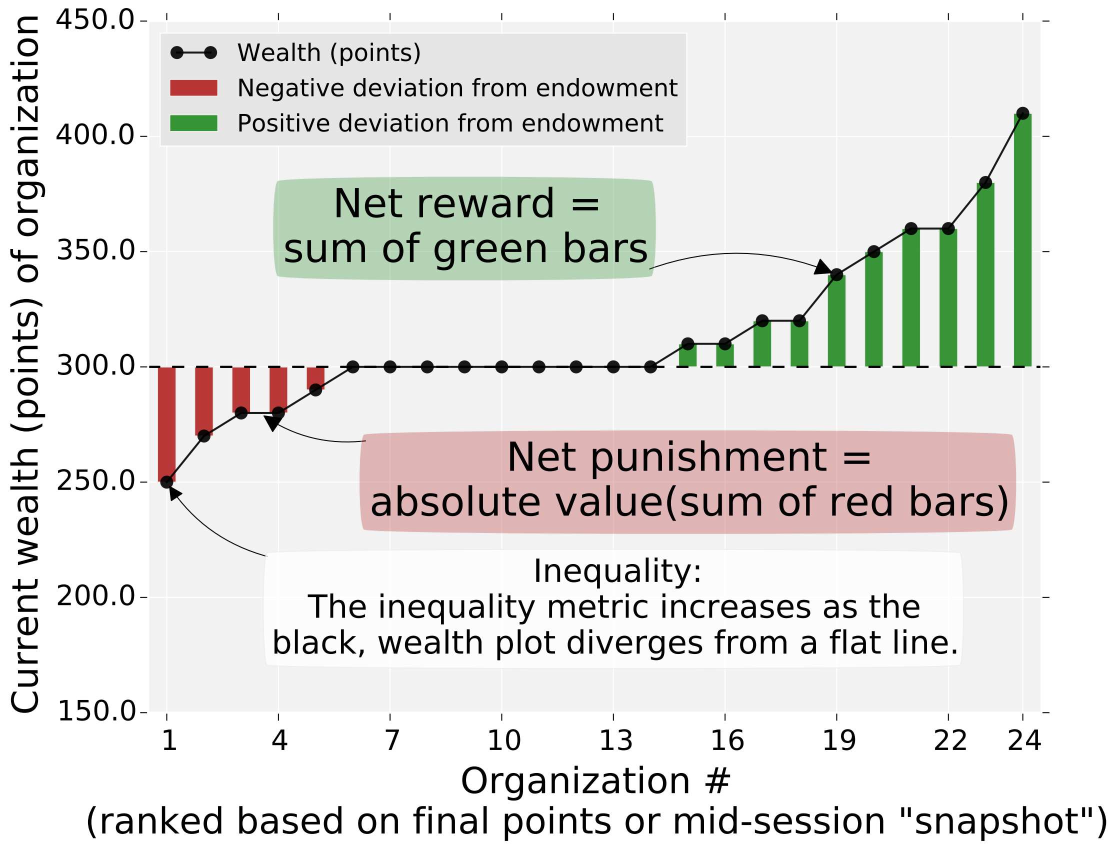


FIGURE 1.2: Illustration of the three world-level metrics (stylized data)

[6] The total amounts of punishment and reward do not reflect how punishment and reward can nullify each other's effects. Therefore, the total amounts have an ambiguous relation to inequality.

1.4.2.2. Definitions of metrics

Let $w = 1, \ldots, W$ index worlds in the experiment and $i_w = 1, \ldots, 30$ index the players in world w. Let the reward and punishment variables be $r_{i_w,k}$ and $p_{i_w,k}$, respectively. They represent the amount of points added or subtracted by a subject. The variable $r_{i_w,k}$ takes the value 10 (ten) if participant i_w rewarded story k and 0 otherwise. The variable $p_{i_w,k}$ is defined analogously.

Definition 1 *The wealth (points) inequality of the organizations in a given world is measured by the* Theil index, *which is given by*

$$Th(\mathbf{v}) := \frac{1}{24} \sum_{k=1}^{24} \left(\frac{v_k}{\bar{v}} \log \frac{v_k}{\bar{v}} \right), \tag{1.3}$$

where $\mathbf{v} = (v_k)_{k=1,\ldots,24}$ *is the vector of points for each of the 24 organizations. The mean of points is* $\bar{v} = \frac{1}{24} \sum_{k=1}^{24} v_k$. *A larger Theil index indicates that the distribution of points among the organizations is more unequal.*

Definition 2 *The* net reward of story k *is given by*

$$R_k := \left(\sum_{i=1}^{30} r_{i_w,k} - p_{i_w,k} \right)^+, \tag{1.4}$$

where the notation $(x)^+$ *means the positive part of* x. *The (interim) net reward after* n *participants can be calculated in the same way by summing over* $i = 1, \ldots, n$.

Definition 3 *The* net punishment of story k *is given by*

$$P_k := \left(\sum_{i=1}^{30} p_{i_w,k} - r_{i_w,k} \right)^+. \tag{1.5}$$

Note that for any given story k, *at most one of* R_k *and* P_k *can be non-zero. The (interim) net punishment after* n *participants can be calculated in the same way by summing over* $i = 1, \ldots, n$.

Definition 4 *The* net reward in world w *is given by*

$$\bar{R}_w := \sum_{k=1}^{24} R_k. \tag{1.6}$$

Notice that the net reward *in a particular world is the net increase in wealth (points) among the set of stories that were rewarded more than they were punished (story-wise).*

Definition 5 *The* net punishment in world w *is given by*

$$\bar{P}_w := \sum_{k=1}^{24} P_k. \tag{1.7}$$

The net reward is the net loss of wealth (points) among the set of stories that were punished more than they were rewarded (story-wise).

1.4.2.3. Estimators of world-level metrics

The distribution of aggregate outcomes of independent-condition worlds can be estimated by re-sampling individual participants. Let there be data for W independent worlds. Then there is a data set D composed of $30 \times W$ independent participants. For any world-level metric M let F^*_M be the distribution of M generated by drawing a subsample of size 30 from D without replacement.

The re-sampling estimator of the expected value of metric M is computed from B sub-sampled worlds $\{M^*_b : b = 1, \ldots, B\}$ where $M^*_b \overset{\text{iid}}{\sim} F^*_M$. The estimator is $\mu^*_M = \frac{1}{B}\sum_{b=1}^{B} M^*_b$. The estimator of the variance is $\sigma^2_{M^*} = \frac{1}{B}\sum_{b=1}^{B}(M^*_b - \mu^*_M)^2$. The simulation results in the appendix show that, compared to the conventional estimators, the re-sampling estimator of the mean is somewhat more efficient and the re-sampling estimator of the variance much more so. The conventional estimator of the standard error (of the mean) does not apply to the re-sampling estimator. Therefore, the standard error is estimated by the bootstrap which requires two levels of iteration. That is, J bootstrap data sets are created by drawing with replacement from the independent-condition participants. For each bootstrap data set $j = 1, \ldots, J$, we compute the re-sampling estimate of the mean $\mu^*_{M,j}$. The estimator of the standard error is then $\widehat{\text{se}(\mu^*_M)} := \sqrt{\frac{1}{J}\sum_{j=1}^{J}(\mu^*_{M,j} - \mu^*_M)^2}$.

Since the individuals participating in the social-condition worlds can influence each other, it is not appropriate to re-sample social-worlds. Therefore, the means and standard deviations of outcomes in the social worlds are estimated using the conventional estimators: the sample mean and sample standard deviation treating each world as an independent observation (rather than each individual).

To test the null hypothesis that the means of a world-level metric M are equal in the two conditions, $\mathrm{H}_0 : \mu_{M,\text{soc}} = \mu_{M,\text{ind}}$, I use a simple percentile bootstrap procedure.[7] The test statistic is the difference of the two means

$$d_{\text{obs}} = \hat{\mu}_{M,\text{soc}} - \mu^*_M. \tag{1.8}$$

The null distribution of the statistic is estimated by bootstrapping each of the means. For bootstrap replicates $j = 1, \ldots, J$, the social-world mean $\hat{\mu}^*_{M,\text{soc},j}$ is generated by drawing with replacement from the 12 social worlds and then computing the mean of M in the replicate sample j. The bootstrap distribution of μ^*_M is generated as described above, creating replicate samples of 120 individuals. Each replicate of 120 individuals is then used to compute a realization of the sub-sampling estimator $\mu^{**}_{M,j}$. The j-th bootstrap realization of the test statistic is

$$d^*_j := (\hat{\mu}^*_{M,\text{soc},j} - \hat{\mu}_{M,\text{soc}}) - (\mu^{**}_{M,j} - \mu^*_M). \tag{1.9}$$

The two-tailed p-value of the test is then $p_M := \frac{1}{J+1}\left(1 + \sum_{j=1}^{J}\{|d^*_j| \geq d_{\text{obs}}\}\right)$.

An alternative approach to hypothesis testing follows that of Salganik et al. (2006), in which the value of a statistic observed in the social worlds is compared to the distribution generated by re-sampling individuals from the independent worlds (effectively creating synthetic independent worlds). Let $\mathbf{M}^s = \{M_w : w = 1, \ldots, s\}$ represent outcomes of some metric M from s social worlds. Let $\mathbf{M}^{s*}$ be a vector of the same metric M in s worlds generated by re-sampling from F^*_M. Let $T_s(\mathbf{M}^s)$ be some statistic, for example, the sample mean or sample standard deviation of the metric M. For the null hypothesis $T_s(\mathbf{M}^s) \overset{\text{iid}}{\sim} T_s(\mathbf{M}^{s*})$, we can conduct a test by comparing the observed value from the social worlds $T_s(\mathbf{m})$ to the distribution of $T_s(\mathbf{M}^*)$ re-sampled from the independent data. Let $\{t^*_b : b = 1, \ldots, B\}$ be a set of B re-sampled realizations of $T_s(\mathbf{M}^{s*})$. Then we can estimate the probability p that the re-sampling distribution would generate a value at least as large as the one observed in the social worlds. That estimator is

$$\hat{p} = \tfrac{1}{B+1}\left(1 + \sum_{b=1}^{B}\{T_s(\mathbf{m}^s) \geq t^*_s\}\right) \tag{1.10}$$

[7] The advantage of this approach over a bootstrap t-test is that the standard error does not need to be computed for each bootstrap replicate, which would require an additional level of computationally-intensive iteration.

wher $\mathbf{m}^s$ is the observed vector of the metric M observed in the s social worlds. This approach to testing ignores sampling variation in F_M^* itself but is still presented for comparison.

1.5. Results

1.5.1. Analysis at the level of individual participants

1.5.1.1. Characteristics of the sample

Table 1.1 shows summary statistics for the two groups of participants. Some variables are demographic characteristics reported in the questionnaire, and others are based on recorded behavior during the experiment. In general, the participants are young, highly educated, and heavily female. The mean age is about 35.2 with a standard deviation of 11.3. Over half of the participants received a college degree (56.2 percent). The percentage female is 58.9 percent.

The participants are similar to a representative sample of U.S. households in terms of their ability to withstand a negative income shock. The variable designated "low wealth" indicates if the participant answered negatively to the following question: "If you were to lose your main source of income (e.g., job, government benefits), could you cover your expenses for 3 months by borrowing money, using savings, selling assets, or borrowing from friends/family?" The question was taken from a household survey by the Federal Reserve where 42.2 percent of respondents answered that they could *not* cover the expenses (Reserve 2014). In the present experiment the subjects reported being in slightly better condition, with 37 percent and 38 percent of the individual and social groups being unable to cover expenses.

The variable designated "has smartphone" indicates if the participant answered affirmatively to the following question: "Do you have a smartphone like an iPhone, Samsung Galaxy phone, or Windows Phone?" The independent and social groups reported having a smartphone at the rates of 78.5 and 81.6 percent, respectively. The question was adopted from the Pew Internet & American Life Project Poll, where 65 percent reported having a smartphone (Center 2014).

The variable "engaged in political protest" indicates if the participant answered affirmatively to the following question: "Have you ever done any of the following forms of political action? Joining in boycotts, attending peaceful demonstrations, joining strikes."

The variable designated "degree of belief in donation" was asked to check whether the subjects believe that the donations will be paid to the non-profit organizations. The question text was: "How much confidence do you have that we will pay out the points as explained?" The response was a scale with the points (1="No confident at all"), (2="Not too much confidence"), (3="Some confidence"), and (4="A lot of confidence"). The empirical probabilities are (0.0252, 0.0943, 0.4151, 0.4654). Therefore, the modal participant has "a lot of confidence" in the third-parties' payoffs.

The summary statistics indicate that many participants found the study interesting. Most participants (71 percent) visited more than one news story and therefore spent more time than required on study (in exchange for no additional earnings). The mean (median) number of stories visited was 3.25 (2). The maximum number of stories (9) was viewed by 8.75 percent of participants. The mean (median) amount of time browsing the news site was 5.27 (4.21) minutes. The fact that participants would spend more time than necessary on the study is not surprising because the study is designed to replicate a behavior that many people do in their leisure time.

1.5.1.2. Predictors of individual-level behavior

Table 1.2 shows several regressions that quantify how subjects' characteristics predict their behavior in the experiment. The binary outcomes are modeled using linear probability models, while the number of stories viewed is modelled using Poisson regression. A broad conclusion is that behavior in the experiment is predicted well by demographic characteristics, while technology adoption has somewhat less predictive power.

The variable "ever punished" indicates if the participant subtracted points from any of the organizations. Punishment is predicted by three demographic variables but none of the technological variables. In particular, engaging in political or economic protest activity is estimated to increase the probability of punishment by 0.122. Being female and being college-educated increased the probability of punishment by 0.117 and 0.093, respectively. This suggests that the propensity to punish in the experiment reflects general characteristics of the individuals rather than a technology- or domain-specific characteristic.

The variable "ever rewarded" is not strongly linked to any of the observed demographic variables. The only significant predictor is where the participant uses the social network Twitter.

TABLE 1.1: Summary statistics of the individual participants

	Independent condition			Social condition		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Age (years)	35.475	11.641	33	35.192	11.185	33
Male	0.370	0.485	0	0.425	0.495	0
College degree	0.622	0.487	1	0.542	0.499	1
Low wealth	0.370	0.485	0	0.380	0.486	0
Has smartphone	0.782	0.415	1	0.816	0.388	1
Uses Facebook	0.808	0.395	1	0.833	0.373	1
Uses Twitter	0.425	0.496	0	0.378	0.486	0
Has engaged in protest activity	0.227	0.421	0	0.226	0.419	0
Hours/week on MTurk	10.933	10.357	8	12.081	12.263	10
Dollars/week on MTurk	38.437	47.058	25	35.277	43.396	20
Behavior in experiment:						
- Stories viewed	2.992	2.413	2	3.339	2.449	3
- Viewed >1 story	0.633	0.484	1	0.739	0.440	1
- Browsing time (minutes)	4.652	3.368	4	5.471	4.230	4
- Points subtracted	7.667	11.132	0	9.306	12.279	10
- Points added	17.333	16.840	10	19.389	16.662	10
- Ever punished	0.458	0.500	0	0.517	0.500	1
- Ever rewarded	0.792	0.408	1	0.853	0.355	1
- Anger level (1–9)	2.487	1.904	1	2.522	1.919	2
- Degree of belief in donations	3.303	0.720	3	3.327	0.757	3
Observations	120			360		

Another important outcome is whether the participant visited more than one news story. Since each participant was only required to visit one story, any additional visits represent a choice by the subject to spend more time on the experiment without earning any additional money. Possible reasons for visiting additional stories include a simple desire to get information about the story, a desire to add or subtract more points, or an interest in how a story relates to the behavior of others. The last two factors appear to be important because the social treatment has a statistically significant positive effect on the probability of reading additional stories. The demographic variables are also important predictors. College education and previous protest behavior are predictors of visiting additional stories. However, different variables predict total browsing time. Men spend on average about 22 percent less time browsing. Older people also spend less time browsing. The social treatment is associated with a 17.5 percent increase in browsing time.

TABLE 1.2: Individual characteristics as predictors of behavior in the experiment

			Binary measures of behavior		
	Total stories viewed	Log total browsing time	Read > 1 story	Ever punished	Ever rewarded
College graduate	0.121^{*}	0.083	0.113^{***}	0.093^{**}	0.005
	(0.070)	(0.062)	(0.043)	(0.047)	(0.035)
Male	-0.025	-0.217^{***}	0.022	-0.117^{**}	0.002
	(0.070)	(0.065)	(0.042)	(0.048)	(0.035)
Has engaged in protest activity	0.134^{*}	-0.056	0.145^{***}	0.122^{**}	0.057
	(0.078)	(0.073)	(0.044)	(0.054)	(0.037)
Age	0.003	0.018^{***}	0.001	0.000	-0.000
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Has low wealth	0.090	0.083	0.031	-0.009	0.032
	(0.072)	(0.065)	(0.043)	(0.049)	(0.034)
Uses Facebook	-0.003	-0.017	-0.101^{**}	0.076	0.008
	(0.085)	(0.082)	(0.051)	(0.061)	(0.049)
Uses Twitter	0.035	-0.064	0.050	-0.010	0.113^{***}
	(0.071)	(0.061)	(0.041)	(0.048)	(0.032)
Owns smartphone	-0.014	-0.078	0.049	0.064	0.010
	(0.093)	(0.079)	(0.056)	(0.061)	(0.047)
Social treatment	0.121	0.175^{**}	0.113^{**}	0.075	0.061
	(0.081)	(0.069)	(0.049)	(0.052)	(0.042)
Obs.	477	477	477	477	477

Notes. The first two columns show semi-elasticities. The remaining columns show coefficient estimates (average marginal effects) from linear probability models. Heteroscedasticity robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The standard errors in Table 1.2 are heteroscedasticity-robust. The results are in general robust to clustering the standard errors by world. In some cases this approach provides stronger evidence of effects (see Appendix Table 1.17). For example, clustering the standard errors by world moderately shrinks the standard errors on the social treatment effect in all 5 models.

Figure 1.3 groups all participants according to their self-reported anger rating, which is intended to capture the degree of anger experienced overall during the experiment. The probabilities that the user "ever punished" and "ever rewarded" within each bin are displayed. Higher anger ratings are correlated with increasing probability of ever punishing. However, there is little relationship between rewarding and anger. Since participants on average visit more than one story, this result indicates that anger at one story does not tend to decrease the probability that the participant rewards some other organization. That is, emotional spill over effects appear to be fairly unimportant.

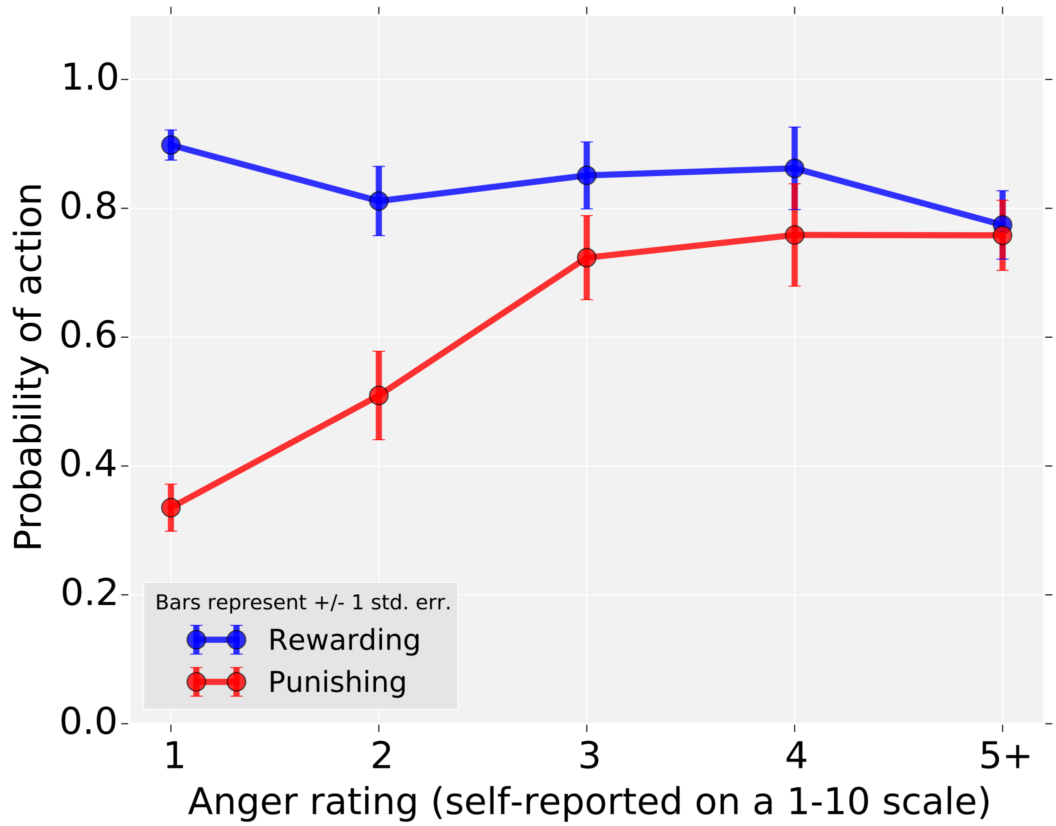


FIGURE 1.3: Probability that participant ever punished and ever rewarded *versus* anger rating

1.5.2. Analysis at the level of worlds

As discussed in the methods section, the analysis focuses on the net punishment and net reward metrics. However, additional world-level outcomes such as the total rewards (total number of points added) and total punishment are presented in the appendix along with tests for differences between the groups. These tests provide some weak evidence for positive treatment effects of the social condition on total reward, total punishment, number of organizations punished on net, and the efficiency of punishment (with p values in the range 0.10–0.20). There is no evidence of effects on the number of organizations rewarded nor reward efficiency.[8]

[8] Reward efficiency is defined as the ratio of net reward to total reward and must take a value in $[0, 1]$. Punishment efficiency is defined analogously.

Figure 1.4 shows how net punishment and net reward evolved in each of the 12 social worlds and 4 independent worlds. The two metrics are calculated after each of the 30 participants to produce the plotted time-series. It is evident that net reward tends to increase approximately linearly as individuals participate. However, while net punishment also tends to grow, it exhibits more variable evolution with more non-monotonicities. Since the punished organizations tend to be controversial rather than universally despised, there are often individuals willing to reward even heavily punished organizations, which then decreases the net punishment.

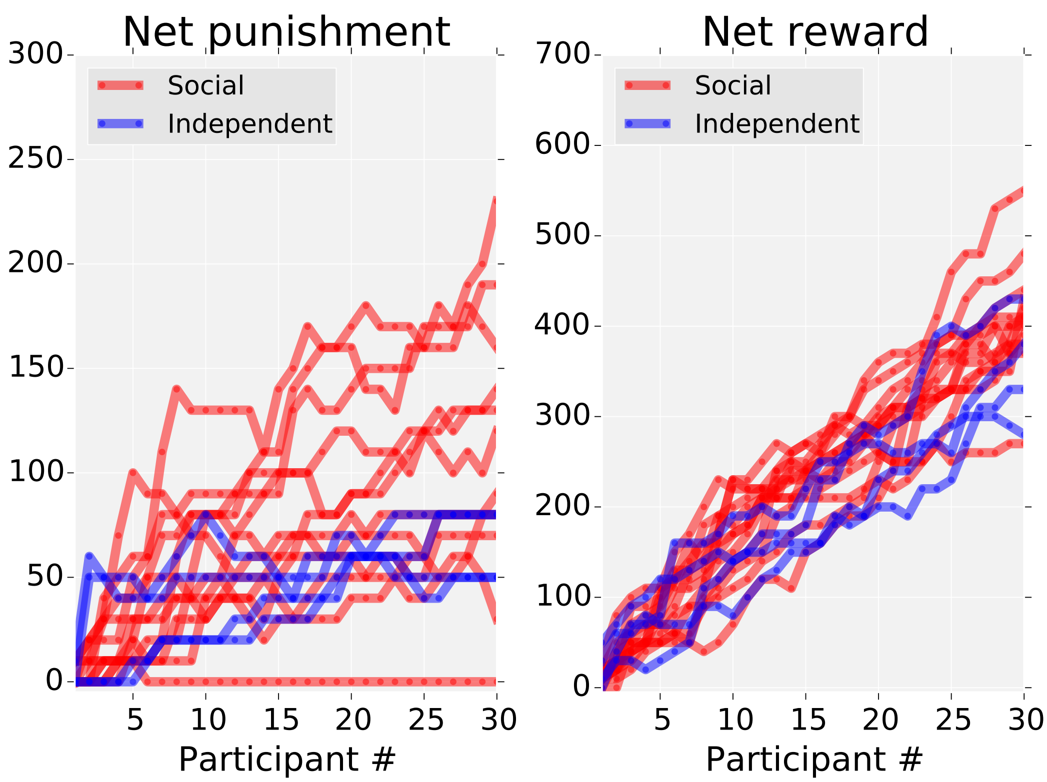


FIGURE 1.4: Evolution of net reward and punishment in the 16 worlds

The means of the net reward, net punishment, and the Theil index are displayed in Figure 1.5. Each panel shows the difference in means d and the two-tailed p-value of the percentile bootstrap test. The null hypothesis is that the means are equal in the social and independent conditions. The net punishment is higher in the social condition, but the test does not reach statistical significance at conventional levels (412.5 *vs.* 354.6, $p = 0.148$). Mean net punishment is higher in the social condition (110.0 *vs.* 64.0, $p = 0.067$). The most robust difference between the two conditions is in wealth inequality (Theil index), which is significantly higher in the social condition (0.0040

TABLE 1.3: Summary statistics for world-level metrics: Social vs. Independent

	Independent		Social		p-value		
	Mean	SD	Mean	SD	U-test	t-test	Levene's
Net punishment	64.03	32.09	110.00	65.37	0.080	0.204	0.075
Net reward	354.57	70.60	412.50	66.35	0.101	0.153	0.781
Theil index*100	0.23	0.08	0.40	0.12	0.005	0.017	0.001

Notes. Test results are conventional tests based on the aggregate data.

vs. 0.0023, $p = 0.005$). The results from using a Mann-Whitney U test on the world-level data are similar in all three cases. For net punishment, net reward, and the Theil index, respectively, the test statistics are $(U = 12.0, p = 0.080)$, $(U = 13.0, p = 0.101)$, and $(U = 2.0, p = 0.005)$. The explanation for this difference can be seen in the net reward and punishment metrics. Under the social condition, the popular organizations are rewarded more intensely while the unpopular ones are punished more intensely. This generates greater inequality in the final points totals of the organizations.

Comparing the variability of the outcomes indicates that the social condition increased the variability of net punishment and wealth inequality. Differences in the variances of the outcomes are tested using Levene's test on the world-level outcomes. The results follow a similar pattern to the means. No evidence is found for a difference in the variance of net punishment between the two conditions. The standard deviations of net reward are very similar in the two conditions: 66.4 in the social condition and 70.6 in the independent condition. However, the standard deviations of net punishment in the social and independent conditions are 65.4 and 32.1, respectively ($p = 0.075$). The standard deviation of the Theil index in the social condition is about 55 percent higher than in the independent condition ($p < 0.01$).

Figure 1.6 displays the results of the alternative approach to testing using the re-sampling tests. This approach tests whether the outcome in the social condition is likely to have been generated by the distribution created by re-sampling individuals from the independent data. Four different world-level metrics are examined: (1) mean net reward, (2) standard deviation of net reward, (3) mean net punishment, and (4) standard deviation of net punishment. The histograms show the realizations of each metric computed from samples of 12 worlds drawn from the re-sampling distribution. The actual values computed from the 12 social worlds are marked on each plot with

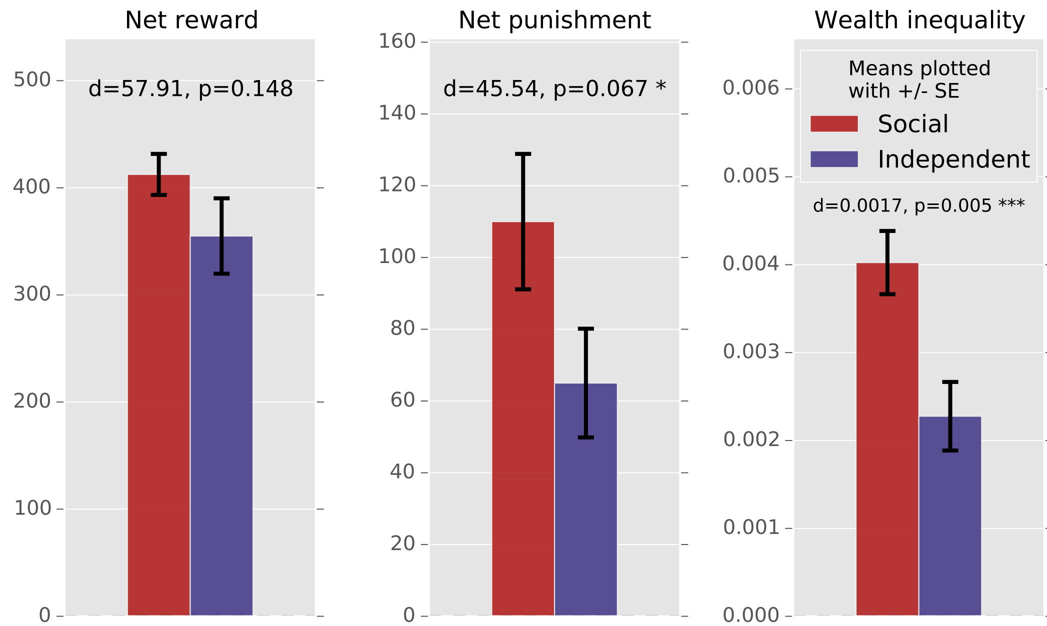


FIGURE 1.5: Mean world-level outcomes: Social *versus* independent conditions

a vertical line. For three of the four metrics, the observed statistic in the social worlds is far out into the tail of the re-sampling distribution. For example, among the 12 social worlds the sample standard deviation of net punishment is 65.4. However, this value is larger than any of those generated by re-sampling 12 independent worlds and then computing the sample standard deviation from those 12 values. In the fourth metric, standard deviation of net reward, the observed value in the social worlds is very close to the center of the re-sampling distribution.

1.5.3. Analysis at the level of choices

The final section of the analysis examines how individual participants' choices result in the aggregate effects observed in the social condition. This analysis uses participant-by-story panel data where each participant contributes an observation for each of the 24 stories (organizations). The social condition data has 8,640 observations while the independent condition data has 2,880 observations. Individuals are modelled as making 24 binary decisions (about each decision type: punishing, rewarding, and viewing). These decisions are allowed to depend on the features of the stories at the time that the relevant participant views the news page. The most important features are the points added and subtracted by previous participants. Functions of these points are included in the model to allow for social influences of punishment and reward. The initial models simply in-

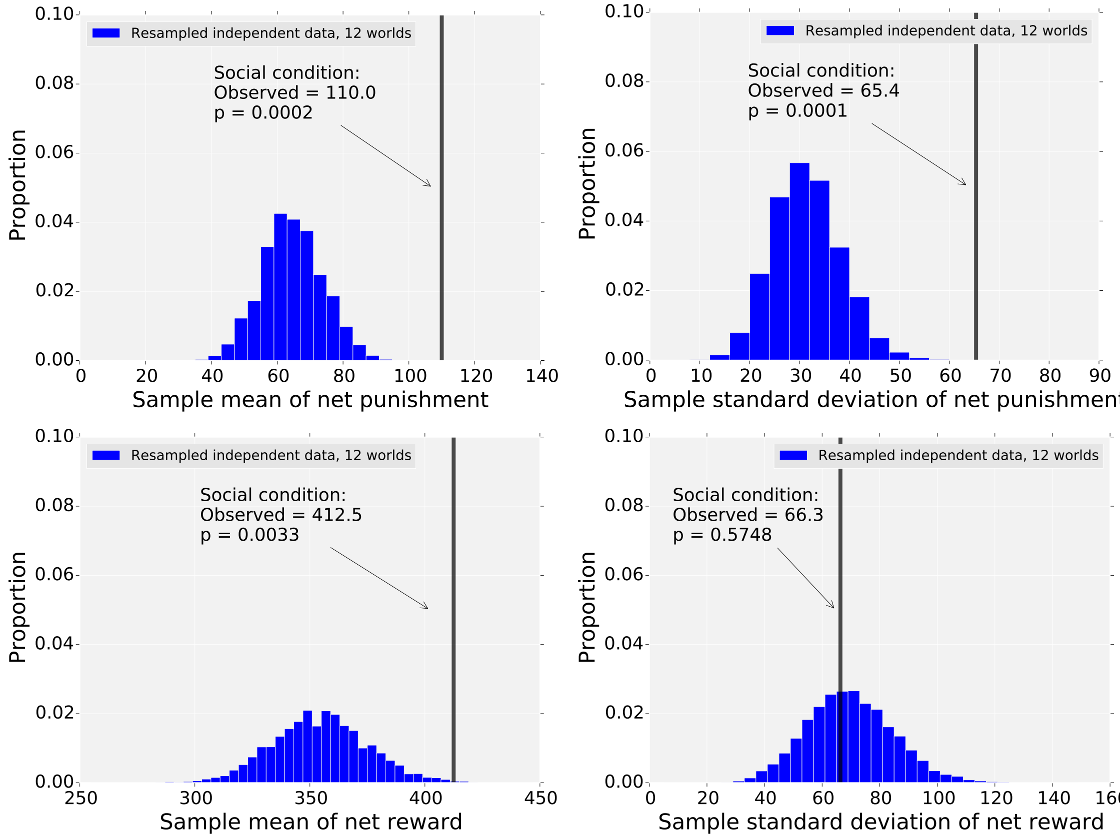


FIGURE 1.6: Resampling tests: Social vs. independent

clude binary variables that indicate whether a given story was punished at all or rewarded at all by the previous participants in the world. However, the results indicate that this binary effect decays rapidly over the course of a given world. Therefore, the models also include the interaction of the dummy variables with the logarithm of the participant's number within the world (from 1 to 30). This feature is called logarithmic decay. The logit coefficient estimates in Table 1.5 show a significant negative effect on the interaction variable, which strongly supports a decaying social effect. A behavioral interpretation of this feature is that social information preferentially attracts attention to those stories that have been previously punished or rewarded. However, the social information may not increase the overall propensity of a participant to punish or reward. Such a model would generate an effect that decays over the course of the experiment as more stories are punished or rewarded. An **example specification** with binary effects of points added and subtracted that decay logarithmically would have the form

$$
\begin{aligned}
&P(i \text{ punishes story } k) = \\
&\quad \Lambda(s_k + \beta_1 \text{PagePosition}_{ik} \\
&\quad + \beta_2 \text{PagePosition}_{ik}^2 \\
&\quad + \beta_3 \log(\text{Participant \#}) \\
&\quad + \delta_1 \text{AnyPointsAdded}_{ik} + \gamma_1 \text{AnyPointsAdded}_{ik} \times \log(\text{Participant \#}) \\
&\quad + \delta_2 \text{AnyPointsSubtracted}_{ik} + \gamma_2 \text{AnyPointsSubtracted}_{ik} \times \log(\text{Participant \#})),
\end{aligned}
$$

where Λ is the logistic function. The variable $\text{AnyPointsAdded}_{ik}$ takes the value one if any participant before i added points to (rewarded) organization k and zero otherwise. The variable $\text{AnyPointsSubtracted}_{ik}$ takes the value one if any participant before i subtracted points from (punished) organization k and zero otherwise.

Average marginal effects using the binary definition of social effects appear in Table 1.4. The logit coefficient estimates appear in Table 1.5. *These models do not yet include terms for the page position of the headlines.* Data from only the social condition is used to estimate the models in the first three columns, which are logit models of the probability of any given participant punishing, rewarding, or viewing any given story. The point estimates are the average marginal effects of the social effect variables. All three models show that if a participant punishes or rewards a story, then subsequent participants are significantly more likely to punish, reward, or view that story. For example, the average marginal effect of punishment on a given story is to increase, for each subsequent participant, that story's (1) probability of punishment by 0.027 ± 0.008, (2) probability of reward by 0.020 ± 0.008, and (3) probability of being viewed by 0.049 ± 0.012. The average marginal effect of prior reward is to increase the probability of subsequent punishment by 0.016 ± 0.005 and the probability of subsequent reward by 0.034 ± 0.009. These social effects therefore tend to concentrate more punishment on stories that have been punished before and more reward on those that have been rewarded before. This pattern tends to increase inequality across the stories, which is the result seen in the aggregate data. It may be surprising to see that both punishment and reward tend to increase later punishment and reward. However, both behaviors strongly draw additional attention (views), which drives the additional punishment and reward. Later analyses

TABLE 1.4: Simple effects of punishment/reward on individual behavior (page controls omitted)

	Social condition Individual behavior:			**Independent condition** (placebo test) Individual behavior:		
	Punish	Reward	View	Punish	Reward	View
Social effect:						
Points sub'ed (dummy)	0.027^{***}	0.020^{**}	0.049^{***}	-0.042^{***}	0.009	-0.018
	(0.008)	(0.008)	(0.012)	(0.013)	(0.017)	(0.019)
Points added (dummy)	0.016^{***}	0.034^{***}	0.059^{***}	-0.019	-0.011	-0.022
	(0.005)	(0.009)	(0.011)	(0.014)	(0.018)	(0.020)
Individuals	360	360	360	120	120	120
Obs.	8,640	8,640	8,640	2,400	2,760	2,760
Avg. log-likelihood	-0.145	-0.256	-0.367	-0.143	-0.241	-0.340

Notes. Average marginal effects of previous punishment (points sub'ed) and reward (points added) printed. Effects are specified in binary form with logarithmic decay. Estimates come from a logit model with a dummy variable for each story but *no controls for page position*. Standard errors, in parentheses, are clustered by participant. Standard errors, in parentheses, are clustered by participant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

show that this attentional effect predominantly accounts for the social effects in the present models.

Table 1.4 also shows the results from estimating identical models using only data from the independent condition (columns 4–6) as a placebo test. These models reveal little evidence of social effects, and most of the point estimates are negative. This result starkly shows that the social condition causes any activity towards a particular story to attract additional attention towards the same story. This effect is the main underlying mechanism for the increase in points inequality in the social condition. In particular, these models show that rewards have a relatively large effect on attracting attention (0.059 ± 0.014).

The placebo tests indicate a significant *negative* effect of punishment on subsequent punishment. This estimate is not robust to plausible alternative specifications of the social effect that also fit the data well (Table 1.6). One such case is to treat the social effects as *shares* of punishment and reward. This specification explicitly represents the idea of social effects being diluted and decaying as more stories are punished and rewarded. In this specification, if n stories have been punished, then the punishment variable for those stories is set to $\frac{1}{n}$. The remaining $24 - n$ stories will have the punishment variable set to 0. The reward variable is defined analogously. This definition allows the social effects to be diluted. An interpretation of this definition is that any existing punishment generates one unit of additional propensity to punish, which is distributed uniformly

TABLE 1.5: Coefficient estimates from punish/reward logit models (page controls omitted)

	Punishment		Reward	
	(1) Social	(2) Independent	(3) Social	(4) Independent
Points sub'ed (dummy)	2.222***	-0.545	-0.670	0.512
	(0.562)	(1.199)	(0.546)	(0.858)
Points sub'ed × Log(participant #)	-0.661***	-0.215	0.375**	-0.156
	(0.202)	(0.445)	(0.185)	(0.305)
Points added (dummy)	0.621	-2.067	1.360***	-0.611
	(0.591)	(1.379)	(0.364)	(0.849)
Points added × Log(participant #)	-0.070	0.585	-0.373***	0.179
	(0.218)	(0.502)	(0.139)	(0.297)
Individuals	360	120	360	120
Obs.	8,640	2,400	8,640	2,760
Avg. log-likelihood	-0.14450	-0.14308	-0.25634	-0.24144

Notes. Table displays coefficients from logit models using participant-by-story panel data and *no controls for page position*. The points subtracted (added) dummy indicates if the story was punished (rewarded) by any previous participant. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

over all stories that have been punished. The average marginal effect of such a social variable can be complicated to interpret. In terms of a social effect of punishment, the marginal effect could be viewed as the change in the outcome probability for a story when it has been punished and no other story has been punished. Analogous social and placebo results for the share specification appear in Table 1.6. These again reveal substantial positive effects of punishment and reward on all behaviors in the social condition. As expected, no social effects are found in the independent condition.

Returning attention to the binary specification in Table 1.4, one might note that the average marginal (social) effects of punishment on punishment and rewarded are similar in magnitude. It may therefore appear that these effects will cancel out and therefore the effect on net punishment may be difficult to account for. These results highlight the difficulty in understanding the experiment by looking only at average marginal effects over the entire data set or at model coefficients. Much of the remaining analysis will present marginal effects in graphical form in order to understand how the effects vary over the course of a world.

Further understanding the relationship between individual behavior and overall outcomes requires a more subtle analysis along with careful consideration of the punishment and reward pre-

TABLE 1.6: Share effects of punishment/reward on individual behavior (social vs. independent)

	Social condition Individual behavior:			**Independent condition** (placebo test) Individual behavior:		
	Punish	Reward	View	Punish	Reward	View
Social effect:						
Points sub'ed (share)	0.055^{***}	0.057^{**}	0.131^{***}	-0.084	0.080	0.035
	(0.014)	(0.025)	(0.033)	(0.085)	(0.073)	(0.099)
Points added (share)	0.086^{***}	0.190^{***}	0.372^{***}	-0.029	-0.027	0.086
	(0.023)	(0.040)	(0.061)	(0.102)	(0.114)	(0.100)
Individuals	360	360	360	120	120	120
Obs.	8,640	8,640	8,640	2,400	2,760	2,760
Avg. log-likelihood	-0.145	-0.257	-0.368	-0.146	-0.241	-0.340

Notes. Average marginal effects of previous punishment (points sub'ed) and reward (points added) printed. Effects are specified in the share form. Estimates come from a logit model with a dummy variable for each story but no controls for page position. Standard errors, in parentheses, are clustered by participant. Standard errors, in parentheses, are clustered by participant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

dictor variables. First, it is possible to further divide the stories into those that have been punished only and not rewarded. Therefore, alternative models were estimated where the observations are partitioned into those that have been (1) neither punished nor rewarded, (2) punished only, (3) rewarded only, or (4) both punished and rewarded, each interacted with a logarithmic decay. The probabilities of punishment and reward over the course of a world for group 2 (punished only) are printed in the first panel of Figure 1.7. This result shows that among stories that have been only punished the probability of subsequent punishment is always greater than or equal to the probability of reward. Therefore, among these stories the net punishment will tend to increase. A second factor to consider is that the average marginal effects in Table 1.4 do not account for the actual patterns of punishment. Since this model is non-linear, the effect of punishment can vary depending on the values of the covariates (in this case, the story dummy variables). Panel two of Figure 1.7 shows the average marginal effect of punishment on subsequent punishment but with the effects broken out into two groups: (1) those observations (stories) that were actually punished during the experiment and (2) those observations that were not punished. That is, the marginal effects were computed separately over the two corresponding sets of observations in the panel data. This result shows that the marginal effect of punishment is greater among those stories that were actually punished, typically by a factor of 2. In effect, the participants selectively targeted the stories where punishment would have the greatest effect on motivating subsequent punishment.

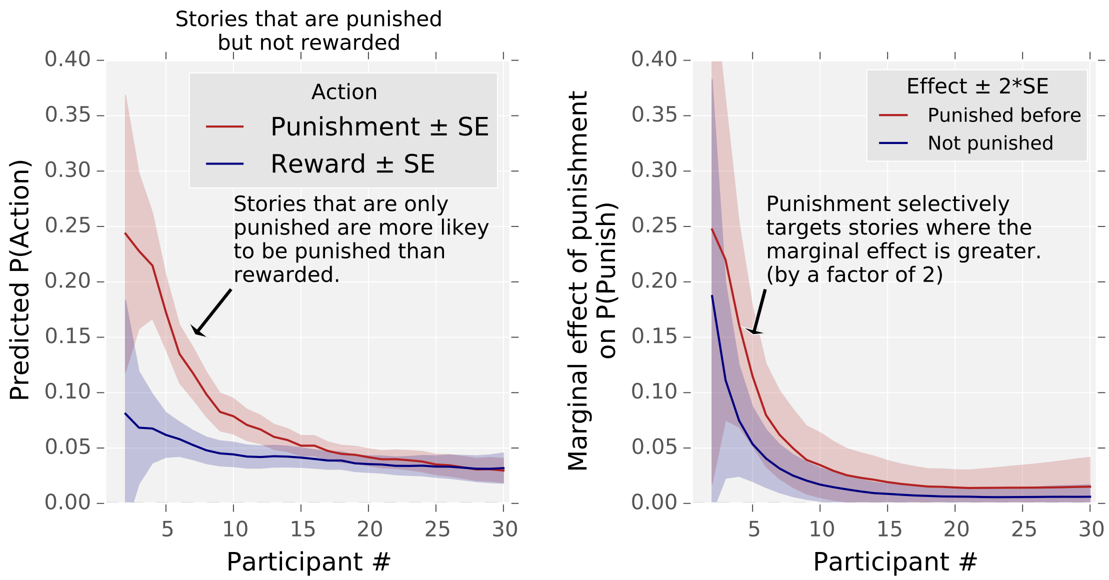


FIGURE 1.7: Additional characteristics of punishment

1.5.4. Number of organizations with leverage on the net metrics

The aim of the next analysis is understand why net punishment tends to evolve differently in social worlds and independent worlds (as seen in net punishment "paths" plotted in Figure 1.4). In particular, net punishment tends to keep growing and varying in the social worlds, whereas the metric tends to stay relatively stable in the independent worlds.

Recall that net punishment is calculated by taking all organizations with wealth < 300 and summing up their deviations from 300 points. Therefore, net punishment only changes when a participant (1) punishes an organization with wealth ≤ 300 or (2) rewards an organization with wealth < 300. Any organization with wealth > 300 has zero net punishment and no "leverage" on the net punishment number. For example, suppose an organization currently has 370 points. If a new participant punishes that organization, its points will be reduced to 360, which is greater than 300. Since only deviations below 300 points contribute to net punishment, the organization with 370 points has *no leverage* on the net punishment metric.

Table 1.7 shows this relationship. An organization has *leverage on net punishment* if it has wealth ≤ 300. Conversely, an organization has *leverage on net reward* if it has wealth ≥ 300.

TABLE 1.7: Leverage of an organization on the net punishment metric

Number of points the organization currently has	Leverage	Effect on *net punishment* of one more instance of... Punishment	Reward
< 300	Yes	+10	-10
300	Yes	+10	0
> 300	No	0	0

Organizations with exactly 300 points have leverage on both net reward and net punishment.

Definition 6 *The* numbers of organizations with leverage *for participant i are given by the expressions*

$$\sum_{k=1}^{24} \mathbf{1}\{v_{k,i} \leq 300\} \qquad \text{(number of organizations with leverage on net punishment)}$$

and

$$\sum_{k=1}^{24} \mathbf{1}\{v_{k,i} \geq 300\} \qquad \text{(number of organizations with leverage on net reward)}$$

where $v_{k,i}$ is the wealth (points) of organization k when participant i arrives at the news page.

Consider the independent condition. By the time the 29th participant reaches the news page, most of the organizations will have wealth $>$ 300 because of rewarding behavior by the earlier participants. If there are few organizations with wealth $\leq$ 300, then it is unlikely that the 29th participant would, by chance, view a story with wealth $\leq$ 300 and act on it so as to influence net punishment. In addition, there is no social information telling the participant which organizations are low on points. In an abstract sense, it is "difficult" for a participant in this environment to have an immediate effect on the net punishment metric. Such a "difficult" situation is typical of the later participants in a world.

However, in the social condition, the participants can see the points of the organizations. This information allows a participant to seek out those organizations with wealth $\leq$ 300 and add or subtract points. If participants in reality do this, then participants in the social condition should be more likely to affect net punishment. **In addition, the advantage provided by the social information matters most when there are few organizations with leverage on net punishment.** We test for this effect in the following analysis.

To test for this relationship a simple logit model was estimated on the participant-level data (480 observations).[9] The model predicts whether or not the participant had any effect on net punishment during his visit to the page. Alternatively, we could think of the model as describing whether the participant made the net punishment path in Figure 1.4 tick up or down. The right-hand side variables were (1) an indicator for the social condition, (2) a scalar term showing the number of organizations with leverage on net punishment at the time the participant arrived at the page, and (3) the interaction of variables 1 and 2. The coefficients from estimating this model appear in Table 1.8. The average marginal effects are plotted in Figure 1.8. The table also includes the analogous but "inverted" model for net reward. This model replaces the left-hand side with an indicator for influencing net reward and the second predictor variable with a term showing the number of organizations with leverage on net reward. The first two columns show coefficient estimates for affecting net punishment. The scalar term has a positive effect, which means that a participant is more likely to influence net punishment when there are more organizations with leverage on net punishment. The other way of viewing this is that a participant is *less* likely to influence net punishment when few organizations have leverage on the metric. However, this relationship is inverted when looking at the social condition. The interaction term is negative and larger in magnitude than the simple scalar term. In addition, the effect of the social condition dummy is positive and significant.

The overall result is that when the number of organizations with points ≤ 300 is small, the participants in the social condition are more likely than those in the independent condition to influence net punishment. This relationship is depicted in Figure 1.8 which shows average marginal effects of the social condition evaluated at different values of the scalar variable in the regression. The low end of the x-axis corresponds to the *later* part of a world. The positive marginal effects at the low end of the x-axis confirms the idea that social information can help participants to target actions on the few organizations with leverage. The average marginal effects from the net reward model are also plotted. These results do not reveal any effect of the social condition on the probability of influencing net reward. This finding makes sense because there are usually many

[9] This deliberately simplified for illustrative purposes. The appendix includes a corresponding analysis using the panel data, where page position and story effects can be incorporated into the model to control for dependencies. See Appendix Table 1.13. These results provide even stronger evidence that the leverage variable and its interaction with the social dummy should be in the model along with the social dummy. The joint test of those three variables is significant at level 0.001.

TABLE 1.8: Coefficient estimates from models of the participant-wise probabilities of affecting net punishment and reward

	Left-hand side variable (binary): The participant changed the net metric listed below during his visit to the news page.			
	Net punishment		Net reward	
	(1)	(2)	(3)	(4)
Social condition (dummy)	0.552^{**}	1.814^{**}	0.370	-0.111
	(0.219)	(0.851)	(0.287)	(3.415)
State of the news page when visited by the participant:				
Number of organizations w/ points ≤ 300 [i.e., with leverage on net punishment]		0.061		
		(0.050)		
Social dummy $\times$ (# orgs. w/ points ≤ 300)		-0.086		
		(0.055)		
Number of organizations w/ points ≥ 300 [i.e., with leverage on net reward]				-0.064
				(0.149)
Social dummy $\times$ (# orgs. w/ points ≥ 300)				0.022
				(0.163)
Average marginal effect of social cond.	0.133	0.138	0.047	0.044
Significance of AME of social cond. (p)	0.009	0.006	0.225	0.253
Joint sig. of 3 coefs. above (p)	.	0.032	.	0.523
Individuals	480	480	480	480

Notes. Coefficient estimates from logit models with standard errors in parentheses. The average marginal effect is the average effect on the probability of the modeled outcome. The printed p-value of the AME corresponds to a test with the null hypothesis that the AME is 0. The joint significance test has the null hypothesis that all 3 coefficients in the column are zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

organizations with leverage on net reward. They are not "difficult" to find, so social information is less important for finding them. The supports of the data on the x-axis correspond to the heavily shaded areas of the plot. For example, the number of organizations with leverage on net reward never dropped below 15.

The consequence of this result at the aggregate level is that as a social world evolves we see net punishment continue to change (mainly increasing), which leads to the higher levels of net punishment and higher variance of net punishment observed in the aggregate results. Another way to interpret this result is to note that net punishment evolves like a random walk with drift. The social condition allows this "random walk" to have a greater number of time steps because each participant has a greater probability of making the walk transition to a new state (as shown in this analysis). A higher number of steps translates to higher variance at the end of the world. However, the results for net reward do not show the increasing marginal effect seen for net reward. No significant advantage is seen for the social condition in changing net reward. This finding agrees with the world-level results where there is no evidence of a difference between the two conditions in the variance of net reward.

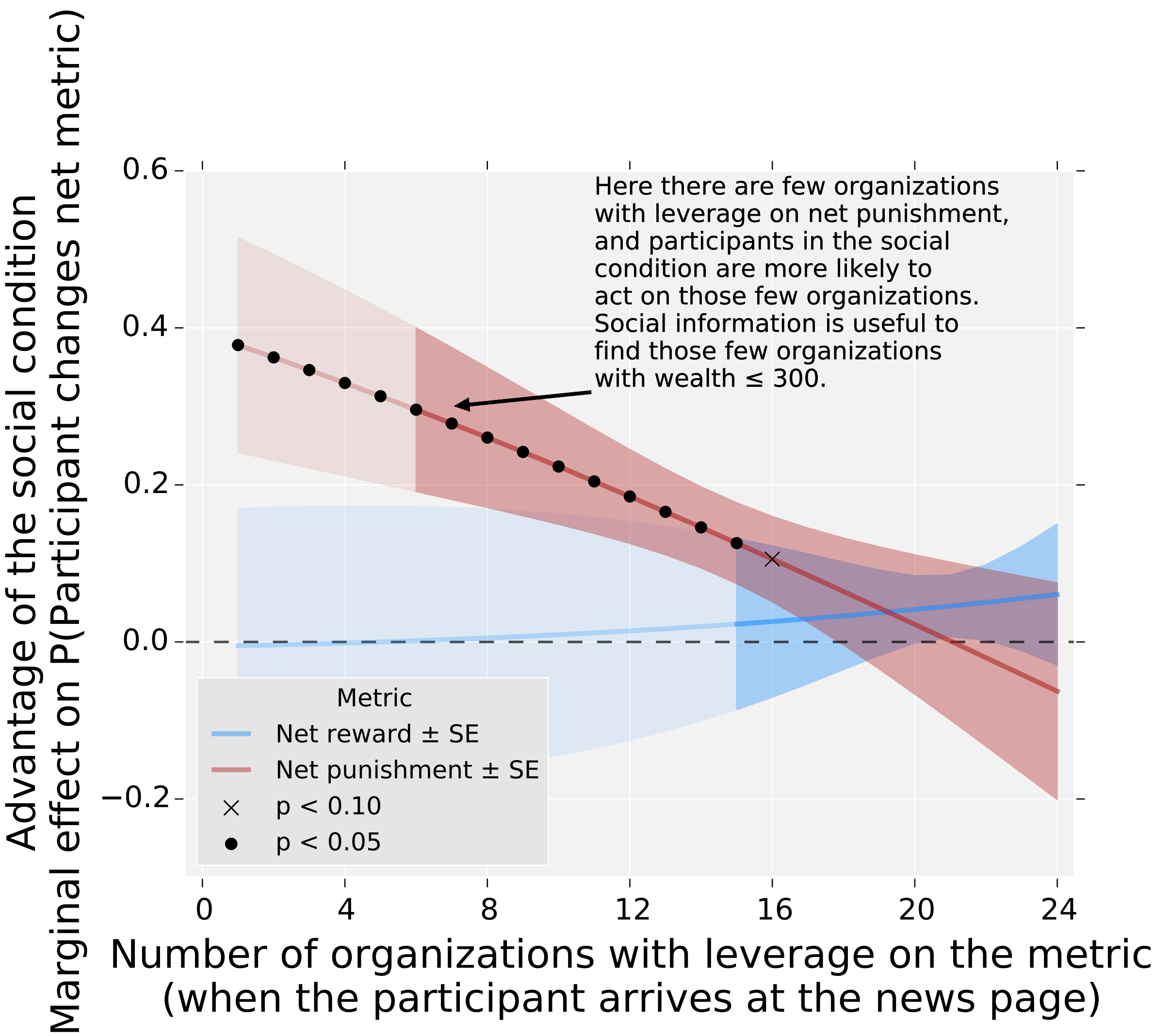


FIGURE 1.8: Effect of the social condition on choices that affect net punishment/reward

TABLE 1.9: Social effects on behavior when controlling for page position

	Not controlling for page position			Controlling for page position		
	Punish	Reward	View	Punish	Reward	View
Social effect:						
Points sub'ed (dummy)	0.027***	0.020**	0.049***	0.006	-0.016**	-0.017
	(0.008)	(0.008)	(0.012)	(0.008)	(0.008)	(0.011)
Points added (dummy)	0.016***	0.034***	0.059***	-0.002	-0.019*	-0.020
	(0.005)	(0.009)	(0.011)	(0.007)	(0.011)	(0.013)
Individuals	360	360	360	360	360	360
Obs.	8,640	8,640	8,640	8,640	8,640	8,640
Avg. log-likelihood	-0.145	-0.256	-0.367	-0.142	-0.250	-0.356

Notes. Average marginal effects of previous punishment (points sub'ed) and reward (points added) printed. Effects are specified in the binary form with logarithmic decay. Estimates come from a logit model with a dummy variable for each story. Page position is controlled for by a quadratic function. Standard errors, in parentheses, are clustered by participant. Standard errors, in parentheses, are clustered by participant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

1.5.4.1. Social influence vs. attention manipulation by algorithm

The previous analyses of the effects of points subtracted and added in the social condition did not include controls for the effect of page position. Therefore, the social effects represented a combination of any direct social influence and the attentional effect of shifting the location of a story on the page. That is, because punishing and rewarding imply viewing a story, the social variables are highly correlated with the story being near the top of the news page, which itself greatly increases the probability of being viewed. To disentangle these effects I estimated the same models with the addition of a quadratic control for page position. The results of this exercise appear in Table 1.9. The estimates from Table 1.4 are reprinted for convenience. Once page position controls are added, many of the social effects become small and insignificant. This finding indicates that the mechanical page position effects dominate the social effects. Once they are accounted for, the participants put little additional weight on points added and subtracted overall. Negative effects are also consistent with a preference for fairness wherein participants prefer to reward organizations that have not yet been rewarded.

The evidence indicates that participants are, in some sense, passive and highly subject to the algorithm that constructs the page (in this case, sorting by views). However, the participants do not entirely disregard social information. The results indicate that social information does have direct effects (controlling for page position), but these effects occur very early in the course of a world. First, alternative specifications using the share definition of social effects reveal robust positive effects of points subtracted on subsequent punishment (see Appendix Table 1.12). These specifications essentially overweight the effects that occur early in a world. Second, these social effects appear when we explicitly examine average marginal effects over the course of a world. To explore individual behavior in greater detail I estimated the familiar logit models predicting punishment, reward, and viewing. Each model again includes story dummies, quadratic page position controls, linear specifications of the points added and subtracted, and those points variables interacted with a quadratic function of the log participant number. The exact specification of the points and the function of the participant number make relatively little difference to the results. Analogous models using binary points effects are printed in the appendix (see Appendix Figure 1.24).

In addition, a model of each behavior was estimated with quadratic controls for the page position, linear terms for added and subtracted points, and quadratic logarithmic decay interactions. This particular specification was chosen based on fit statistics, but a model with binary social effects gives similar results (see Appendix Table 1.24). The average marginal effects of punishment and reward were evaluated at each participant number and are displayed in Figure 1.9. Since these models are based on the entire social data set they are called the unconditional estimates. These results reveal little evidence of social effects. Punishment and, to some extent, reward are found to have some negative effects on rewarding among early and middle participants. The negative effects of reward on rewarding are quantitatively small and not entirely robust to alternative specifications (see additional results in the appendix). These effects are largely accounted for by the increase in exploration among participants in the middle range of a world. However, the effects of punishment show some true social influence. Even controlling for page position, punished organizations are significantly less likely to be rewarded (by a few percentage points) during the first third of a world.

Figure 1.9 also shows results from models estimated using only the participant-by-story observations where the participant viewed the story. These models are called "conditional on viewing." The estimated coefficients for punishment and reward are printed in Table 1.26. These results show significant effects on punishment, which increases the probability of punishing (conditional on viewing) by up to 20 percentage points. The probability of rewarding (conditional on viewing)

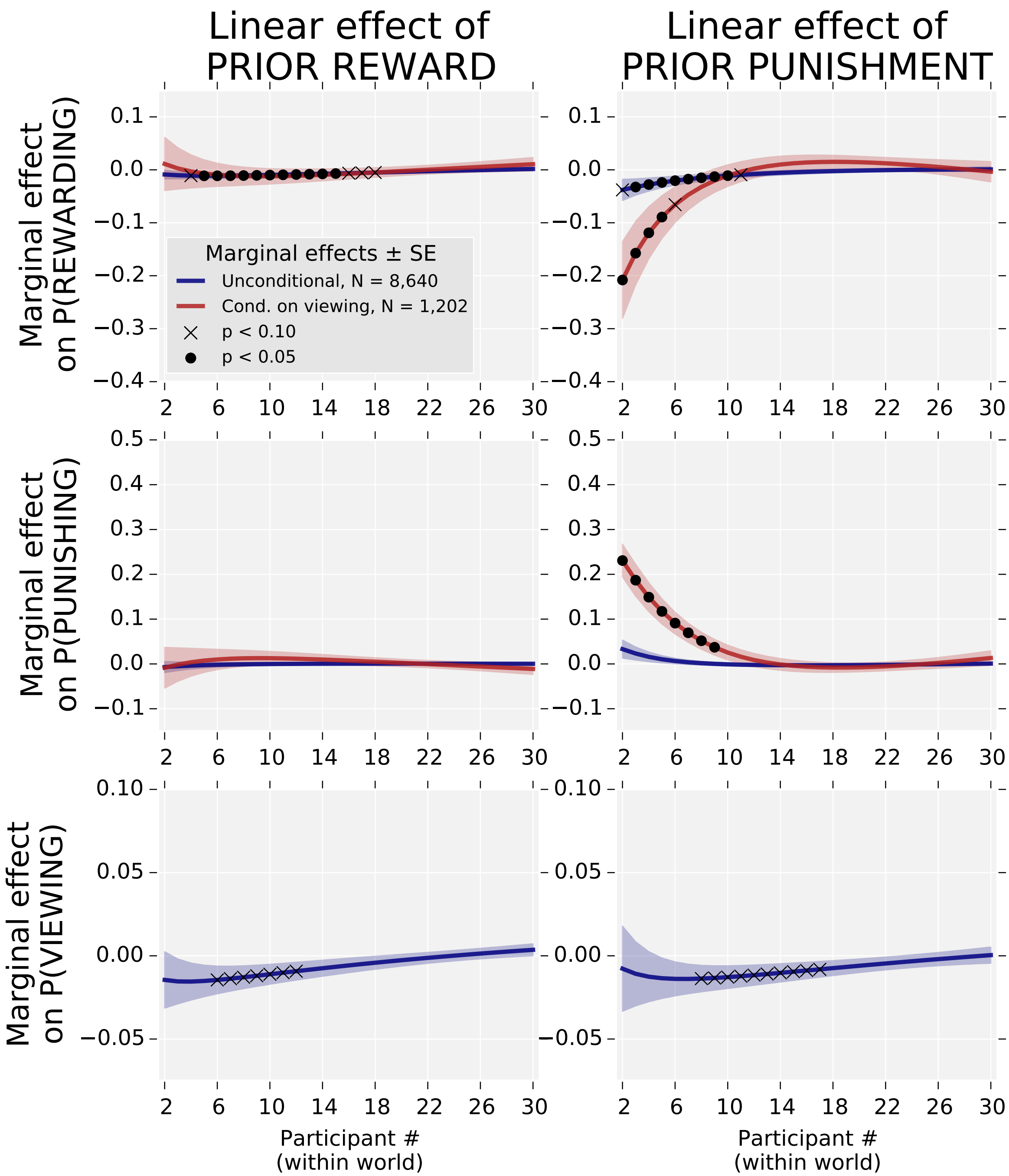


FIGURE 1.9: Effect of previous punishment and reward on individual behavior

is decreased by a similar amount. The similarity of the two effect magnitudes suggests that punishment may persuade those who would otherwise reward to punish instead. The models' estimated coefficients are printed in Table 1.26, which again shows that punishment has effects on punishment and reward that are similar in magnitude but opposite in sign. However, it is likely that, in addition to persuasive effects, selection also plays a role. Punished stories may attract the attention of individual participants that have a high propensity to punish and low propensity to reward. The decaying form of the effect also suggests an important role for selection.

The same conditional-on-viewing models do not show any social effects of reward on subsequent reward or punishing. Therefore, punishment seems to be special in generating social effects that go beyond algorithmic manipulation of attention.

1.6. Conclusion

The experiment revealed that adding social information to the media environment had two main effects on aggregate outcomes. First, the inequality in points across the organizations became more unequal. Effectively, the popular organizations tended to be more popular while the unpopular organizations tended to be more unpopular. Second, the expected net punishment in a given world became greater but also more variable.

A further investigation of the individual behavior during the experiment provided some answers about why the aggregate outcomes were affected by social information. The page-construction algorithm, which sorted the most-viewed stories to the top of the headlines page, played a dominant role in concentrating attention in a herding-like way. That is, participants always had to view a story before punishing or rewarding. These views tended to push that story to the top of the page where it was more likely to be viewed, which in turned caused the story to receive more punishment or reward. This mechanism generated a strong positive correlation between being punished/rewarded in the past and being punished/rewarded in the future. However, once the models accounted for the position of the headline on the page, the effects of social information about punishment and reward were greatly diminished. This result suggests that participants offloaded a substantial portion of their decision making process to the page-constructing algorithm. The actual effect of the algorithm on choices depends on how well it aligns with the participants' own preferences. If they are perfectly aligned, then the participants would, by some measure, make better choices or make choices as if they had zero search costs or "thinking aversion" (Ortoleva 2013). Alternatively, if the participants incur search costs, then the choices of participants could be systematically manipulated by the algorithm. One such example is the herding experiment on an artificial music market by Salganik and Watts (2008), in which social information about song popularity was deceptively inverted.

That finding gives a different impression from recent research by Bakshy et al. (2015) on news consumption through Facebook. Those researchers argue that political bias in news consumption was driven more by individuals' own choices than through the Facebook News Feed algorithm. In practice, the degree of selection through the two sources will be highly dependent on the particular context and algorithm used. Future research might investigate this topic by varying the page/menu-construction algorithm in a way somewhat like Salganik and Watts (2008) along with the types of social information available. The present experiment provided a fairly sparse form of social information. Richer forms of communication may lead to stronger selection through individual choice.

The present results also reveal an asymmetry in positive and negative social effects (reward and punishment). This finding is consistent with research showing that negative events and information tend to have greater effects than positive (Baumeister et al. 2001). Punishment was found to have two substantive social effects, whereas reward had little. Participants used social information to selectively target unpopular organizations (mostly for punishment but also reward). It could be argued that this result only occurred because unpopular organizations tended to be much rarer than popular. If popular organizations were rare instead, it is possible that participants would then use social information to target them. However, a second unique effect of punishment is that it had a significant social effect even after controlling for the effect of the page-construction algorithm. When participants viewed a story about a previously punished organization, they were significantly more likely to punish that organization instead of reward. These findings suggest that, compared to positive activity, negative activity may be more dependent on herding-like behavior.

1.7. Appendix of chapter 1

1.7.1. Examples of real Internet punishment

TABLE 1.10: 48 selected cases of punishment and shaming involving the Internet

Case	Time	Situation	Consequences
George & Dragon British Pub, Orlando, FL[1]	May 2015	Owner accused of racist threat with gun.	Angry Yelp reviews posted. Owner arrested.
Melbourne Man, Melbourne, Australia[2]	May 2015	Mother posted photo of man on Facebook with accusation that he was sex offender.	Man received death threats but was cleared by police.
Rubbin Buttz BBQ, Milliken, CO[3]	May 2015	Enacted 10% discount for white people.	Angry Yelp reviews posted. Discount extended to all customers.
PLOS ONE[4]	May 2015	Reviewer asked female authors to add a male co-author.	Authors posted reviewer reports on Twitter. Reviewer was banned by PLOS ONE.
Cigars & Stripes Bar, Berwyn, IL[5]	April 2015	Two-way mirror allows looking into women's restroom.	Angry Yelp reviews posted. Police investigated.
Alphagraphics, Suwanee, GA[6]	April 2015	Refused to print invitations for a gay wedding.	Angry Yelp reviews posted.
Popeyes Chicken, Channelview, TX[7]	April 2015	Fired employee that was robbed.	Angry Yelp reviews. Employee was offered job back.
Dieseltec, Grandville, MI[8]	April 2015	Announced policy of not serving gay clients.	Angry Yelp reviews posted.
Clorox[9]	April 2015	Accused of a racist tweet.	Attacked by Twitter users.
New York University[10]	April 2015	Facebook user accused campus store of having sexist baby clothing.	Attacked on social media by students and almuni.
Memories Pizza, Walkerton, IN[11]	April 2015	Announced policy of not serving gay clients.	Attacked on Yelp. GoFundMe campaign launched in support.
Days Inn, Pine Bluff, AR[12]	March 2015	Fired employee for speaking to a reporter about a minimum wage bill.	Attacked on Yelp and social media.
F&R Auto Sales, Westport, MA[13]	Jan. 2015	Leaked video of argument with pizza delivery man.	Attacked on Yelp/Google reviews. Website went down.
Indian men on a bus (Rohtak, India)[14]	Dec. 2014	Three men allegedly harassed two sisters on a bus.	The sisters hit the men with belts. A cell phone video spread. The men were arrested. The sisters were accused of misrepresenting the incident.

Racists Getting Fired[15]	Dec. 2014	The RGF website found racist comments on social media and recruited people to pressure employers to fire the commenters. The owner of the website was tricked by a fruadulent account and then counter attacked by 4chan.	RGF owner was forced out by threats. Site continues to operate with a different owner.
Daryl Sharma[16]	Oct. 2014	Sharma groped a woman on the street in Seattle	Police ignored the woman's report, but she tweeted a photo of the offender (Sharma). Sharma's probation officer saw the photo and arrested him.
Armed clown pranksters (France)[17]	Oct. 2014	Teens dressed as clowns harassed people around France.	Anti-clown vigilantes organized using social media (Facebook).
CEO of Centerplate Inc.[18]	Aug. 2014	Security guard recorded the CEO kicking a dog and posted the video online.	CEO forced to resign and donate $100k to animal non-profit.
Big Earl's Bait House and Country Store, Pittsburg, TX[19]	May 2014	Waitress kicked out gay patrons.	Attacked on Yelp/Google reviews. Website went down.
Comics for Kids, Tishomingo, MS[20]	May 2014	Caught by Twitter users giving donations to a family member.	Non-profit was shut down.
NYPD[21]	April 2014		Twitter outreach was co-opted to post photos of NYPD beating and arresting protestors.
Mozilla Foundation[22]	March 2014	CEO Brendan Eich's donation to support Prop. 8 was uncovered.	A "Twitter Storm" formed, and Eich resigned.
Andrea Cardosa[23]	Feb. 2014	A vice principal sexually abused two under-age students.	One student posted an accusation video on YouTube. The teacher was charged with 16 felonies.
Mid-Michigan Teen Parties, Flint, MI[24]	Jan. 2014	Announced "Freedom 2 Twerk" party with an image of Martin Luther King, Jr. showing a gang sign on the poster.	Image spread on social media and King's family criticized the event. The venue management received many complaints and cancelled the event.
Shugaland, Glen Burnie, MD[25]	Jan. 2014	Party-hosting business kicked children out. Mom posted on YouTube.	Mall management shut down Shugaland.
Ani DiFranco[26]	Dec. 2013	The musician planned an event at a former plantation.	Many protesters wrote on the event's Facebook page to criticise the venue. The event was cancelled.
Justine Sacco[27]	Dec. 2013	Tweeted joke about race and AIDS.	Became #1 trending topic on Twitter, and Sacco was fired the same day.

Murdoch's Range & Home Supply, Evansville, WY[28]	Dec. 2013	Two employees killed a raccoon, which was reported on Facebook.	The two employees resigned.
Specialized Bicycle[29]	Dec. 2013	Specialized accused a small bike shop of trademark infringement.	After social media criticism, Specialized dropped the suit, and the CEO personally apologized to the shop owner.
Duck Dynasty (TV show)[30]	Dec. 2013	GQ magazine published an interview in which Robertson (actor on the show) expressed anti-gay opinions.	The interview spread on social media. A&E suspended Robertson. *Duck Dynasty* merchandise was removed from stores.
Devin Barnes + Red Lobster, Franklin, TN[31]	October 2013	Waitress claimed Barnes wrote a slur on the receipt.	Barnes received death threats. Waitress received $10,000 in donations. Barnes sued Red Lobster and waitress.
FedEx[32]	July 2013	Video of delivery driver throwing packages spread on the web.	Driver was fired.
Golden Corral, Port Orange, FL[33]	July 2013	Pictures purported to be of a dirty kitchen at GC were posted on Reddit.	GC manager was fired for improper food handling.
Taco Bell[34]	June 2013	Picture of employee licking taco shells (for an internal photo contest) spread on the net.	The employee was fired.
Sunil Tripathi + Reddit[35]	April 2013	Reddit users crowdsourcing the search for the Boston Marathon bombers mistakenly accused several people.	The accusations were spread by mainstream media. Several innocent people were threatened and harassed.
Adria Richards[36]	March 2013	Richards posted a photo of a man making sexual jokes at a programming conference in order to shame him.	The man was fired. Hackers attacked Richards and her employer. Richards was fired.
SeaWorld[37]	2013-2015	Accused of mistreating whales in a documentary and on social media.	Attendance and stock price fell. CEO resigned. Layoffs.
Thai Noodles Etc., Austin, TX[38]	Dec. 2012	Restaurant owner posted racist message on Facebook about the Sandy Hook shooting.	Attacked on Yelp. Owner received death threats. Restaurant deleted Facebook page.
Lindsey Stone[39]	Oct. 2012	Stone appeared in an image giving the middle finger in jest at the Arlington National Cemetery.	The image spread on social media. A Facebook page appeared with the goal of having Stone fired. She was fired.

George Zimmerman[40]	2012	Zimmerman fatally shot Trayvon Martin but was not charged.	Social media (incl. change.org petition) and protests were used to pressure officials to file charges. Zimmerman was charged 6 weeks after the shooting.
Beef Products Inc.[41]	2012	ABC reported on a beef food product treated with ammonia (pink slime).	Public pressure (incl. a change.org petition) convinced many buyers to abdandon BPI products. BPI closed 3 of 4 factories.
Steubenville, Ohio rape case[42]	2012–2013	Digital evidence of a rape circulated on social media.	Hackers revealed the identities and video footage of the alleged rapists. Two students were convicted of rape. Several school officials were charged. A hacker was indicted on federal charges.
Domino's Pizza, Conover, NC[43]	April 2009	YouTube videos showed employees doing disgusting things to pizza ingredients.	The employees were fired and arrested.
Choi Jin-sil (South Korean actress)[44]	October 2008	Rumors spread on the web that Choi, a superstar South Korean actress, was a loan shark who contributed to the suicide of a debtor.	Choi committed suicide. The government introduced legislation to decrease anonymity online and facilitate punishment of libel. Suicide rates allegedly increased by 70% during the month after Choi's suicide ("copy cats").
Patrick Pogan (NYPD)[45]	July 2008	Officer Pogan pushed a bicyclist off his bike during a Critical Mass event. Pogan reported that the cyclist ran into him.	Video of the incident spread on YouTube. Pogan was convicted of filing false documents.
Rocky Mountain Chocolate Factory, Huntington Beach, CA[46]	June 2008	Store refused to allow 5-yo girl to use the bathroom, who defecated in her clothes.	The store franchisee's address was posted on the web, and she received death threats. The CEO apologized to the mother.
"Dog Poop Girl" (Seoul, South Korea)[47]	June 2005	A girl allowed her dog to defecate in the Seoul subway.	Web users ("netizens") publicly identified her and her family. The girl was harassed and left her university.
Human flesh search engine[48]	2001–		Chinese web users have revealed and shamed numerous people for corruption, abuse, adultery, and other transgressions.

Notes

[1]Silverstein (2015)

[2]Pearlman (2015)

[3]Associated Press (2015)

[4]Woolston (2015)

[5]Ward and Gutowski (2015)

[6]Markiewicz (2015)

[7]Wagner (2015)

[8]Zoladz (2015)

[9]CNN (2015)

[10]Firstpost.com (2015)

[11]TMZ.com (2015)

[12]Moran (2015)

[13]Spargo (2015)

[14]Arya (2015)

[15]McDonald (2014)

[16]News (2014)

[17]Goldhammer (2014)

[18]Garrett (2014)

[19]Shah (2014)

[20]Johnston (2014)

[21]McKechnie (2014)

[22]Kim (2014)

[23]Goff and Shin (2015)

[24]DeJohn (2014)

[25]Scharper (2014)

[26]Anderson (2013)

[27]Ronson (2015)

[28]Associated Press (2013)

[29]Babin (2013)

[30]Ford (2013)

[31]Wagner (2014)

[32]Jacobson (2013)

[33]Ward (2013)

[34]Sun (2013)

[35]Levenson (2015)

[36]Martin (2013)

[37]Lomax (2015)

[38]Kuruvilla (2012)

[39]Sieczkowski (2012)

[40]Selter (2012)

[41]Blaney (2012)

[42]Kushner (2013)

[43]Hutchinson (2009)

[44]Sang-Hun (2008)

[45]Yaniv (2010)

[46]Burris (2008)

[47]Krim (2005)

[48]Hatton (2014)

1.7.2. Description and images of experimental news site

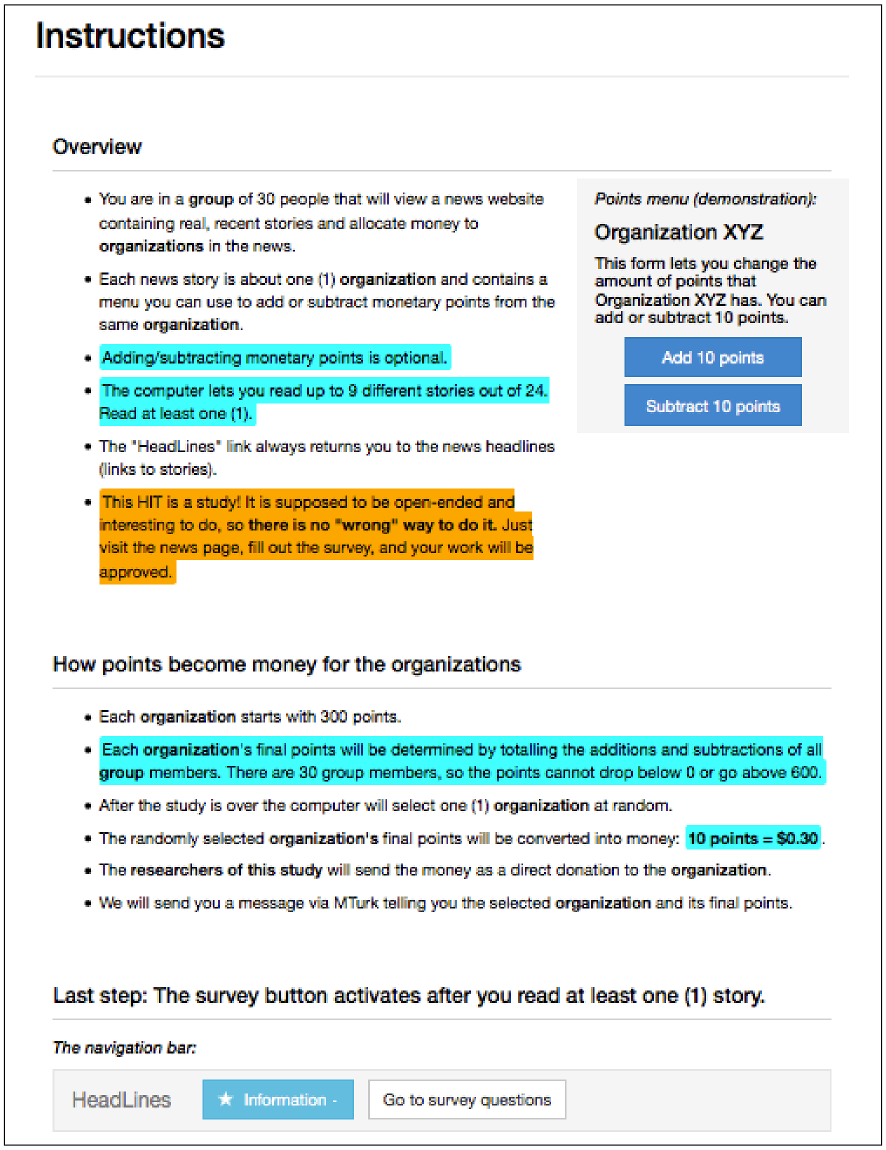


FIGURE 1.10: Instructions to the participants (social and independent).

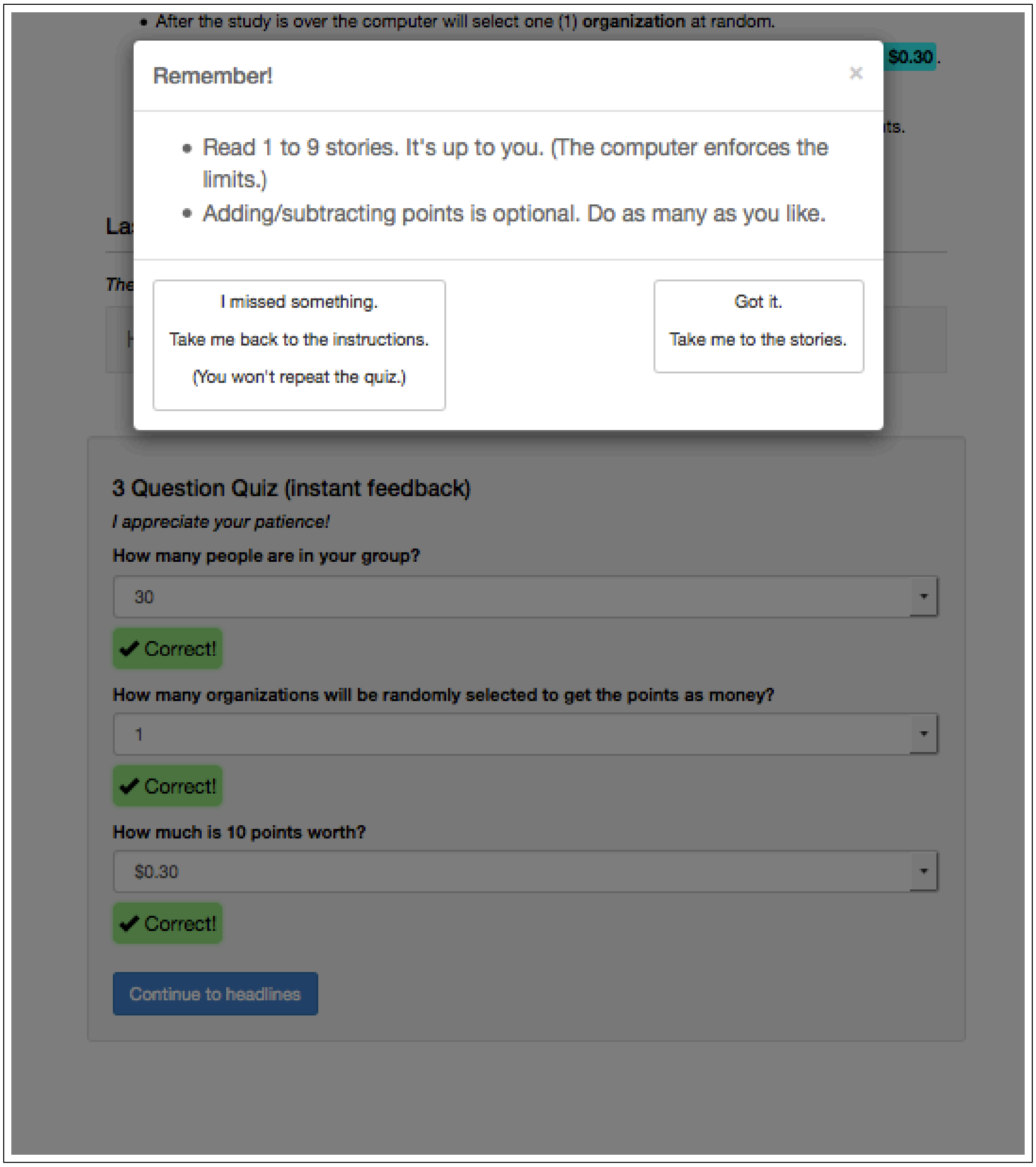


FIGURE 1.11: Reminder to participants after quiz.

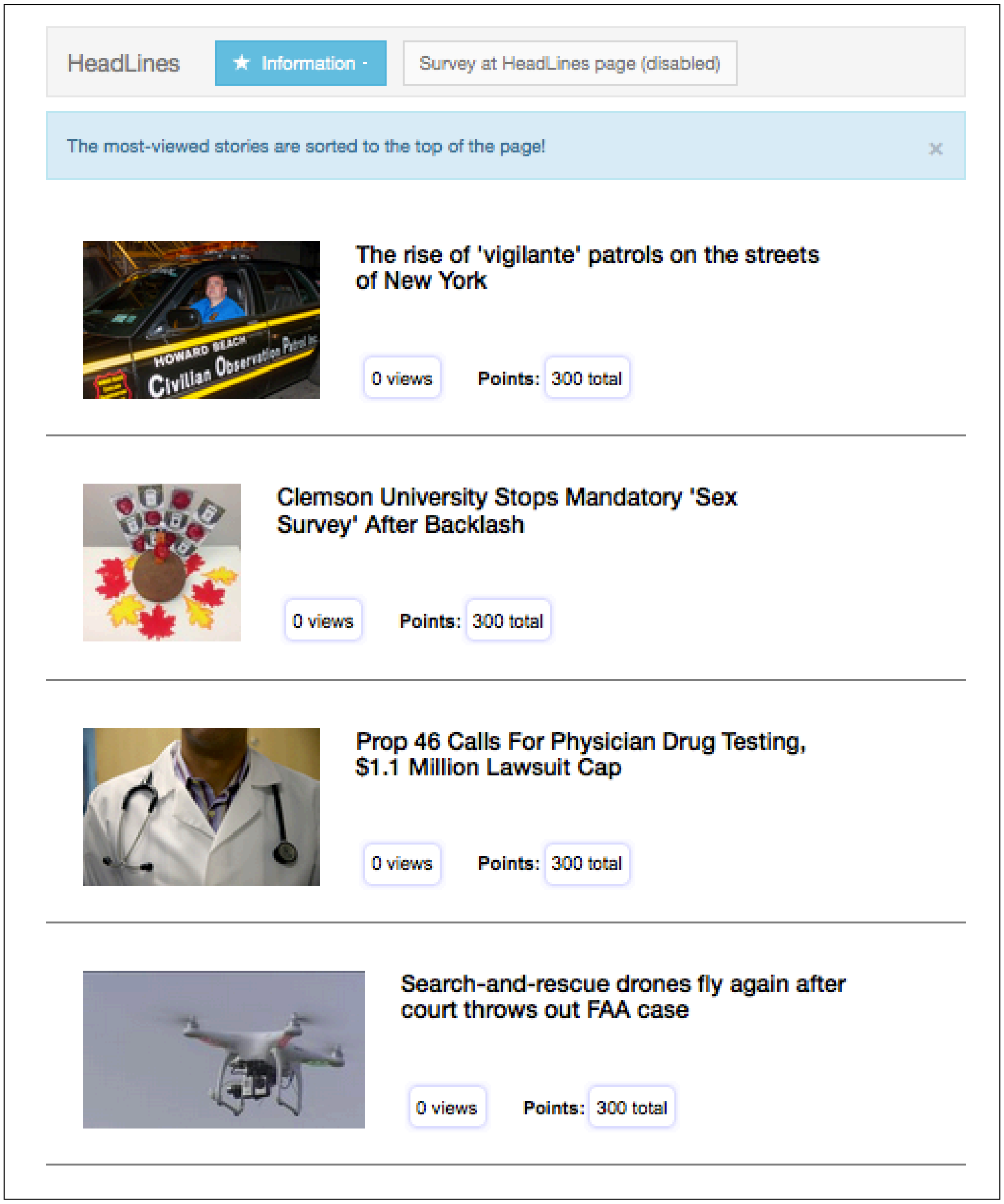
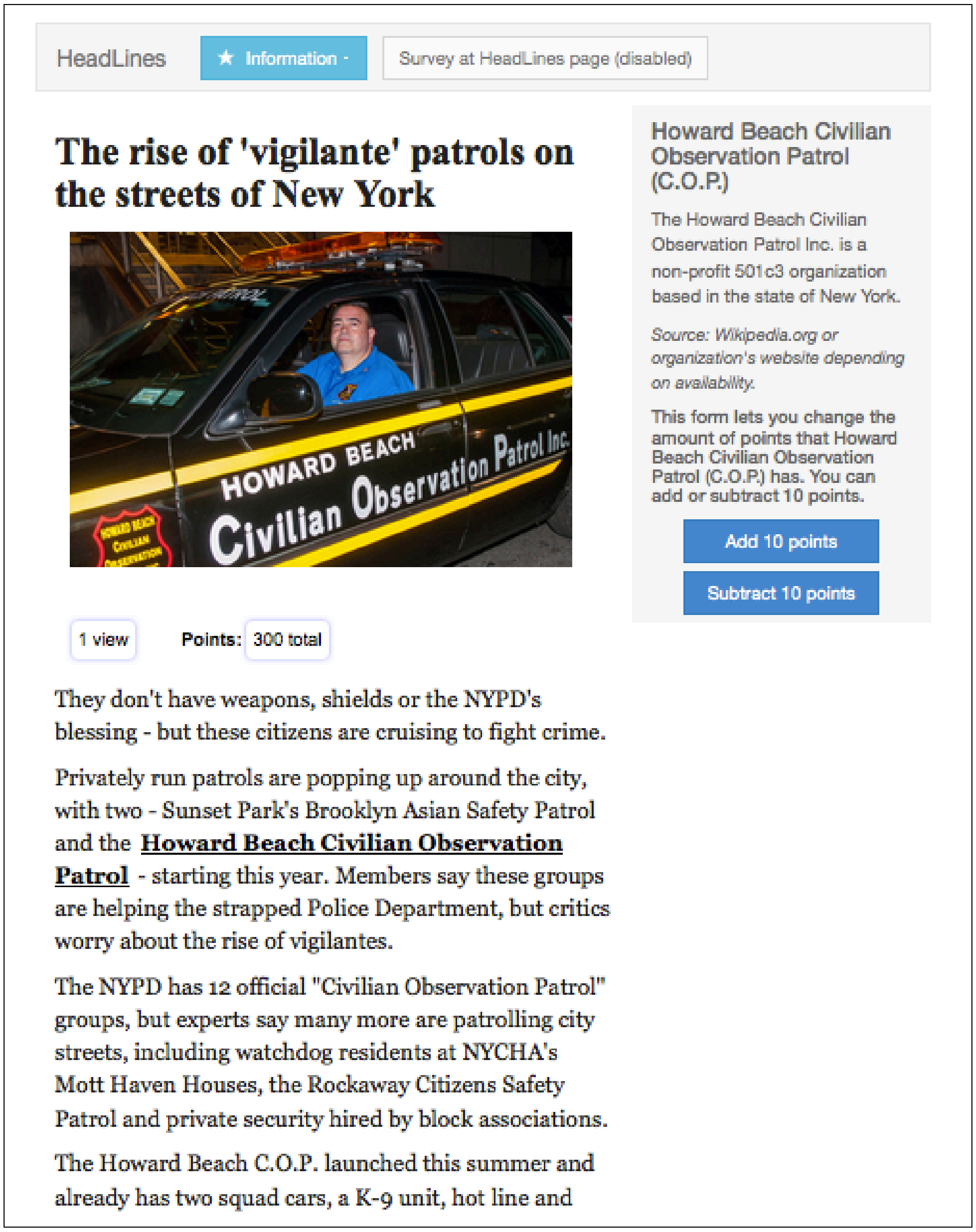


FIGURE 1.12: Headlines page (social condition).



HeadLines ★ Information · Survey at HeadLines page (disabled)

The rise of 'vigilante' patrols on the streets of New York

1 view **Points:** 300 total

They don't have weapons, shields or the NYPD's blessing - but these citizens are cruising to fight crime.

Privately run patrols are popping up around the city, with two - Sunset Park's Brooklyn Asian Safety Patrol and the **Howard Beach Civilian Observation Patrol** - starting this year. Members say these groups are helping the strapped Police Department, but critics worry about the rise of vigilantes.

The NYPD has 12 official "Civilian Observation Patrol" groups, but experts say many more are patrolling city streets, including watchdog residents at NYCHA's Mott Haven Houses, the Rockaway Citizens Safety Patrol and private security hired by block associations.

The Howard Beach C.O.P. launched this summer and already has two squad cars, a K-9 unit, hot line and

Howard Beach Civilian Observation Patrol (C.O.P.)

The Howard Beach Civilian Observation Patrol Inc. is a non-profit 501c3 organization based in the state of New York.

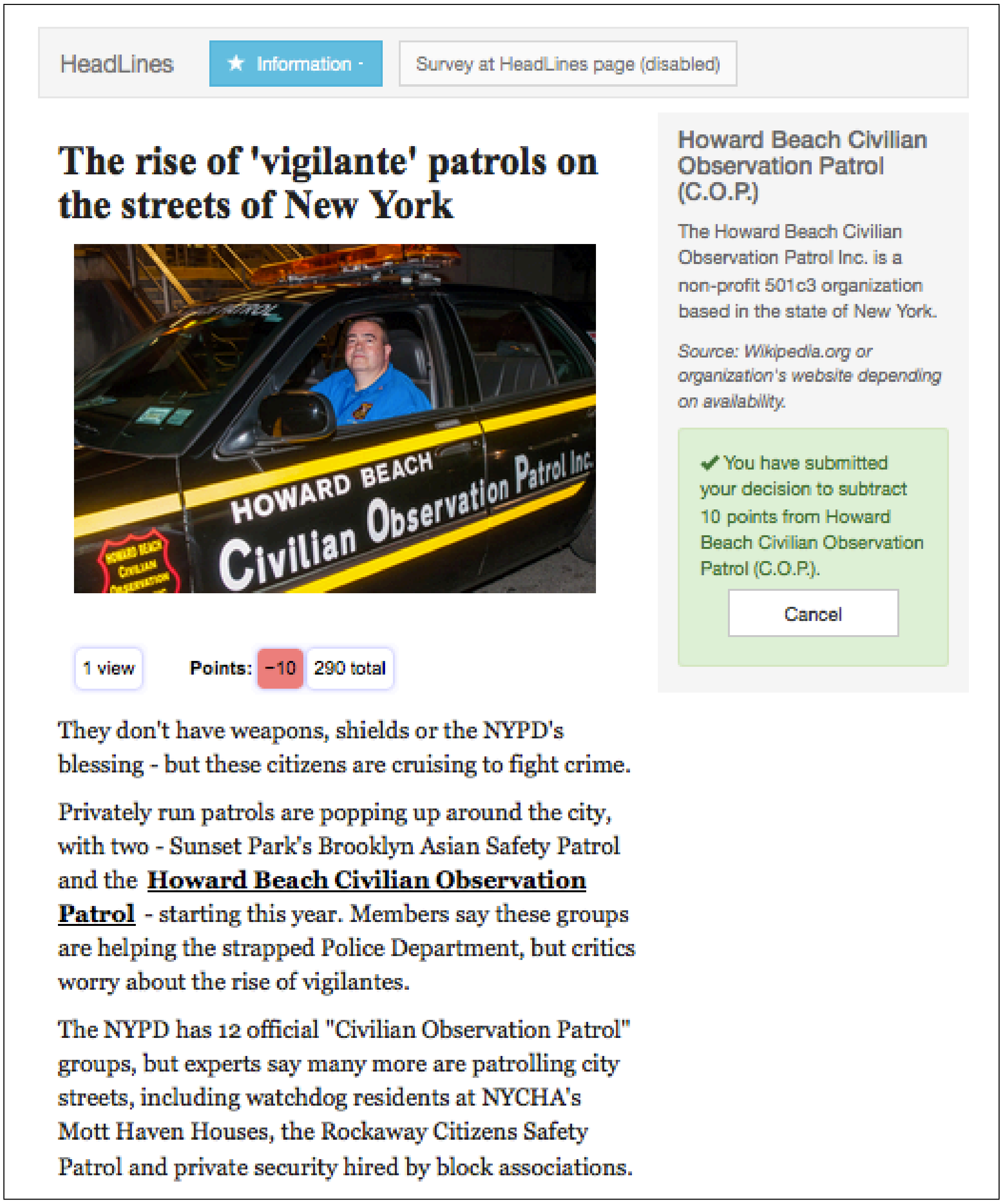
Source: Wikipedia.org or organization's website depending on availability.

This form lets you change the amount of points that Howard Beach Civilian Observation Patrol (C.O.P.) has. You can add or subtract 10 points.

Add 10 points

Subtract 10 points

FIGURE 1.13: One of the news stories.



HeadLines | ★ Information - | Survey at HeadLines page (disabled)

The rise of 'vigilante' patrols on the streets of New York

1 view   **Points:** −10   290 total

They don't have weapons, shields or the NYPD's blessing - but these citizens are cruising to fight crime.

Privately run patrols are popping up around the city, with two - Sunset Park's Brooklyn Asian Safety Patrol and the **Howard Beach Civilian Observation Patrol** - starting this year. Members say these groups are helping the strapped Police Department, but critics worry about the rise of vigilantes.

The NYPD has 12 official "Civilian Observation Patrol" groups, but experts say many more are patrolling city streets, including watchdog residents at NYCHA's Mott Haven Houses, the Rockaway Citizens Safety Patrol and private security hired by block associations.

Howard Beach Civilian Observation Patrol (C.O.P.)

The Howard Beach Civilian Observation Patrol Inc. is a non-profit 501c3 organization based in the state of New York.

Source: Wikipedia.org or organization's website depending on availability.

✔ You have submitted your decision to subtract 10 points from Howard Beach Civilian Observation Patrol (C.O.P.).

Cancel

FIGURE 1.14: One of the news stories (after choosing to punish).

TABLE 1.11: Stories and organizations used in the experiment

Headline: *Organization*:	Shooting Clinic for the Blind Northeast Passage
Headline: *Organization*:	Artist Collective Sues Non-Profit Over "Booklyn" Copyright Booklyn Artists Alliance
Headline: *Organization*:	Clemson University Stops Mandatory 'Sex Survey' After Backlash Clemson University
Headline: *Organization*:	CU-Boulder scientists speak out on research misconduct claim CU-Boulder Department of Chemistry
Headline: *Organization*:	U. of I. doctors under scrutiny for surgical robot ad University of Illinois Hospital & Health Sciences System
Headline: *Organization*:	Federal agency defends use of Ducks Unlimited Ducks Unlimited
Headline: *Organization*:	Aiming for a bear: 7-year-old Alto boy goes on hunt of a lifetime Hunt of a Lifetime
Headline: *Organization*:	First LSD Study in 40 Years Finds Therapeutic Potential Multidisciplinary Association for Psychedelic Studies (MAPS)
Headline: *Organization*:	Ripple Creator Donates $500k in XRP to Artificial Intelligence Research Charity Machine Intelligence Research Institute (MIRI)
Headline: *Organization*:	Videos of rat abuse at Petaluma High School prompt outcry Petaluma Wildlife Museum
Headline: *Organization*:	How to save Dartmoor's hill ponies? Eat them, says animal conservation group Friends of the Dartmoor Hill Pony (Dartmoor Hill Pony Association)
Headline: *Organization*:	Prop 46 Calls For Physician Drug Testing, $1.1 Million Lawsuit Cap Consumer Watchdog
Headline: *Organization*:	So You Want to Live Forever Strategies for Engineered Negligible Senescence (SENS) Research Foundation
Headline: *Organization*:	Should Humanity Try to Contact Intelligent Aliens? SETI Institute
Headline: *Organization*:	Should we make animals smarter? Institute for Ethics and Emerging Technologies(IEET)
Headline: *Organization*:	A 'Suicide Club' Has Been Launched In Britain By Australia's Dr. Death, To Help People Kill Themselves Exit International
Headline: *Organization*:	Search-and-rescue drones fly again after court throws out FAA case Texas EquuSearch
Headline: *Organization*:	What is Tor? A beginner's guide to the privacy tool The Tor Project
Headline: *Organization*:	A manifesto for playing god with human evolution Humanity+
Headline: *Organization*:	Your brain's electrical activity can predict whether you go for risky sex, UCLA study finds Semel Institute for Neuroscience and Human Behavior at the University of California, Los Angeles
Headline: *Organization*:	UNESCO to host meeting on controversial 'memory of water' research United Nations Educational, Scientific and Cultural Organization (UNESCO)
Headline: *Organization*:	Nonprofit Group Gives Hundreds Of Veterans Free Marijuana Operation Grow4Vets
Headline: *Organization*:	The rise of 'vigilante' patrols on the streets of New York Howard Beach Civilian Observation Patrol (C.O.P.)
Headline: *Organization*:	How Web archivists and other digital sleuths are unraveling the mystery of MH17 Wayback Machine

1.7.3. Simulation study of the re-sampling estimator

The simulation creates identical, independent agents that make decisions over 24 stories. Each agent punishes each story with probability $\frac{1}{48}$ and rewards each story with probability $\frac{2}{48}$, which are similar to the observed probabilities. This parametrization gives simulated means and standard deviations that are close to the actual data. The simulation creates 1,000 data sets composed of n worlds for each $n = 2, 4, 8, 16, 32, 64$. Each world is composed of 30 such agents as in the experiment. The net punishment in each world is calculated as described in the methods section. The conventional estimators are the sample mean and sample standard deviation. The re-sampling estimator is computed as described in the paper. In this case, each simulated data set is sub-sampled 400 times to create 400 worlds. The mean and standard deviation of the sub-sampled worlds are the estimates. The simulation results show that the re-sampling standard deviation estimator has much less variance (smaller standard error) than the conventional estimator. Fairly precise estimates are available even for 2 worlds, where the conventional estimator is highly erratic. The bias of the re-sampling estimator is similar to that of the conventional estimator. The re-sampling estimator of the mean has somewhat less variance than the sample mean, but the advantage is much less than in the case of estimating the standard deviation. The re-sampling estimator of the mean has a small downward bias that decreases with the sample size. The sample mean is, of course, unbiased.

Figure 1.16 shows box and whiskers plots of the results from the 1,000 simulated data sets in each box. The box shows the interquartile range with a line at the median. The whiskers extend 1.5 $\times$ (the interquartile range) beyond the box edges. The plus signs are outliers.

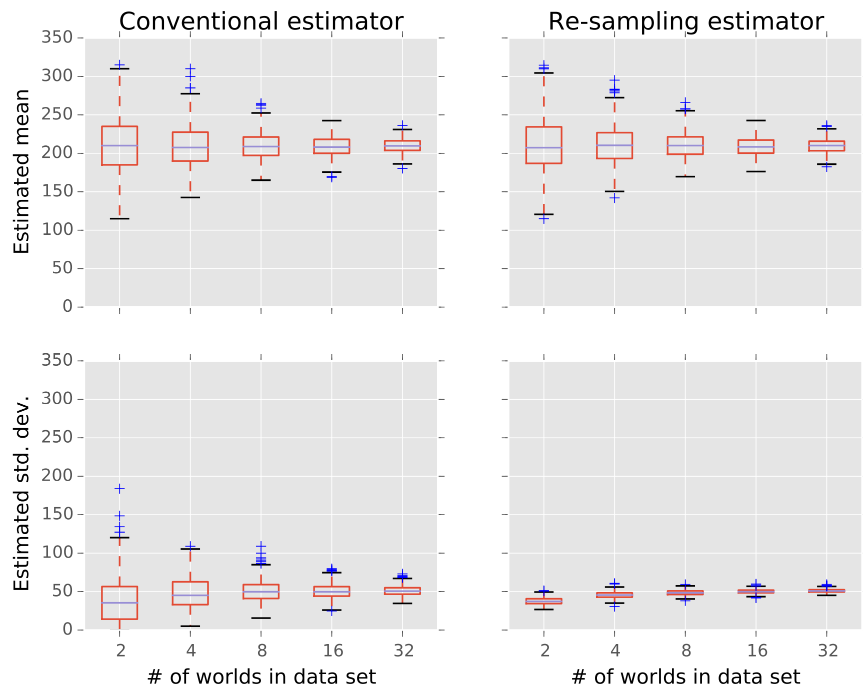


FIGURE 1.15: Net reward: Performance of conventional *vs.* re-sampling estimator.

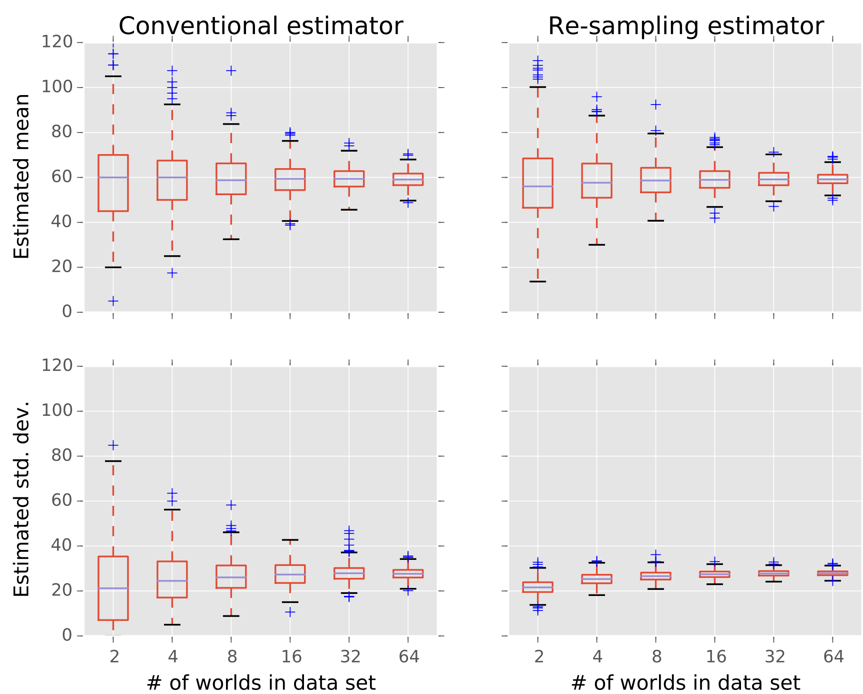


FIGURE 1.16: Net punishment: Performance of conventional *vs.* re-sampling estimator.

1.7.4. Additional results

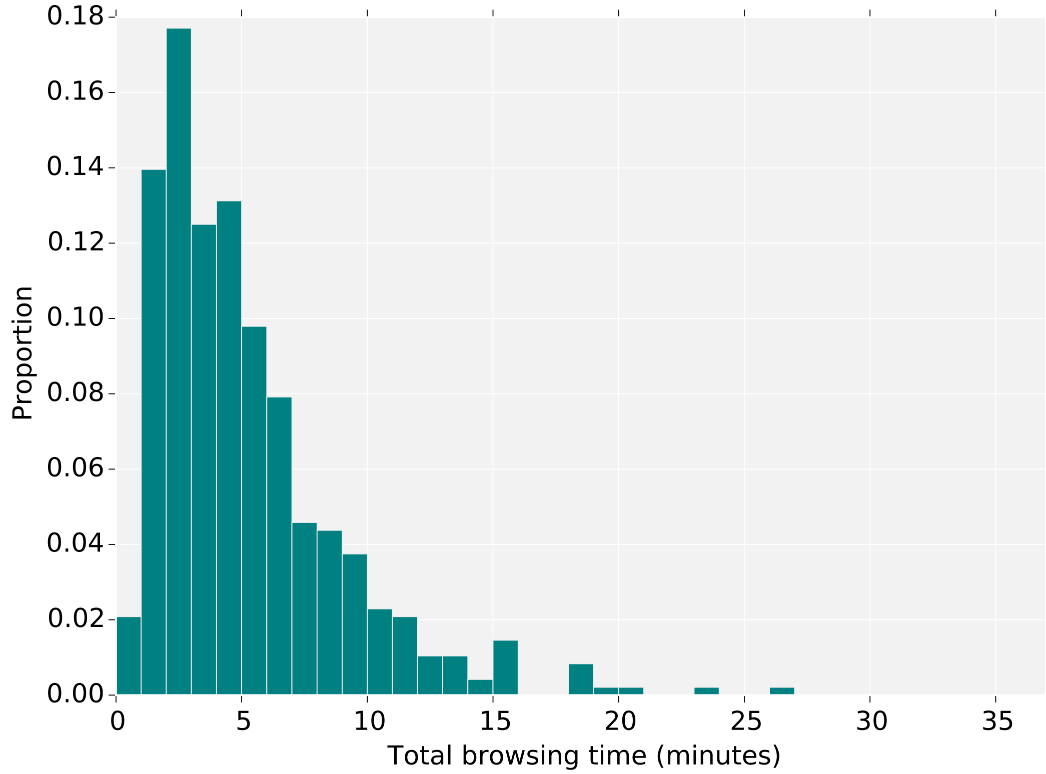


FIGURE 1.17: Histogram of time spent by participants on the news site

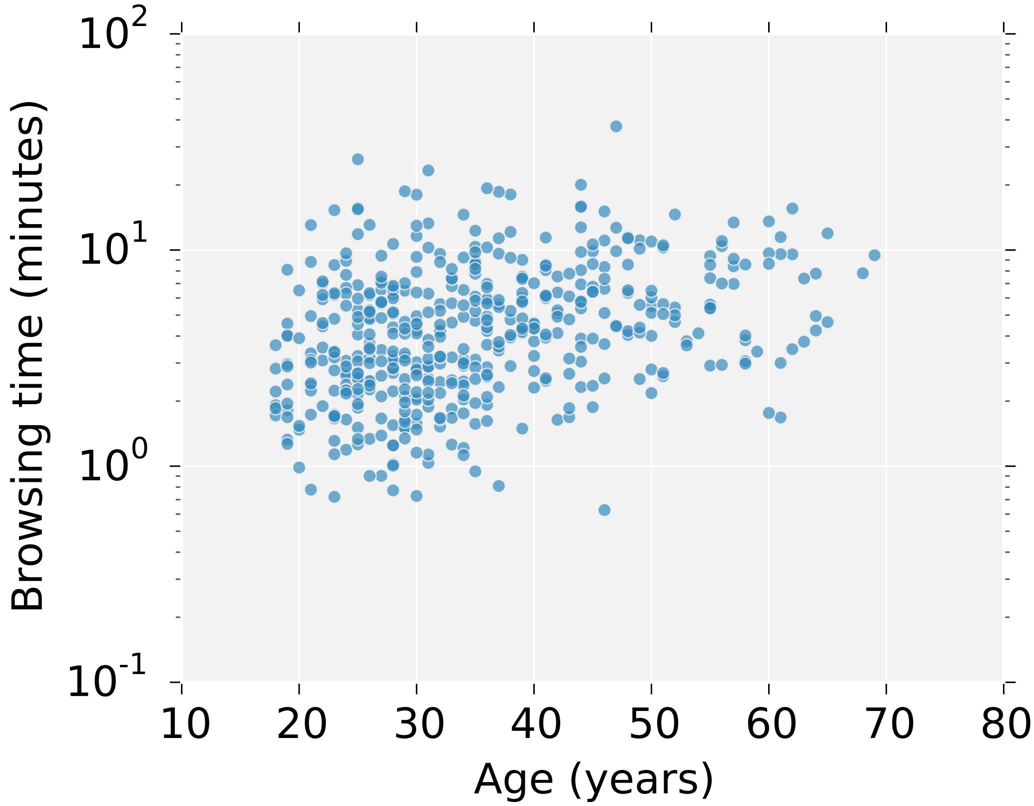


FIGURE 1.18: Participant age vs. browsing time

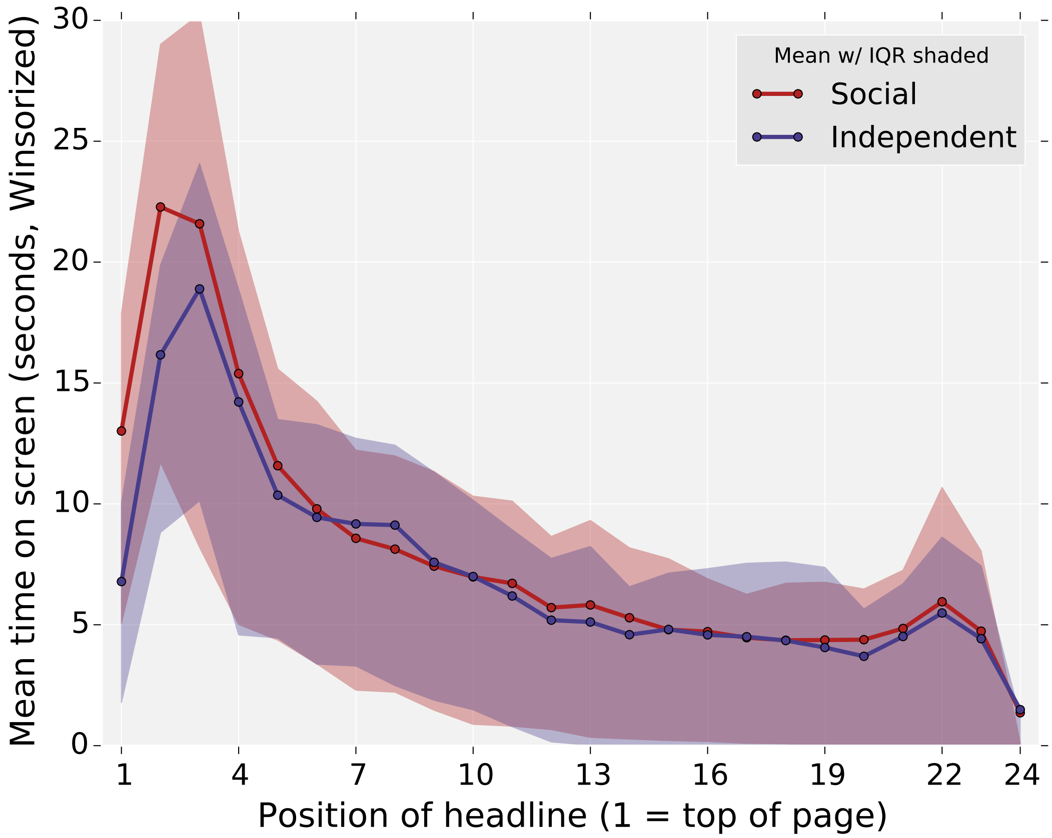


FIGURE 1.19: Headline items' time on screen by visual position and condition

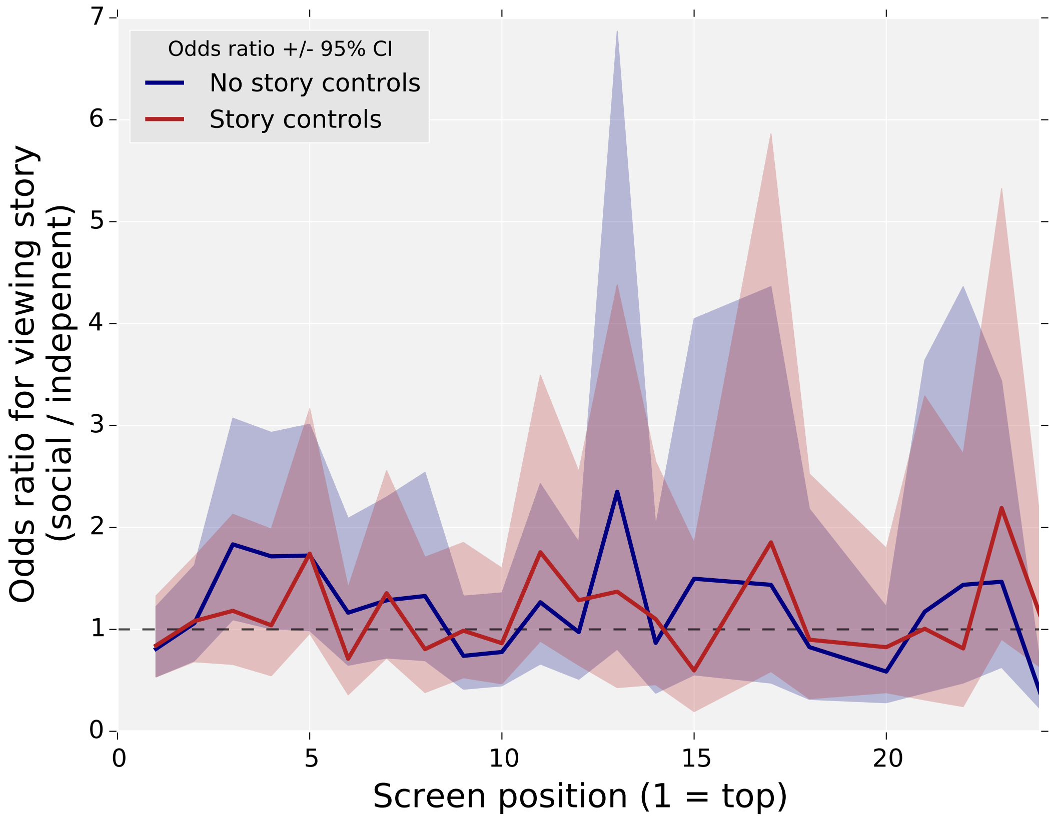


FIGURE 1.20: Social:independent odds ratios for viewing stories

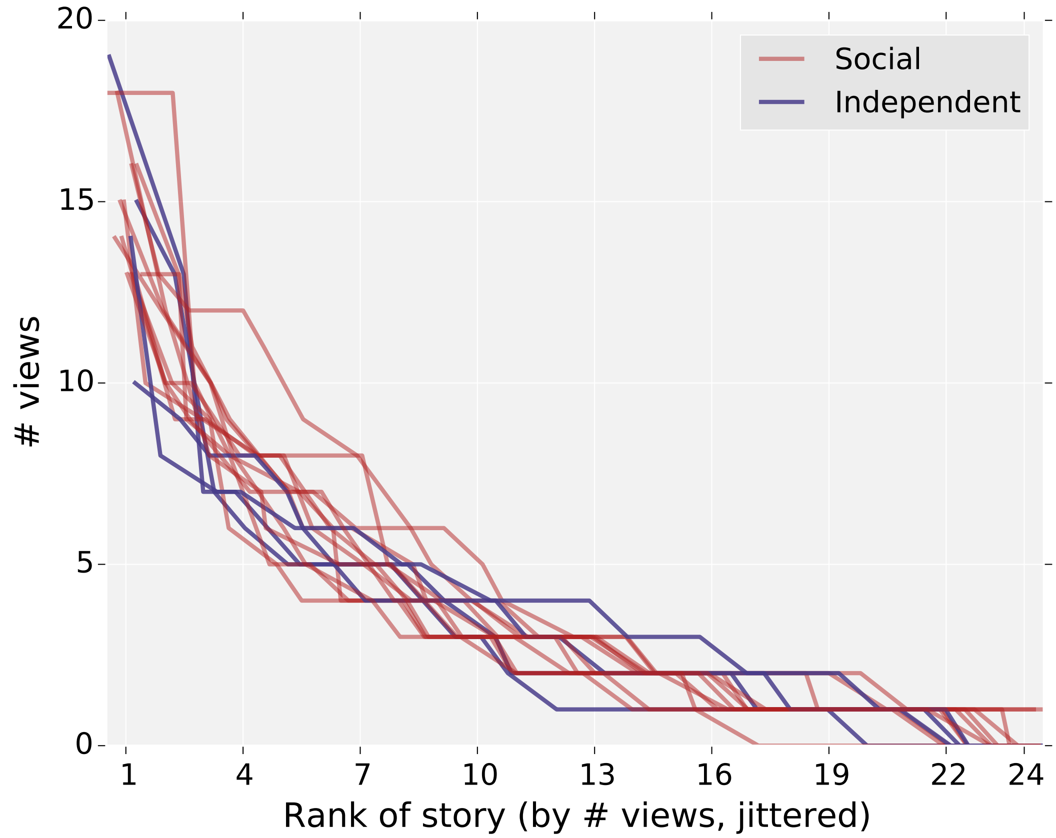


FIGURE 1.21: Distribution of story views for each world

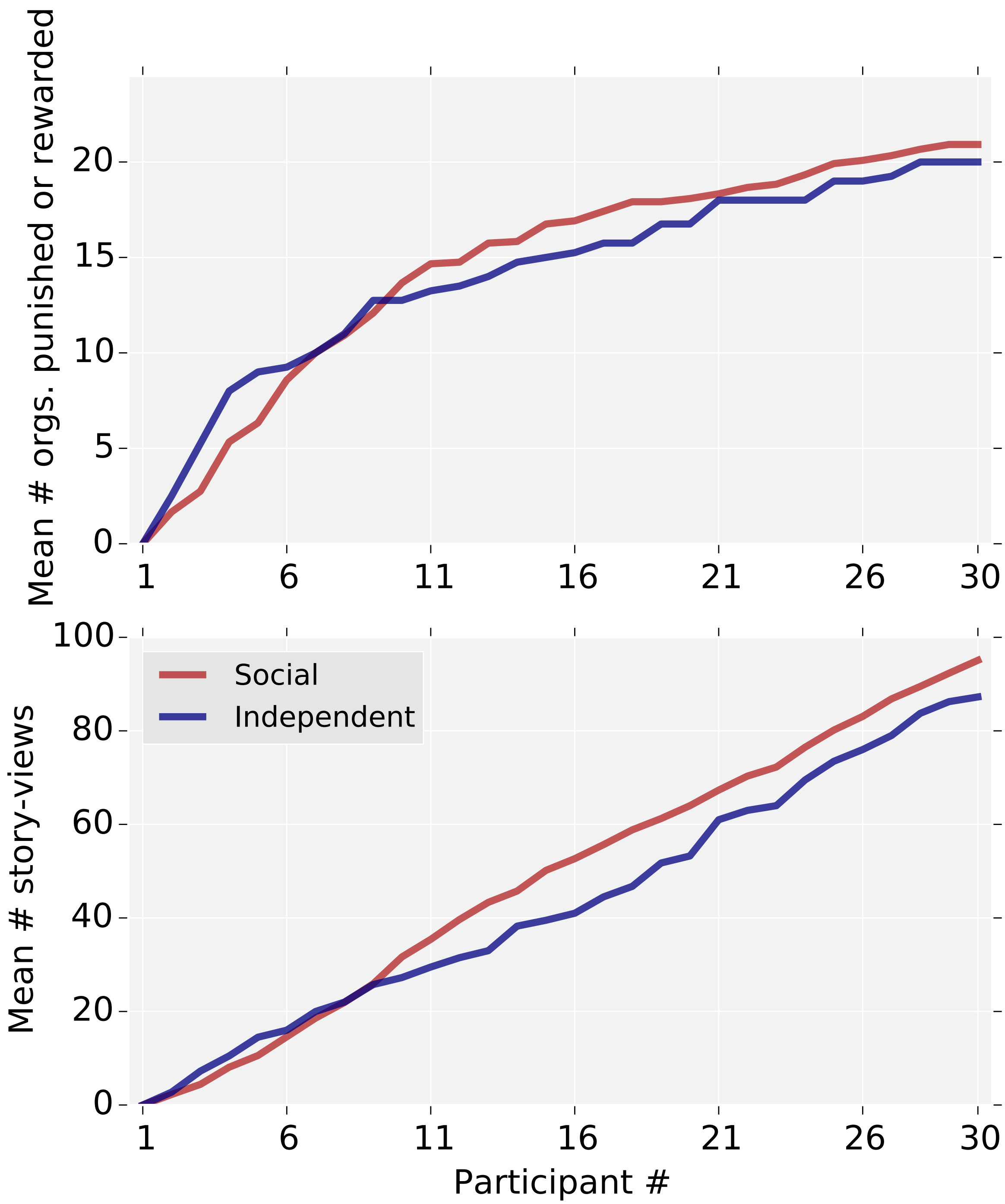


FIGURE 1.22: Evolution of points actions and views

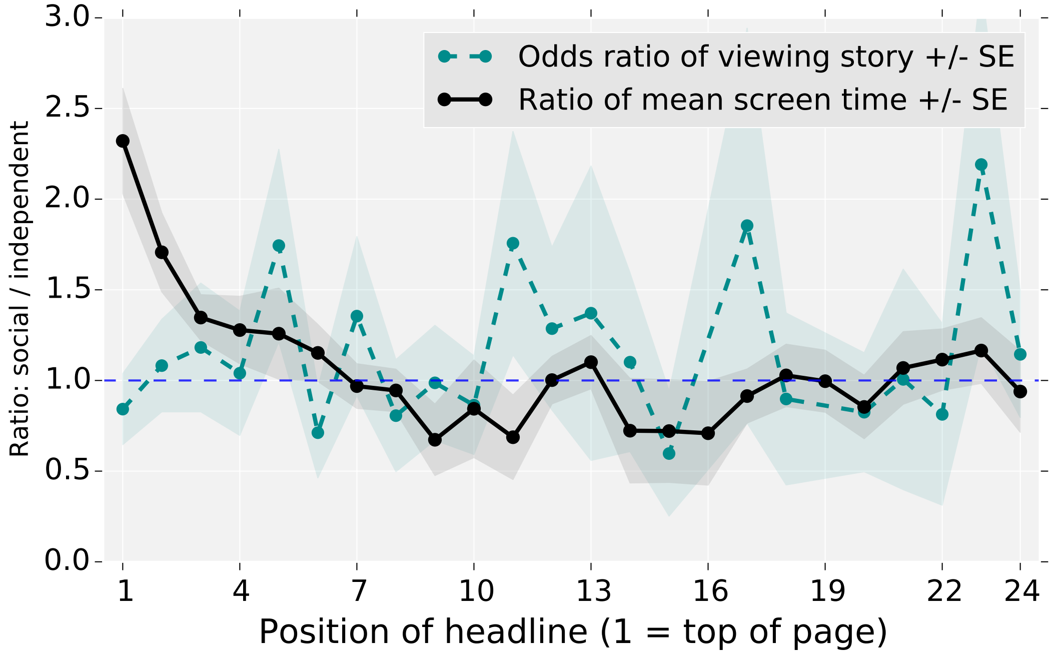


FIGURE 1.23: Headline items' time on screen and click probability by page position

TABLE 1.12: Alternative specifications of social influence on probability of punishing

	Not controlling for page position		Includes quadratic in page position		Includes page position dummy variables	
	Social effect: **Linear**	Social effect: **Quadratic**	Social effect: **Linear**	Social effect: **Quadratic**	Social effect: **Linear**	Social effect: **Quadratic**
Social effect:						
Points sub'ed	0.055^{***}	0.158^{***}	0.029^{**}	0.106^{***}	0.031^{**}	0.113^{***}
	(0.014)	(0.033)	(0.015)	(0.038)	(0.015)	(0.039)
Points added	0.086^{***}	0.149^{***}	0.021	0.054	0.018	0.049
	(0.023)	(0.046)	(0.029)	(0.055)	(0.029)	(0.056)
Individuals	360	360	360	360	360	360
Obs.	8,640	8,640	8,640	8,640	8,640	8,640
Avg. log-likelihood	-0.145	-0.144	-0.143	-0.142	-0.141	-0.140

Notes. Average marginal effects of previous punishment (points sub'ed) and reward (points added) printed. Effects are specified in the share form. Estimates come from a logit model with a dummy variable for each story. Standard errors, in parentheses, are clustered by participant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

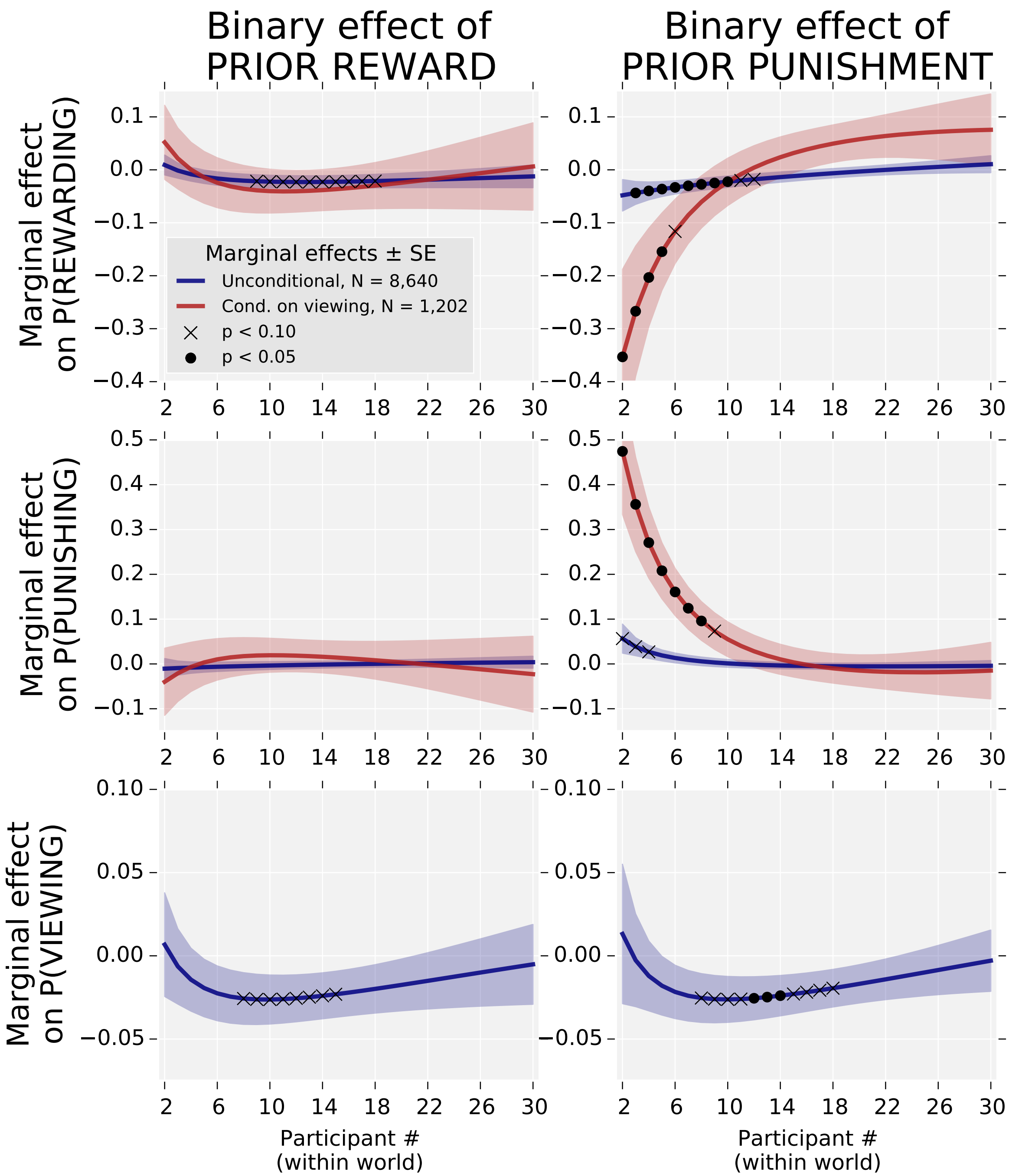


FIGURE 1.24: Binary effect of previous punishment and reward on individual behavior

TABLE 1.13: Coefficient estimates of models for affecting net outcomes (panel data)

	Left-hand side variable (binary): The participant acted on story k such that the net outcome was affected	
	Net punishment	Net reward
Social condition (dummy)	1.278^{*}	-0.700
	(0.776)	(1.468)
State of the news page when visited by the participant:		
Number of organizations w/ points ≤ 300	0.003	
	(0.049)	
Social dummy $\times$ (# orgs. w/ points ≤ 300)	-0.066	
	(0.051)	
Number of organizations w/ points ≥ 300		-0.053
		(0.066)
Social dummy $\times$ (# orgs. w/ points ≥ 300)		0.038
		(0.070)
Average marginal effect of social cond.	0.013	0.006
Significance of AME of social cond. (p)	$\mathbf{0.090}$	0.511
Joint sig. of 3 coefs. above (p)	$\mathbf{0.001}$	0.651
Observations	$6,497$	$9,860$
Individuals	480	480

Notes. Table displays coefficient estimates from pooled logit models and participant-by-story panel data. The data used for the "punish" model includes only observations where the organization had wealth $\leq$ 30. The data used for the "reward" model includes only observations where the organization had wealth $\geq$ 30. Models include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.14: Effects of social condition on individual behavior and net outcomes

	ΔNet punishment		ΔNet reward		Views (exploration)	
	Effect	SE	Effect	SE	Effect	SE
Difficulty						
0	-0.0132	(0.0222)	0.0069	(0.0219)	-0.0085	(0.0229)
1	-0.0072	(0.0171)	0.0073	(0.0132)	-0.0475^{*}	(0.0283)
2	-0.0028	(0.0139)	0.0075	(0.0097)	-0.0717^{*}	(0.0384)
3	0.0009	(0.0117)	0.0076	(0.0094)	-0.0752^{*}	(0.0417)
4	0.0041	(0.0101)	0.0077	(0.0107)	-0.0611	(0.0378)
5	0.0069	(0.0089)	0.0078	(0.0126)	-0.0363	(0.0310)
6	0.0094	(0.0081)	0.0079	(0.0146)	-0.0079	(0.0255)
7	0.0117	(0.0076)	0.0080	(0.0165)	0.0190	(0.0227)
8	0.0139^{*}	(0.0075)	0.0081	(0.0183)	0.0414^{*}	(0.0217)
9	0.0159^{**}	(0.0075)	0.0081	(0.0199)	0.0579^{***}	(0.0213)
10	0.0178^{**}	(0.0078)	0.0082	(0.0215)	0.0681^{***}	(0.0210)
11	0.0196^{**}	(0.0082)	0.0082	(0.0230)	0.0719^{***}	(0.0206)
12	0.0212^{**}	(0.0087)	0.0083	(0.0243)	0.0695^{***}	(0.0201)
13	0.0228^{**}	(0.0093)	0.0083	(0.0256)	0.0614^{***}	(0.0199)
14	0.0244^{**}	(0.0099)	0.0083	(0.0268)	0.0484^{**}	(0.0207)
15	0.0258^{**}	(0.0105)	0.0084	(0.0280)	0.0319	(0.0229)
16	0.0272^{**}	(0.0111)	0.0084	(0.0291)	0.0141	(0.0261)
17	0.0286^{**}	(0.0117)	0.0085	(0.0301)	-0.0013	(0.0286)
18	0.0299^{**}	(0.0124)	0.0085	(0.0311)	-0.0089	(0.0301)
19	0.0311^{**}	(0.0130)	0.0085	(0.0321)	-0.0028	(0.0357)
20	0.0324^{**}	(0.0136)	0.0085	(0.0330)	0.0208	(0.0491)
21	0.0335^{**}	(0.0142)	0.0086	(0.0339)	0.0610	(0.0601)
22	0.0347^{**}	(0.0148)	0.0086	(0.0347)	0.1122^{*}	(0.0629)
23	0.0358^{**}	(0.0154)	0.0086	(0.0355)	0.1728^{**}	(0.0873)
24	0.0369^{**}	(0.0160)	0.0086	(0.0363)	0.2569	(0.1633)
Individuals	480		480		473	
Obs.	$6,497$		$9,860$		$4,614$	
Avg. log-like.	-0.171		-0.284		-0.269	
BIC	2458.15		5864.37		2555.97	

Notes. Table displays estimated marginal effects of the social condition on the probability of changing net punishment, changing net reward, or viewing a story, respectively. Marginal effects are based on pooled logit models and participant-by-story panel data. The data used for estimation contains only the observations such that the organization had wealth≤ 30, wealth $\geq$30, and wealth$= 30$, respectively. *Difficulty* means the number of observations for a given participant that were dropped due to the aforementioned conditioning. Models include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.15: Effects of social condition on net punishment (conditional on points ≤ 30)

	Punishment		Reward		Either	
	Effect	SE	Effect	SE	Effect	SE
Difficulty						
0	-0.0081	(0.0240)	-0.3571	(0.3356)	-0.0132	(0.0222)
1	-0.0022	(0.0169)	-0.2310	(0.1931)	-0.0072	(0.0171)
2	0.0015	(0.0131)	-0.1679	(0.1229)	-0.0028	(0.0139)
3	0.0042	(0.0106)	-0.1282	(0.0827)	0.0009	(0.0117)
4	0.0063	(0.0089)	-0.1002^{*}	(0.0577)	0.0041	(0.0101)
5	0.0081	(0.0078)	-0.0789^{*}	(0.0414)	0.0069	(0.0089)
6	0.0097	(0.0070)	-0.0620^{**}	(0.0307)	0.0094	(0.0081)
7	0.0110^{*}	(0.0065)	-0.0479^{**}	(0.0239)	0.0117	(0.0076)
8	0.0122^{*}	(0.0063)	-0.0360^{*}	(0.0201)	0.0139^{*}	(0.0075)
9	0.0134^{**}	(0.0063)	-0.0256	(0.0185)	0.0159^{**}	(0.0075)
10	0.0144^{**}	(0.0064)	-0.0165	(0.0183)	0.0178^{**}	(0.0078)
11	0.0153^{**}	(0.0066)	-0.0083	(0.0189)	0.0196^{**}	(0.0082)
12	0.0162^{**}	(0.0069)	-0.0009	(0.0199)	0.0212^{**}	(0.0087)
13	0.0170^{**}	(0.0072)	0.0059	(0.0211)	0.0228^{**}	(0.0093)
14	0.0177^{**}	(0.0076)	0.0122	(0.0223)	0.0244^{**}	(0.0099)
15	0.0185^{**}	(0.0079)	0.0180	(0.0234)	0.0258^{**}	(0.0105)
16	0.0191^{**}	(0.0083)	0.0234	(0.0246)	0.0272^{**}	(0.0111)
17	0.0198^{**}	(0.0087)	0.0285	(0.0257)	0.0286^{**}	(0.0117)
18	0.0204^{**}	(0.0090)	0.0333	(0.0268)	0.0299^{**}	(0.0124)
19	0.0210^{**}	(0.0094)	0.0379	(0.0279)	0.0311^{**}	(0.0130)
20	0.0216^{**}	(0.0098)	0.0422	(0.0290)	0.0324^{**}	(0.0136)
21	0.0221^{**}	(0.0101)	0.0464	(0.0300)	0.0335^{**}	(0.0142)
22	0.0226^{**}	(0.0105)	0.0503	(0.0311)	0.0347^{**}	(0.0148)
23	0.0231^{**}	(0.0108)	0.0541^{*}	(0.0322)	0.0358^{**}	(0.0154)
24	0.0236^{**}	(0.0111)	0.0578^{*}	(0.0332)	0.0369^{**}	(0.0160)
Individuals	480		411		480	
Obs.	$6,497$		$1,417$		$6,497$	
Avg. log-like.	-0.141		-0.214		-0.171	
BIC	1887.10		651.91		2275.11	

Notes. Table displays estimated marginal effects of the social condition on the probability of changing net punishment by punishing, rewarding, or either action, respectively. Marginal effects are based on pooled logit models and participant-by-story panel data. The data used for estimation contains only the observations such that the organization had wealth≤ 30, wealth < 30, and wealth ≤ 30, respectively. *Difficulty* means the number of observations for a given participant that were dropped due to the aforementioned conditioning. Models include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.16: Effects of social condition on net reward (conditional on points ≥ 30)

	Punishment		Reward		Either	
	Effect	SE	Effect	SE	Effect	SE
Difficulty						
0	0.0321^{**}	(0.0154)	-0.0036	(0.0214)	0.0069	(0.0219)
1	0.0196^{*}	(0.0109)	0.0026	(0.0119)	0.0073	(0.0132)
2	0.0100	(0.0087)	0.0061	(0.0086)	0.0075	(0.0097)
3	0.0017	(0.0080)	0.0085	(0.0083)	0.0076	(0.0094)
4	-0.0056	(0.0090)	0.0104	(0.0094)	0.0077	(0.0107)
5	-0.0123	(0.0113)	0.0119	(0.0109)	0.0078	(0.0126)
6	-0.0185	(0.0142)	0.0131	(0.0123)	0.0079	(0.0146)
7	-0.0242	(0.0174)	0.0142	(0.0137)	0.0080	(0.0165)
8	-0.0297	(0.0207)	0.0152	(0.0150)	0.0081	(0.0183)
9	-0.0348	(0.0241)	0.0160	(0.0161)	0.0081	(0.0199)
10	-0.0397	(0.0275)	0.0168	(0.0171)	0.0082	(0.0215)
11	-0.0444	(0.0308)	0.0174	(0.0181)	0.0082	(0.0230)
12	-0.0489	(0.0342)	0.0181	(0.0190)	0.0083	(0.0243)
13	-0.0532	(0.0375)	0.0187	(0.0198)	0.0083	(0.0256)
14	-0.0573	(0.0407)	0.0192	(0.0206)	0.0083	(0.0268)
15	-0.0613	(0.0439)	0.0197	(0.0213)	0.0084	(0.0280)
16	-0.0652	(0.0471)	0.0202	(0.0220)	0.0084	(0.0291)
17	-0.0690	(0.0502)	0.0206	(0.0226)	0.0085	(0.0301)
18	-0.0726	(0.0532)	0.0210	(0.0232)	0.0085	(0.0311)
19	-0.0761	(0.0562)	0.0214	(0.0237)	0.0085	(0.0321)
20	-0.0796	(0.0592)	0.0218	(0.0243)	0.0085	(0.0330)
21	-0.0829	(0.0621)	0.0221	(0.0248)	0.0086	(0.0339)
22	-0.0862	(0.0649)	0.0225	(0.0253)	0.0086	(0.0347)
23	-0.0894	(0.0677)	0.0228	(0.0257)	0.0086	(0.0355)
24	-0.0925	(0.0704)	0.0231	(0.0262)	0.0086	(0.0363)
Individuals	459		480		480	
Obs.	$4,482$		$9,860$		$9,860$	
Avg. log-like.	-0.147		-0.255		-0.284	
BIC	1369.73		5087.06		5655.68	

Notes. Table displays estimated marginal effects of the social condition on the probability of changing net reward by punishing, rewarding, or either action, respectively. Marginal effects are based on pooled logit models and participant-by-story panel data. The data used for estimation contains only the observations such that the organization had wealth$>$ 30, wealth $\geq$30, and wealth$\geq$ 30, respectively. *Difficulty* means the number of observations for a given participant that were dropped due to the aforementioned conditioning. Models include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.17: Individual characteristics as predictors of behavior in the experiment (clustering standard errors by world)

			Binary measures of behavior		
	Total stories viewed	Log total browsing time	Read > 1 story	Ever punished	Ever rewarded
College graduate	0.121^{*}	0.083	0.113^{***}	0.093^{*}	0.005
	(0.071)	(0.065)	(0.030)	(0.047)	(0.031)
Male	-0.025	-0.217^{**}	0.022	-0.117^{**}	0.002
	(0.082)	(0.078)	(0.046)	(0.049)	(0.035)
Has engaged in protest activity	0.134	-0.056	0.145^{**}	0.122^{**}	0.057
	(0.089)	(0.090)	(0.053)	(0.056)	(0.033)
Age	0.003	0.018^{***}	0.001	0.000	-0.000
	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
Has low wealth	0.090	0.083	0.031	-0.009	0.032
	(0.074)	(0.056)	(0.044)	(0.056)	(0.032)
Uses Facebook	-0.003	-0.017	-0.101^{*}	0.076	0.008
	(0.104)	(0.105)	(0.056)	(0.057)	(0.049)
Uses Twitter	0.035	-0.064	0.050	-0.010	0.113^{***}
	(0.053)	(0.054)	(0.041)	(0.040)	(0.028)
Owns smartphone	-0.014	-0.078	0.049	0.064	0.010
	(0.085)	(0.068)	(0.073)	(0.059)	(0.066)
Social treatment	0.121^{***}	0.175^{***}	0.113^{**}	0.075^{*}	0.061^{*}
	(0.043)	(0.058)	(0.042)	(0.041)	(0.035)
Obs.	477	477	477	477	477

Notes. The first two columns show semi-elasticities. The remaining columns show coefficient estimates (average marginal effects) from linear probability models. Standard errors, in parentheses, are clustered by world. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.18: Average marginal effects of added and subtracted points (clustering by user vs. world)

	Punish		Reward		View	
	User	World	User	World	User	World
Social effect:						
Points sub'ed (share)	0.0268^{***}	0.0268^{***}	0.0201^{**}	0.0201	0.0493^{***}	0.0493^{***}
	(0.0083)	(0.0083)	(0.0083)	(0.0131)	(0.0117)	(0.0170)
Points added (share)	0.0157^{***}	0.0157^{***}	0.0336^{***}	0.0336^{***}	0.0594^{***}	0.0594^{***}
	(0.0055)	(0.0039)	(0.0091)	(0.0104)	(0.0110)	(0.0123)
N	8640	8640	8640	8640	8640	8640

Notes. Average marginal effects of previous punishment (points sub'ed) and reward (points added) printed. Effects are specified in binary form with logarithmic decay. Estimates come from a logit model with a dummy variable for each story but *no controls for page position*. Standard errors, in parentheses, are clustered by the variable in the table heading. Standard errors, in parentheses, are clustered by participant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.19: Average marginal effects of added and subtracted points (controlling for page position, clustering by user vs. world)

	Punish		Reward		View	
	User	World	User	World	User	World
Social effect:						
Points sub'ed (share)	0.0062	0.0062	-0.0161^{**}	-0.0161^{*}	-0.0165	-0.0165^{*}
	(0.0083)	(0.0080)	(0.0077)	(0.0094)	(0.0108)	(0.0099)
Points added (share)	-0.0024	-0.0024	-0.0194^{*}	-0.0194^{*}	-0.0204	-0.0204
	(0.0068)	(0.0072)	(0.0112)	(0.0117)	(0.0129)	(0.0135)
N	8640	8640	8640	8640	8640	8640

Notes. Average marginal effects of previous punishment (points sub'ed) and reward (points added) printed. Effects are specified in binary form with logarithmic decay. Estimates come from a logit model with a dummy variable for each story controls for page position. Standard errors, in parentheses, are clustered by the variable in the table heading. Standard errors, in parentheses, are clustered by participant. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

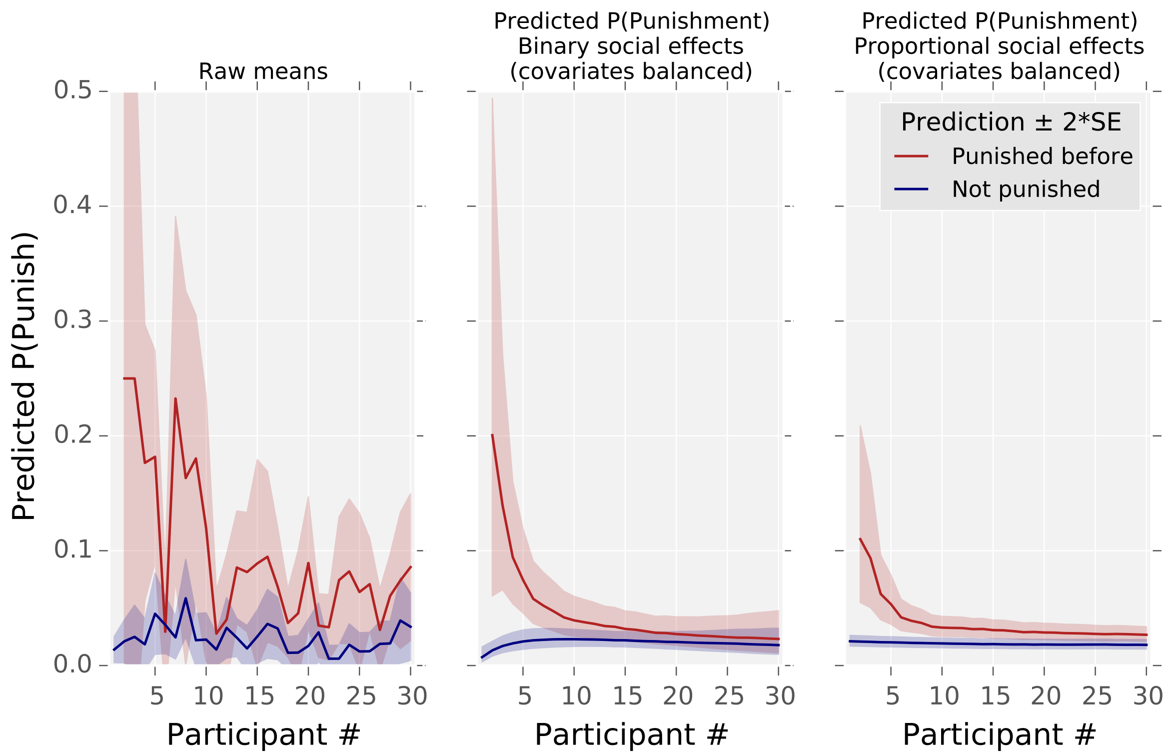


FIGURE 1.25: Probability that participant i punishes story k over the course of a social world

TABLE 1.20: Linear effects of prior punishment/reward (cond. on viewing)

	Punishment		Reward	
	Effect	SE	Effect	SE
Effect of prior punishment				
Participant #				
1	0.2779***	(0.0341)	−0.2767***	(0.0875)
2	0.2307***	(0.0367)	−0.2081***	(0.0727)
3	0.1869***	(0.0358)	−0.1574***	(0.0601)
4	0.1490***	(0.0325)	−0.1190**	(0.0493)
5	0.1173***	(0.0283)	−0.0892**	(0.0406)
6	0.0911***	(0.0244)	−0.0658*	(0.0338)
7	0.0695***	(0.0214)	−0.0472	(0.0287)
8	0.0517***	(0.0192)	−0.0323	(0.0250)
9	0.0372**	(0.0177)	−0.0204	(0.0224)
10	0.0254	(0.0166)	−0.0109	(0.0204)
11	0.0160	(0.0156)	−0.0035	(0.0190)
12	0.0087	(0.0148)	0.0023	(0.0178)
13	0.0031	(0.0140)	0.0067	(0.0168)
14	−0.0011	(0.0133)	0.0100	(0.0159)
15	−0.0042	(0.0126)	0.0123	(0.0149)
16	−0.0063	(0.0119)	0.0139	(0.0141)
17	−0.0075	(0.0113)	0.0148	(0.0132)
18	−0.0081	(0.0108)	0.0151	(0.0125)
19	−0.0081	(0.0104)	0.0149	(0.0118)
20	−0.0076	(0.0101)	0.0144	(0.0114)
21	−0.0067	(0.0099)	0.0135	(0.0111)
22	−0.0055	(0.0100)	0.0123	(0.0111)
23	−0.0039	(0.0102)	0.0109	(0.0114)
24	−0.0020	(0.0107)	0.0093	(0.0120)
25	0.0000	(0.0113)	0.0074	(0.0128)
26	0.0024	(0.0121)	0.0055	(0.0138)
27	0.0048	(0.0130)	0.0034	(0.0150)
28	0.0075	(0.0140)	0.0011	(0.0163)
29	0.0103	(0.0151)	−0.0012	(0.0178)
30	0.0132	(0.0162)	−0.0036	(0.0193)
Effect of prior reward				
Participant #				
1	−0.0180	(0.0552)	0.0242	(0.0658)
2	−0.0086	(0.0455)	0.0112	(0.0503)
3	−0.0015	(0.0375)	0.0027	(0.0392)
4	0.0037	(0.0312)	−0.0030	(0.0314)
5	0.0074	(0.0264)	−0.0069	(0.0259)
6	0.0100	(0.0228)	−0.0094	(0.0220)
7	0.0116	(0.0201)	−0.0111	(0.0192)
8	0.0125	(0.0181)	−0.0120	(0.0172)
9	0.0129	(0.0165)	−0.0124	(0.0158)
10	0.0128	(0.0152)	−0.0123	(0.0146)
11	0.0123	(0.0141)	−0.0119	(0.0137)
12	0.0116	(0.0131)	−0.0113	(0.0128)
13	0.0107	(0.0122)	−0.0105	(0.0120)
14	0.0097	(0.0115)	−0.0096	(0.0113)
15	0.0086	(0.0107)	−0.0085	(0.0107)
16	0.0074	(0.0101)	−0.0074	(0.0101)
17	0.0062	(0.0095)	−0.0062	(0.0096)
18	0.0049	(0.0090)	−0.0049	(0.0091)
19	0.0036	(0.0086)	−0.0037	(0.0088)
20	0.0023	(0.0084)	−0.0024	(0.0085)
21	0.0010	(0.0082)	−0.0011	(0.0084)
22	−0.0003	(0.0082)	0.0002	(0.0085)
23	−0.0017	(0.0084)	0.0015	(0.0087)
24	−0.0030	(0.0087)	0.0029	(0.0090)
25	−0.0044	(0.0091)	0.0042	(0.0094)
26	−0.0058	(0.0097)	0.0055	(0.0100)
27	−0.0071	(0.0103)	0.0068	(0.0106)
28	−0.0085	(0.0110)	0.0081	(0.0113)
29	−0.0098	(0.0117)	0.0093	(0.0120)
30	−0.0111	(0.0125)	0.0106	(0.0128)
Avg. log-like.	−0.50844		−0.59997	

Notes. Table displays estimated marginal effects. Models (1,202 obs., 480 individuals) include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.21: Binary effects of prior punishment/reward (cond. on viewing)

	Punishment		Reward	
	Effect	SE	Effect	SE
Effect of prior punishment				
Participant #				
1	0.6373***	(0.1695)	−0.4742**	(0.2150)
2	0.4744***	(0.1409)	−0.3532**	(0.1654)
3	0.3561***	(0.1056)	−0.2671**	(0.1232)
4	0.2707***	(0.0795)	−0.2033**	(0.0930)
5	0.2080***	(0.0627)	−0.1546**	(0.0731)
6	0.1607***	(0.0527)	−0.1163*	(0.0608)
7	0.1243***	(0.0469)	−0.0857	(0.0536)
8	0.0959**	(0.0433)	−0.0607	(0.0494)
9	0.0733*	(0.0409)	−0.0400	(0.0468)
10	0.0551	(0.0390)	−0.0227	(0.0448)
11	0.0403	(0.0374)	−0.0082	(0.0431)
12	0.0283	(0.0359)	0.0042	(0.0415)
13	0.0184	(0.0347)	0.0148	(0.0399)
14	0.0103	(0.0337)	0.0239	(0.0384)
15	0.0036	(0.0330)	0.0317	(0.0372)
16	−0.0019	(0.0326)	0.0385	(0.0362)
17	−0.0063	(0.0326)	0.0444	(0.0355)
18	−0.0099	(0.0330)	0.0495	(0.0354)
19	−0.0127	(0.0339)	0.0539	(0.0358)
20	−0.0149	(0.0352)	0.0577	(0.0368)
21	−0.0165	(0.0369)	0.0610	(0.0383)
22	−0.0177	(0.0390)	0.0639	(0.0404)
23	−0.0184	(0.0414)	0.0664	(0.0428)
24	−0.0187	(0.0440)	0.0685	(0.0457)
25	−0.0187	(0.0468)	0.0703	(0.0489)
26	−0.0185	(0.0498)	0.0718	(0.0524)
27	−0.0179	(0.0530)	0.0731	(0.0560)
28	−0.0171	(0.0563)	0.0741	(0.0598)
29	−0.0161	(0.0597)	0.0749	(0.0638)
30	−0.0149	(0.0632)	0.0756	(0.0678)
Effect of prior reward				
Participant #				
1	−0.0601	(0.0884)	0.0950	(0.0942)
2	−0.0399	(0.0750)	0.0519	(0.0701)
3	−0.0209	(0.0630)	0.0215	(0.0577)
4	−0.0066	(0.0551)	0.0004	(0.0518)
5	0.0034	(0.0499)	−0.0142	(0.0490)
6	0.0102	(0.0464)	−0.0244	(0.0472)
7	0.0147	(0.0437)	−0.0313	(0.0457)
8	0.0174	(0.0414)	−0.0359	(0.0442)
9	0.0188	(0.0395)	−0.0387	(0.0428)
10	0.0194	(0.0380)	−0.0402	(0.0414)
11	0.0192	(0.0368)	−0.0407	(0.0402)
12	0.0185	(0.0361)	−0.0405	(0.0394)
13	0.0174	(0.0358)	−0.0396	(0.0389)
14	0.0160	(0.0361)	−0.0383	(0.0389)
15	0.0143	(0.0369)	−0.0366	(0.0393)
16	0.0124	(0.0382)	−0.0345	(0.0403)
17	0.0104	(0.0400)	−0.0323	(0.0418)
18	0.0082	(0.0423)	−0.0298	(0.0436)
19	0.0059	(0.0448)	−0.0272	(0.0459)
20	0.0035	(0.0477)	−0.0244	(0.0485)
21	0.0010	(0.0509)	−0.0215	(0.0513)
22	−0.0015	(0.0542)	−0.0186	(0.0544)
23	−0.0040	(0.0577)	−0.0156	(0.0576)
24	−0.0067	(0.0613)	−0.0125	(0.0609)
25	−0.0093	(0.0651)	−0.0094	(0.0644)
26	−0.0120	(0.0689)	−0.0063	(0.0679)
27	−0.0147	(0.0728)	−0.0032	(0.0715)
28	−0.0174	(0.0768)	−0.0000	(0.0752)
29	−0.0201	(0.0808)	0.0032	(0.0788)
30	−0.0229	(0.0848)	0.0063	(0.0825)
Avg. log-like.	−0.50761		−0.59845	

Notes. Table displays estimated marginal effects. Models (1,202 obs., 480 individuals) include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.22: Log effects of prior punishment/reward on individual behavior (cond. on viewing)

	Punishment		Reward	
	Effect	SE	Effect	SE
Effect of prior punishment				
Participant #				
1	0.5885**	(0.2434)	−0.9459**	(0.4775)
2	0.4732***	(0.1300)	−0.6429**	(0.2906)
3	0.3864***	(0.1141)	−0.4712**	(0.1943)
4	0.3061***	(0.1036)	−0.3544**	(0.1477)
5	0.2349***	(0.0879)	−0.2629**	(0.1187)
6	0.1748**	(0.0720)	−0.1880**	(0.0956)
7	0.1257**	(0.0593)	−0.1269	(0.0772)
8	0.0863*	(0.0509)	−0.0775	(0.0640)
9	0.0549	(0.0461)	−0.0379	(0.0559)
10	0.0301	(0.0436)	−0.0066	(0.0514)
11	0.0106	(0.0423)	0.0180	(0.0491)
12	−0.0047	(0.0414)	0.0372	(0.0478)
13	−0.0165	(0.0404)	0.0519	(0.0466)
14	−0.0255	(0.0394)	0.0630	(0.0453)
15	−0.0322	(0.0382)	0.0711	(0.0437)
16	−0.0369	(0.0369)	0.0767*	(0.0419)
17	−0.0400	(0.0356)	0.0802**	(0.0400)
18	−0.0418	(0.0344)	0.0820**	(0.0383)
19	−0.0424	(0.0335)	0.0822**	(0.0368)
20	−0.0420	(0.0330)	0.0813**	(0.0359)
21	−0.0408	(0.0329)	0.0792**	(0.0357)
22	−0.0387	(0.0334)	0.0762**	(0.0364)
23	−0.0361	(0.0347)	0.0724*	(0.0381)
24	−0.0328	(0.0365)	0.0679*	(0.0408)
25	−0.0290	(0.0390)	0.0628	(0.0444)
26	−0.0248	(0.0420)	0.0571	(0.0487)
27	−0.0201	(0.0455)	0.0510	(0.0535)
28	−0.0151	(0.0495)	0.0444	(0.0589)
29	−0.0098	(0.0537)	0.0375	(0.0646)
30	−0.0042	(0.0582)	0.0303	(0.0707)
Effect of prior reward				
Participant #				
1	−0.0127	(0.1576)	−0.0641	(0.2411)
2	−0.0029	(0.1250)	−0.0592	(0.1574)
3	0.0030	(0.1014)	−0.0556	(0.1131)
4	0.0063	(0.0823)	−0.0523	(0.0868)
5	0.0076	(0.0677)	−0.0483	(0.0698)
6	0.0073	(0.0574)	−0.0436	(0.0587)
7	0.0061	(0.0506)	−0.0383	(0.0517)
8	0.0043	(0.0461)	−0.0329	(0.0474)
9	0.0020	(0.0431)	−0.0276	(0.0447)
10	−0.0006	(0.0409)	−0.0223	(0.0428)
11	−0.0033	(0.0392)	−0.0173	(0.0413)
12	−0.0061	(0.0377)	−0.0125	(0.0398)
13	−0.0090	(0.0363)	−0.0079	(0.0384)
14	−0.0119	(0.0351)	−0.0035	(0.0370)
15	−0.0149	(0.0339)	0.0007	(0.0356)
16	−0.0178	(0.0329)	0.0046	(0.0344)
17	−0.0208	(0.0322)	0.0085	(0.0333)
18	−0.0238	(0.0317)	0.0121	(0.0326)
19	−0.0267	(0.0315)	0.0157	(0.0322)
20	−0.0297	(0.0316)	0.0191	(0.0322)
21	−0.0326	(0.0322)	0.0223	(0.0328)
22	−0.0355	(0.0331)	0.0255	(0.0338)
23	−0.0383	(0.0344)	0.0286	(0.0353)
24	−0.0412	(0.0361)	0.0316	(0.0372)
25	−0.0440	(0.0381)	0.0345	(0.0394)
26	−0.0468	(0.0403)	0.0373	(0.0420)
27	−0.0496	(0.0428)	0.0400	(0.0449)
28	−0.0523	(0.0454)	0.0426	(0.0480)
29	−0.0551	(0.0482)	0.0452	(0.0512)
30	−0.0577	(0.0512)	0.0477	(0.0546)
Avg. log-like.	−0.51108		−0.59925	

Notes. Table displays estimated marginal effects. Models (1,202 obs., 480 individuals) include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.23: Linear effects of prior punishment/reward on individual behavior (unconditional)

	Punishment		Reward		Viewing	
	Effect	SE	Effect	SE	Effect	SE
Effect of prior punishment						
Participant #						
1	0.0470	(0.0305)	−0.0438	(0.0275)	−0.0023	(0.0354)
2	0.0331	(0.0202)	−0.0379*	(0.0201)	−0.0077	(0.0256)
3	0.0233	(0.0146)	−0.0325**	(0.0155)	−0.0108	(0.0193)
4	0.0158	(0.0109)	−0.0278**	(0.0124)	−0.0126	(0.0151)
5	0.0104	(0.0082)	−0.0238**	(0.0102)	−0.0135	(0.0122)
6	0.0065	(0.0063)	−0.0204**	(0.0086)	−0.0139	(0.0102)
7	0.0037	(0.0051)	−0.0175**	(0.0075)	−0.0139	(0.0089)
8	0.0016	(0.0043)	−0.0150**	(0.0066)	−0.0137*	(0.0080)
9	0.0002	(0.0038)	−0.0129**	(0.0060)	−0.0133*	(0.0073)
10	−0.0009	(0.0034)	−0.0110**	(0.0056)	−0.0128*	(0.0068)
11	−0.0017	(0.0032)	−0.0094*	(0.0052)	−0.0122*	(0.0064)
12	−0.0022	(0.0030)	−0.0079	(0.0050)	−0.0116*	(0.0061)
13	−0.0026	(0.0028)	−0.0067	(0.0047)	−0.0109*	(0.0058)
14	−0.0028	(0.0027)	−0.0056	(0.0045)	−0.0103*	(0.0055)
15	−0.0029	(0.0025)	−0.0046	(0.0043)	−0.0095*	(0.0053)
16	−0.0030	(0.0024)	−0.0038	(0.0040)	−0.0088*	(0.0050)
17	−0.0029	(0.0023)	−0.0031	(0.0038)	−0.0081*	(0.0047)
18	−0.0028	(0.0022)	−0.0024	(0.0036)	−0.0074	(0.0045)
19	−0.0027	(0.0022)	−0.0019	(0.0035)	−0.0067	(0.0043)
20	−0.0025	(0.0021)	−0.0014	(0.0033)	−0.0060	(0.0041)
21	−0.0022	(0.0021)	−0.0009	(0.0032)	−0.0053	(0.0040)
22	−0.0020	(0.0020)	−0.0006	(0.0031)	−0.0046	(0.0039)
23	−0.0017	(0.0020)	−0.0003	(0.0031)	−0.0039	(0.0038)
24	−0.0014	(0.0020)	0.0000	(0.0032)	−0.0033	(0.0038)
25	−0.0010	(0.0021)	0.0002	(0.0033)	−0.0026	(0.0039)
26	−0.0007	(0.0021)	0.0004	(0.0035)	−0.0020	(0.0040)
27	−0.0004	(0.0022)	0.0006	(0.0038)	−0.0014	(0.0042)
28	0.0000	(0.0023)	0.0007	(0.0040)	−0.0008	(0.0044)
29	0.0004	(0.0025)	0.0008	(0.0044)	−0.0002	(0.0046)
30	0.0008	(0.0026)	0.0009	(0.0047)	0.0004	(0.0049)
Effect of prior reward						
Participant #						
1	−0.0094	(0.0155)	−0.0063	(0.0105)	−0.0120	(0.0221)
2	−0.0070	(0.0123)	−0.0088	(0.0083)	−0.0145	(0.0170)
3	−0.0052	(0.0098)	−0.0102	(0.0069)	−0.0154	(0.0135)
4	−0.0038	(0.0078)	−0.0110*	(0.0060)	−0.0155	(0.0112)
5	−0.0027	(0.0064)	−0.0113**	(0.0054)	−0.0151	(0.0095)
6	−0.0019	(0.0053)	−0.0113**	(0.0050)	−0.0145*	(0.0083)
7	−0.0013	(0.0046)	−0.0111**	(0.0047)	−0.0137*	(0.0075)
8	−0.0008	(0.0041)	−0.0108**	(0.0045)	−0.0129*	(0.0069)
9	−0.0005	(0.0037)	−0.0104**	(0.0043)	−0.0120*	(0.0064)
10	−0.0002	(0.0034)	−0.0099**	(0.0042)	−0.0111*	(0.0060)
11	−0.0000	(0.0032)	−0.0094**	(0.0040)	−0.0102*	(0.0057)
12	0.0001	(0.0030)	−0.0088**	(0.0039)	−0.0092*	(0.0054)
13	0.0003	(0.0028)	−0.0082**	(0.0038)	−0.0083	(0.0051)
14	0.0004	(0.0026)	−0.0076**	(0.0036)	−0.0075	(0.0048)
15	0.0004	(0.0025)	−0.0070**	(0.0035)	−0.0066	(0.0046)
16	0.0005	(0.0023)	−0.0064*	(0.0034)	−0.0058	(0.0044)
17	0.0005	(0.0022)	−0.0058*	(0.0032)	−0.0049	(0.0042)
18	0.0006	(0.0021)	−0.0051*	(0.0031)	−0.0042	(0.0039)
19	0.0006	(0.0020)	−0.0045	(0.0030)	−0.0034	(0.0038)
20	0.0006	(0.0019)	−0.0039	(0.0029)	−0.0026	(0.0036)
21	0.0006	(0.0018)	−0.0033	(0.0028)	−0.0019	(0.0034)
22	0.0006	(0.0018)	−0.0027	(0.0027)	−0.0012	(0.0033)
23	0.0006	(0.0018)	−0.0021	(0.0026)	−0.0006	(0.0032)
24	0.0005	(0.0018)	−0.0016	(0.0025)	0.0001	(0.0032)
25	0.0005	(0.0018)	−0.0010	(0.0025)	0.0007	(0.0032)
26	0.0005	(0.0018)	−0.0005	(0.0025)	0.0013	(0.0032)
27	0.0004	(0.0019)	0.0001	(0.0025)	0.0019	(0.0032)
28	0.0004	(0.0020)	0.0006	(0.0025)	0.0025	(0.0033)
29	0.0004	(0.0021)	0.0011	(0.0026)	0.0031	(0.0035)
30	0.0003	(0.0022)	0.0016	(0.0027)	0.0036	(0.0036)
Avg. log-like.	−0.14184		−0.24959		−0.35528	

Notes. Table displays estimated marginal effects. Models (8,640 obs., 480 individuals) include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.24: Binary effects of prior punishment/reward on individual behavior (unconditional)

	Punishment		Reward		Viewing	
	Effect	SE	Effect	SE	Effect	SE
Effect of prior punishment						
Participant #						
1	0.0897	(0.0648)	−0.0536	(0.0451)	0.0426	(0.0721)
2	0.0565*	(0.0325)	−0.0481	(0.0298)	0.0132	(0.0419)
3	0.0384*	(0.0201)	−0.0437**	(0.0217)	−0.0027	(0.0279)
4	0.0269*	(0.0148)	−0.0398**	(0.0169)	−0.0121	(0.0210)
5	0.0190	(0.0123)	−0.0363**	(0.0142)	−0.0180	(0.0177)
6	0.0133	(0.0111)	−0.0331***	(0.0127)	−0.0217	(0.0161)
7	0.0090	(0.0103)	−0.0302**	(0.0118)	−0.0240	(0.0152)
8	0.0057	(0.0097)	−0.0274**	(0.0113)	−0.0254*	(0.0147)
9	0.0032	(0.0092)	−0.0248**	(0.0110)	−0.0260*	(0.0142)
10	0.0012	(0.0088)	−0.0224**	(0.0108)	−0.0262*	(0.0138)
11	−0.0004	(0.0084)	−0.0201*	(0.0106)	−0.0260*	(0.0134)
12	−0.0016	(0.0080)	−0.0179*	(0.0103)	−0.0255**	(0.0130)
13	−0.0026	(0.0077)	−0.0158	(0.0101)	−0.0248**	(0.0126)
14	−0.0034	(0.0074)	−0.0138	(0.0098)	−0.0239**	(0.0122)
15	−0.0040	(0.0072)	−0.0118	(0.0095)	−0.0230*	(0.0118)
16	−0.0045	(0.0071)	−0.0100	(0.0092)	−0.0219*	(0.0116)
17	−0.0049	(0.0071)	−0.0082	(0.0090)	−0.0207*	(0.0114)
18	−0.0051	(0.0071)	−0.0065	(0.0089)	−0.0195*	(0.0113)
19	−0.0053	(0.0073)	−0.0048	(0.0089)	−0.0182	(0.0114)
20	−0.0054	(0.0075)	−0.0032	(0.0089)	−0.0168	(0.0115)
21	−0.0054	(0.0077)	−0.0016	(0.0091)	−0.0155	(0.0118)
22	−0.0054	(0.0081)	−0.0001	(0.0095)	−0.0141	(0.0122)
23	−0.0054	(0.0084)	0.0014	(0.0099)	−0.0127	(0.0128)
24	−0.0053	(0.0089)	0.0028	(0.0105)	−0.0114	(0.0134)
25	−0.0052	(0.0093)	0.0042	(0.0113)	−0.0099	(0.0141)
26	−0.0050	(0.0098)	0.0055	(0.0121)	−0.0085	(0.0148)
27	−0.0048	(0.0103)	0.0068	(0.0130)	−0.0071	(0.0157)
28	−0.0046	(0.0108)	0.0081	(0.0140)	−0.0057	(0.0165)
29	−0.0044	(0.0113)	0.0094	(0.0150)	−0.0043	(0.0175)
30	−0.0042	(0.0118)	0.0106	(0.0161)	−0.0029	(0.0184)
Effect of prior reward						
Participant #						
1	−0.0122	(0.0393)	0.0265	(0.0312)	0.0300	(0.0507)
2	−0.0106	(0.0226)	0.0091	(0.0189)	0.0067	(0.0312)
3	−0.0092	(0.0158)	−0.0014	(0.0145)	−0.0063	(0.0227)
4	−0.0081	(0.0128)	−0.0083	(0.0131)	−0.0143	(0.0190)
5	−0.0072	(0.0114)	−0.0131	(0.0128)	−0.0194	(0.0173)
6	−0.0063	(0.0106)	−0.0164	(0.0127)	−0.0225	(0.0165)
7	−0.0056	(0.0101)	−0.0188	(0.0126)	−0.0245	(0.0160)
8	−0.0049	(0.0097)	−0.0205	(0.0125)	−0.0257*	(0.0155)
9	−0.0042	(0.0093)	−0.0216*	(0.0124)	−0.0262*	(0.0151)
10	−0.0036	(0.0089)	−0.0223*	(0.0122)	−0.0263*	(0.0147)
11	−0.0030	(0.0086)	−0.0228*	(0.0120)	−0.0260*	(0.0144)
12	−0.0025	(0.0083)	−0.0230*	(0.0119)	−0.0255*	(0.0141)
13	−0.0020	(0.0080)	−0.0230*	(0.0118)	−0.0249*	(0.0139)
14	−0.0015	(0.0079)	−0.0229*	(0.0119)	−0.0240*	(0.0138)
15	−0.0010	(0.0078)	−0.0226*	(0.0120)	−0.0231*	(0.0138)
16	−0.0006	(0.0077)	−0.0222*	(0.0122)	−0.0221	(0.0140)
17	−0.0002	(0.0078)	−0.0218*	(0.0125)	−0.0210	(0.0142)
18	0.0002	(0.0079)	−0.0212*	(0.0128)	−0.0199	(0.0146)
19	0.0006	(0.0081)	−0.0206	(0.0133)	−0.0187	(0.0150)
20	0.0009	(0.0084)	−0.0200	(0.0138)	−0.0175	(0.0156)
21	0.0013	(0.0088)	−0.0193	(0.0144)	−0.0163	(0.0162)
22	0.0016	(0.0092)	−0.0186	(0.0151)	−0.0150	(0.0169)
23	0.0019	(0.0096)	−0.0179	(0.0158)	−0.0138	(0.0177)
24	0.0023	(0.0101)	−0.0172	(0.0165)	−0.0125	(0.0185)
25	0.0025	(0.0106)	−0.0164	(0.0173)	−0.0113	(0.0194)
26	0.0028	(0.0111)	−0.0156	(0.0181)	−0.0101	(0.0203)
27	0.0031	(0.0117)	−0.0148	(0.0189)	−0.0088	(0.0212)
28	0.0034	(0.0122)	−0.0140	(0.0197)	−0.0076	(0.0221)
29	0.0036	(0.0128)	−0.0132	(0.0206)	−0.0063	(0.0231)
30	0.0039	(0.0134)	−0.0124	(0.0214)	−0.0051	(0.0240)
Avg. log-like.	−0.14175		−0.24959		−0.35541	

Notes. Table displays estimated marginal effects. Models (8,640 obs., 480 individuals) include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.25: Log effects of prior punishment/reward on individual behavior (unconditional)

	Punishment		Reward		Viewing	
	Effect	SE	Effect	SE	Effect	SE
Effect of prior punishment						
Participant #						
1	0.1019	(0.0827)	−0.0816	(0.0785)	0.0815	(0.1401)
2	0.0660	(0.0559)	−0.0709	(0.0590)	0.0492	(0.0981)
3	0.0434	(0.0361)	−0.0598	(0.0448)	0.0274	(0.0704)
4	0.0285	(0.0245)	−0.0498	(0.0347)	0.0126	(0.0519)
5	0.0180	(0.0177)	−0.0410	(0.0274)	0.0024	(0.0394)
6	0.0104	(0.0137)	−0.0333	(0.0224)	−0.0045	(0.0312)
7	0.0047	(0.0115)	−0.0267	(0.0190)	−0.0093	(0.0258)
8	0.0004	(0.0102)	−0.0210	(0.0169)	−0.0125	(0.0224)
9	−0.0030	(0.0094)	−0.0161	(0.0155)	−0.0144	(0.0202)
10	−0.0055	(0.0089)	−0.0117	(0.0146)	−0.0155	(0.0188)
11	−0.0075	(0.0085)	−0.0080	(0.0139)	−0.0159	(0.0178)
12	−0.0090	(0.0081)	−0.0046	(0.0134)	−0.0158	(0.0169)
13	−0.0101	(0.0078)	−0.0017	(0.0128)	−0.0154	(0.0162)
14	−0.0109	(0.0075)	0.0008	(0.0122)	−0.0146	(0.0154)
15	−0.0115	(0.0072)	0.0031	(0.0116)	−0.0136	(0.0147)
16	−0.0119*	(0.0070)	0.0051	(0.0110)	−0.0124	(0.0140)
17	−0.0120*	(0.0068)	0.0068	(0.0104)	−0.0111	(0.0134)
18	−0.0121*	(0.0066)	0.0084	(0.0099)	−0.0097	(0.0128)
19	−0.0120*	(0.0064)	0.0098	(0.0094)	−0.0082	(0.0123)
20	−0.0118*	(0.0064)	0.0110	(0.0090)	−0.0066	(0.0119)
21	−0.0116*	(0.0064)	0.0121	(0.0088)	−0.0050	(0.0116)
22	−0.0112*	(0.0064)	0.0131	(0.0087)	−0.0034	(0.0116)
23	−0.0108*	(0.0066)	0.0139	(0.0089)	−0.0018	(0.0117)
24	−0.0104	(0.0068)	0.0147	(0.0092)	−0.0001	(0.0120)
25	−0.0099	(0.0071)	0.0154	(0.0096)	0.0015	(0.0125)
26	−0.0094	(0.0074)	0.0159	(0.0102)	0.0031	(0.0131)
27	−0.0088	(0.0078)	0.0165	(0.0110)	0.0047	(0.0139)
28	−0.0082	(0.0082)	0.0169	(0.0118)	0.0063	(0.0147)
29	−0.0076	(0.0087)	0.0173	(0.0127)	0.0079	(0.0157)
30	−0.0070	(0.0092)	0.0177	(0.0137)	0.0094	(0.0167)
Effect of prior reward						
Participant #						
1	−0.0267	(0.0372)	−0.0315	(0.0337)	−0.0563	(0.0629)
2	−0.0200	(0.0246)	−0.0303	(0.0265)	−0.0487	(0.0465)
3	−0.0156	(0.0177)	−0.0278	(0.0210)	−0.0413	(0.0353)
4	−0.0125	(0.0137)	−0.0248	(0.0172)	−0.0346	(0.0279)
5	−0.0102	(0.0114)	−0.0219	(0.0148)	−0.0288	(0.0232)
6	−0.0085	(0.0100)	−0.0190	(0.0133)	−0.0237	(0.0203)
7	−0.0071	(0.0093)	−0.0163	(0.0124)	−0.0192	(0.0185)
8	−0.0060	(0.0088)	−0.0138	(0.0118)	−0.0152	(0.0173)
9	−0.0051	(0.0084)	−0.0114	(0.0114)	−0.0117	(0.0164)
10	−0.0043	(0.0082)	−0.0092	(0.0111)	−0.0086	(0.0156)
11	−0.0037	(0.0079)	−0.0072	(0.0107)	−0.0058	(0.0150)
12	−0.0032	(0.0077)	−0.0054	(0.0103)	−0.0033	(0.0143)
13	−0.0027	(0.0074)	−0.0037	(0.0100)	−0.0010	(0.0136)
14	−0.0023	(0.0072)	−0.0021	(0.0096)	0.0011	(0.0130)
15	−0.0020	(0.0069)	−0.0006	(0.0092)	0.0030	(0.0124)
16	−0.0017	(0.0067)	0.0008	(0.0088)	0.0047	(0.0118)
17	−0.0014	(0.0065)	0.0021	(0.0084)	0.0063	(0.0113)
18	−0.0012	(0.0063)	0.0032	(0.0081)	0.0078	(0.0108)
19	−0.0010	(0.0062)	0.0043	(0.0079)	0.0091	(0.0105)
20	−0.0009	(0.0061)	0.0054	(0.0077)	0.0103	(0.0103)
21	−0.0007	(0.0061)	0.0063	(0.0076)	0.0115	(0.0102)
22	−0.0006	(0.0062)	0.0072	(0.0077)	0.0125	(0.0102)
23	−0.0005	(0.0063)	0.0081	(0.0078)	0.0135	(0.0104)
24	−0.0004	(0.0064)	0.0088	(0.0080)	0.0144	(0.0107)
25	−0.0004	(0.0066)	0.0096	(0.0082)	0.0153	(0.0111)
26	−0.0003	(0.0069)	0.0103	(0.0086)	0.0161	(0.0117)
27	−0.0002	(0.0072)	0.0109	(0.0090)	0.0168	(0.0123)
28	−0.0002	(0.0075)	0.0115	(0.0095)	0.0175	(0.0129)
29	−0.0002	(0.0079)	0.0121	(0.0100)	0.0181	(0.0136)
30	−0.0001	(0.0082)	0.0126	(0.0106)	0.0187	(0.0144)
Avg. log-like.	−0.14195		−0.24975		−0.35554	

Notes. Table displays estimated marginal effects. Models (8,640 obs., 480 individuals) include dummy variables for each story and a quadratic effect of page position. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.26: Effects on punish/reward choices conditional on viewing (social vs. independent)

	Punishment		Reward	
	(1) Social	(2) Independent	(3) Social	(4) Independent
Points sub'ed (dummy)	2.595***	-1.326	-2.170***	0.730
	(0.693)	(1.695)	(0.693)	(1.007)
Points sub'ed × Log(participant #)	-0.878***	-0.013	0.809***	-0.021
	(0.255)	(0.598)	(0.249)	(0.365)
Points added (dummy)	-0.301	-1.912	0.145	-1.049
	(0.676)	(1.449)	(0.540)	(1.089)
Points added × Log(participant #)	0.126	0.695	-0.096	0.355
	(0.264)	(0.559)	(0.212)	(0.420)
Individuals	360	119	360	120
Obs.	1,202	345	1,202	353
Avg. log-likelihood	-0.50882	-0.49301	-0.59917	-0.61571

Notes. Table displays coefficients from logit models using participant-by-story panel data conditioned on viewing a story. Models do not include controls for page position. The points subtracted (added) dummy indicates if the story was punished (rewarded) by any previous participant. Standard errors, in parentheses, are clustered by individual. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

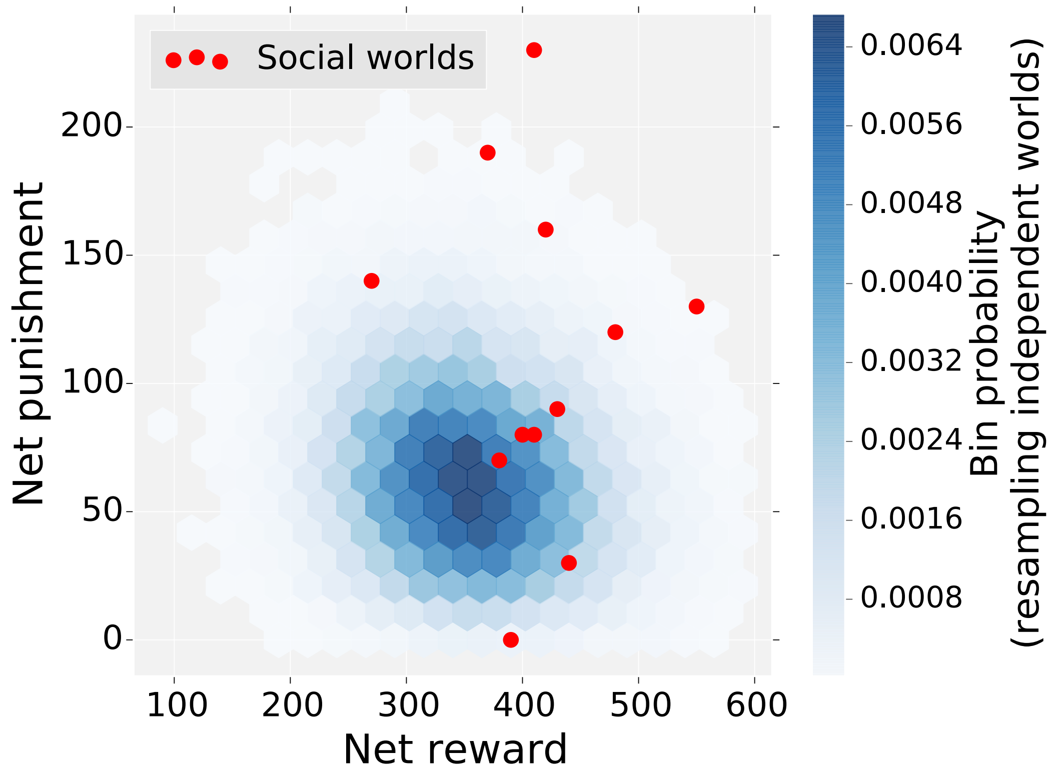


FIGURE 1.26: Bivariate histogram of 50,000 resampled independent worlds vs. observed social worlds

Figure 1.26 shows the results of drawing 50,000 worlds from the independent worlds re-sampling distribution. The distribution of net reward and net punishment is plotted as a bivariate histogram (heatmap). The observed values from the 12 social worlds at overlaid as red points. The most evident pattern is that the realizations of net punishment in the social worlds are highly dispersed, with many of the observations lying in regions where the re-sampling distribution has low density.

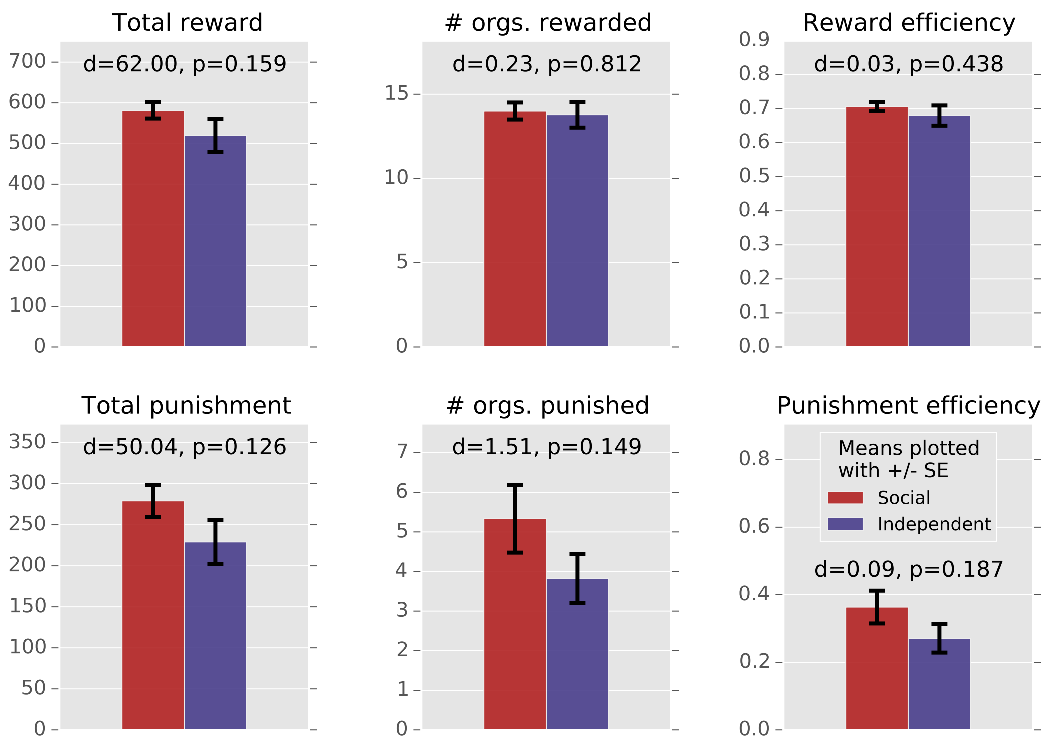


FIGURE 1.27: Additional tests for world-level outcomes

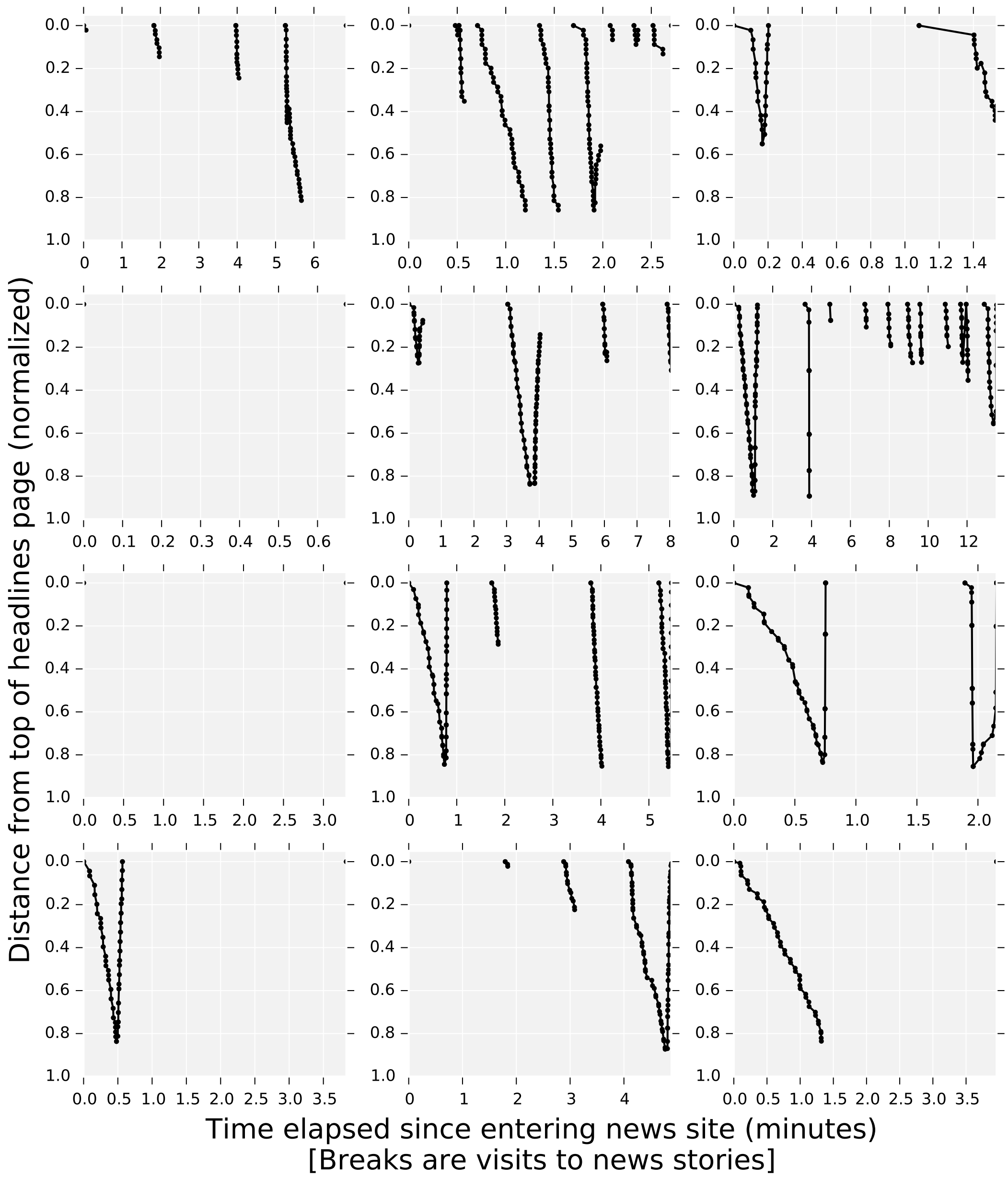


FIGURE 1.28: Search behavior of 12 individuals on the headlines page

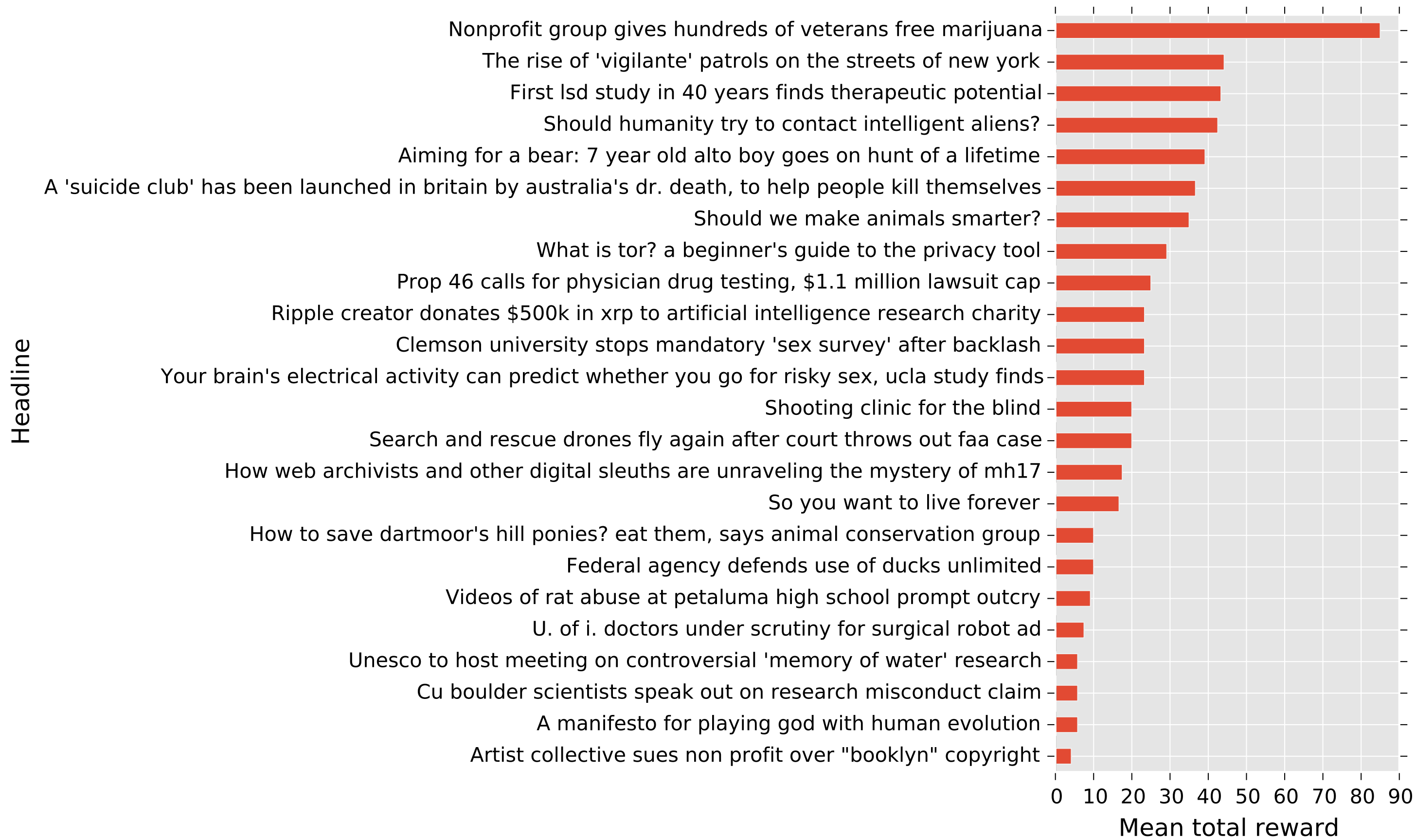


FIGURE 1.29: Mean total reward by story

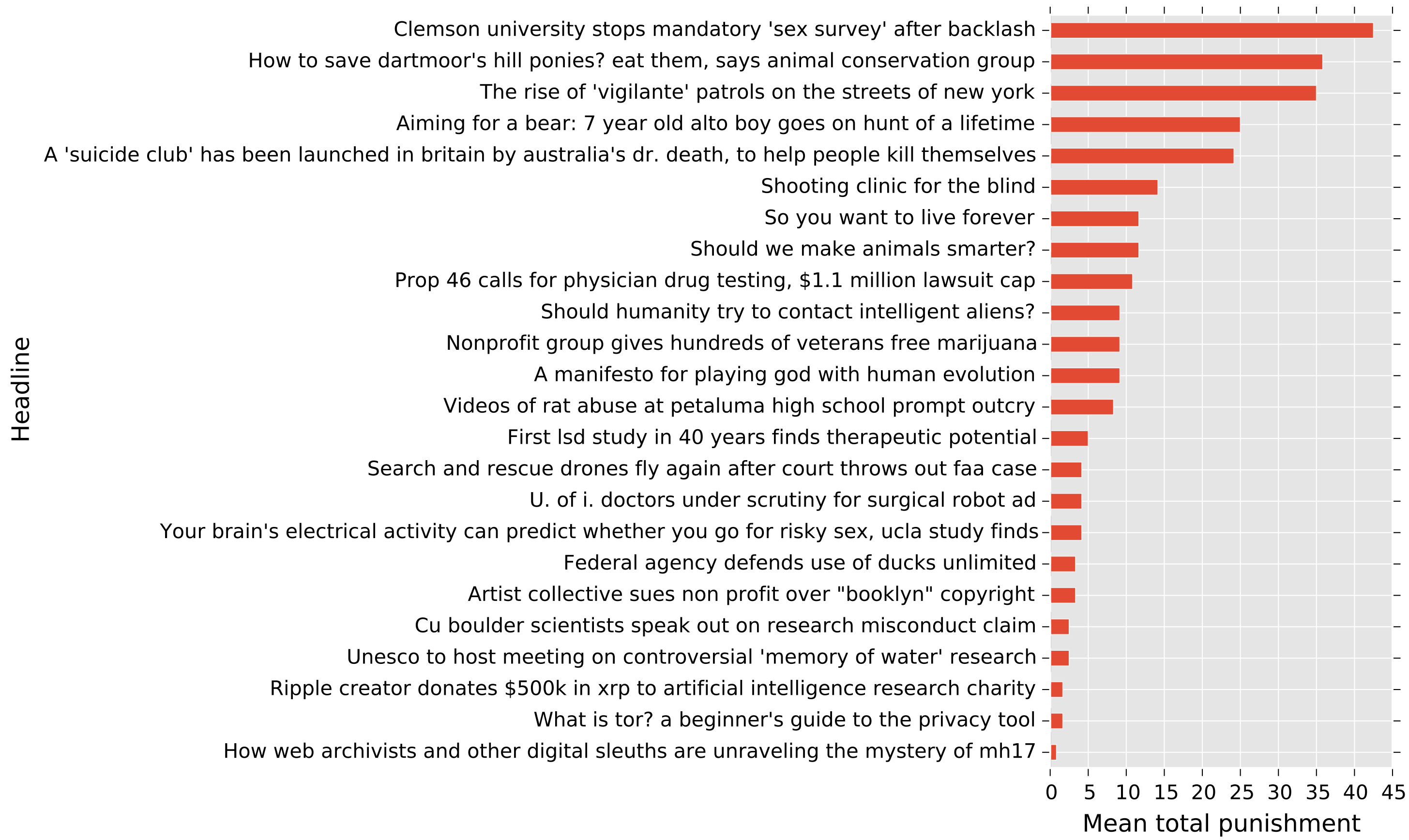


FIGURE 1.30: Mean total punishment by story

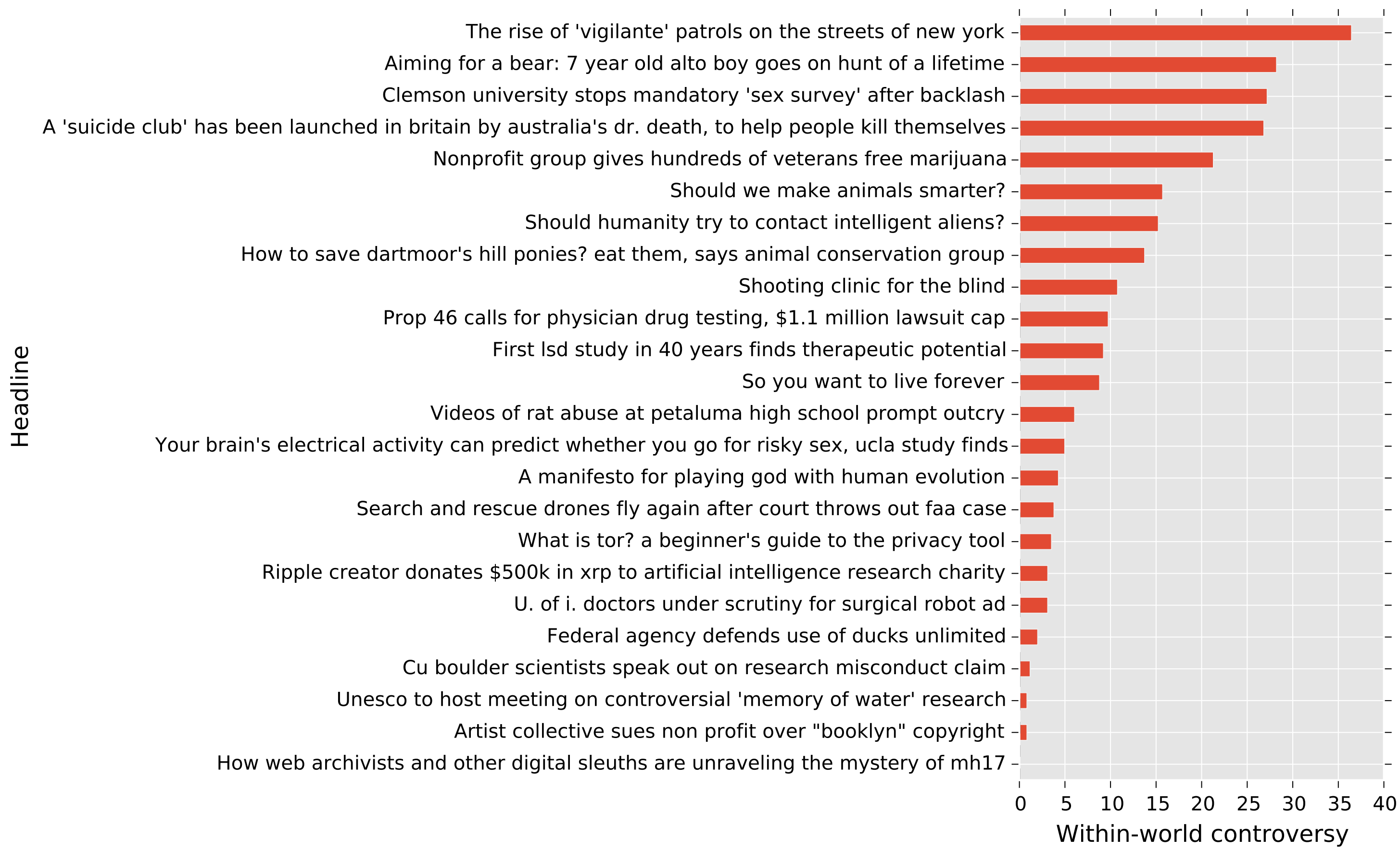


FIGURE 1.31: Mean controversy by story

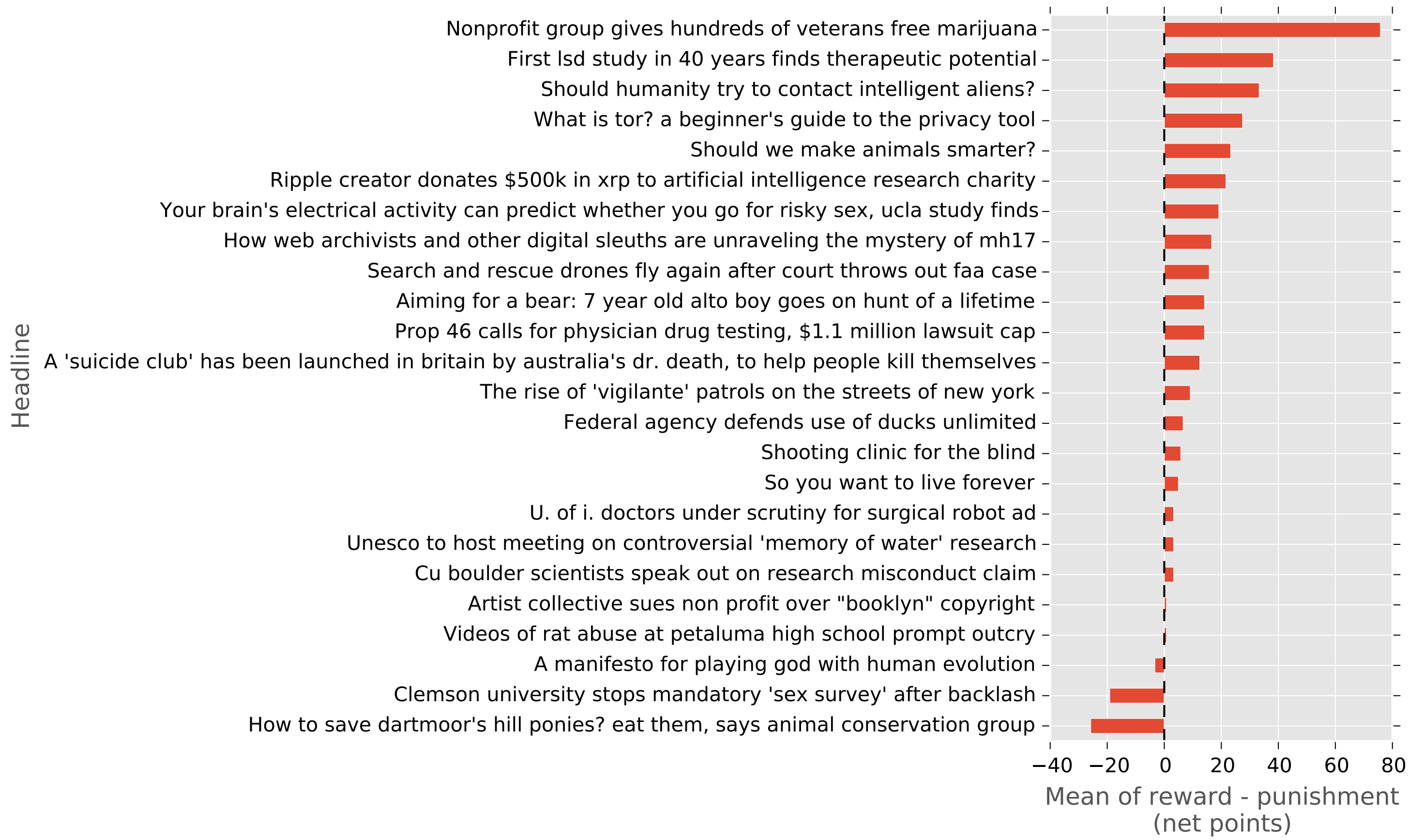


FIGURE 1.32: Mean net change in points by story

1.7.5. Open-ended descriptions of experienced anger (raw questionnaire data)

Responses to the prompt: “Please use this text box to describe any feelings of anger or indignation that you experienced during the study.”’

	experience
1.	People were too uptight about a survey.
2.	I didn’t have any feelings of anger or indignation.
3.	I didn’t experience any anger at all even when I might be in possible opposition to the story/fun..
4.	I did not feel any anger.
5.	one story did affect mw
6.	
7.	None
8.	I thought the hunting story was ridiculous. Killing animals for sport is worthless.
9.	Reading about the students abusing the rats in the museum was frustrating, saddening, and made me..
10.	Not at all. The stories do not really pertain to my immediate situation.
11.	
12.	I did not experience anger or indignation in any way.
13.	Thought it was weird about the handing out of marijuana
14.	none
15.	Civilian patrols are a good idea but beating up on minorities is a dick move.
16.	I find it hard to believe that some scientists think that altering animal intelligence is benefic..
17.	no anger
18.	
19.	
20.	I was a bit angry at the story that proposed the eating of pony meat, but I am not a very angry p..
21.	A bit of anger at some of the stories.
22.	
23.	i was calm while reading the stories
24.	I experienced no feelings of anger.
25.	
26.	
27.	
28.	
29.	The suicide story made me angry because it seemed as if someone was profiting off of people’s suf..
30.	None
31.	I was angry at seeing all the crimes committed and it took a civilian to make a vigilante force t..

32. — none, i like reading about medicine and neuroscience —
33. — I'm amazed at the things people think are good ideas. It's as if things are never thought through.. —
34. — —
35. — none - I purposely avoided the story (rats) that I felt would make me angry —

---

36. — none —
37. — I was not angry, I was intrigued. —
38. — I didn't experience any anger, or at least I don't think I did. —
39. — I was an am intrested on how New York in particular would handle the recent decline in funding Po.. —
40. — I thought that Clemson UNiversity was being unnecessarily invasive and overstepping their bounds .. —

---

41. — N/A —
42. — I was angry at the fact that medical practicioners are not subjected to random drug tests and at .. —
43. — Anger mostly at accounts of invasion of personal privacy by an institution. —
44. — I hard a hard time believing what one organization wanted to do. I could understand their motivat.. —
45. — I really didn't feel any type of anger reading these two stories. —

---

46. — The reason I gave it a 4 is because in the second story I was not happy with the way the civilian.. —
47. — Interesting what kind of programs people fund. —
48. — How can companies get away with some of those things? —
49. — I thought it was not okay for the Clemson survey to ask such invasive questions —
50. — I didn't experience any anger because I completely agree with testing doctors who are off duty fo.. —

---

51. — I feel that I did not really experience too much anger because I always look at the positive side.. —
52. — I was a bit angry when reading about the vigilante patrols. —
53. — i did not experience any anger —
54. — One particular story was about the ponies.They had a solution to save them by eating them!!They d.. —
55. — no anger, just interst —

---

56. — When I read about lab rats being abused by students at Petaloma High School, I became very angry... —
57. — The hunting article and the horse eating article made me angry. It shows a disrespect for other .. —
58. — Anger at being labeled —
59. — The suicide article got to me. I am strongly against euthanasia. I knew someone that lived reckle.. —
60. — —

---

61. — No anger from this story. I found it to be interesting idea of neighbors helping neighbors, espec.. —
62. — Vigilantism can go horribly wrong. —
63. — Surprised at some of organizations and their goals much less excepting donations . How could you.. —
64. — I get upset when personal privacy isn't respected. —
65. — —

---

66. — —
67. — none —
68. — I was very angry at the thought of a shooting school for the blind. Americans with eyesight are a.. —
69. — Not really anger. Amazed at what people will do —

70. — —

---

71. — I didn't like the that the sources were from wikipedia. —
72. — —
73. — It was bad that one of the vigilante groups beat a gay black man. —
74. — I was not surprised, we live in 1984 now —
75. — I read about how LSD is making a comeback for use with cancer patients. Drugs kill so I am a bit .. —

---

76. — —
77. — —
78. — none —
79. — the suicide club really angered me i think it would take a persons choice away, much like the ore.. —
80. — I didn't feel anger. —

---

81. — All of the stories I read made me slightly anger because the abuse of power in them. —
82. — No anger experienced. —
83. — i didn't feel anything. —
84. — —
85. — I experienced some indignation, particularly with the story regarding the sex survey that the sch.. —

---

86. — —
87. — When I read about the Jewish Orthodox patrol group beating a gay black man, I felt a little indig.. —
88. — I read the story about the patrol groups for neighborhoods and I did experience some anger about .. —
89. — —
90. — I felt angry when reading the story about the students abusing the rats at the museum. Regardless.. —

---

91. — not really angry, but annoyed at times at the things people feel are important, and the things pe.. —
92. — none whatspever —
93. — —
94. — I was angry at the vigilantes that had harmed people and not reported a missing child and at the .. —
95. — I did not experience anger. —

---

96. — Didnt really feel angry at all. —
97. — didnt like that students were abusing rats. —
98. — I experienced indignation with a couple articles. Especially accepting suicide as a right and it.. —
99. — —
100. — I didn't feel much anger. It just seemed law enforcement does not understand that other people ca.. —

---

101. — I feel that people need to stop trying to play God. If animals were meant to be smarter, then the.. —
102. — Really, they were kind of mild-mannered, even-keeled stories. —
103. — This made me feel angry because I don't think that this is the right way to go about assisted sui.. —
104. — I want to be able to trust the law enforcement in an area but also understand feelings of concern.. —
105. — I didn't feel anything. —

---

106. — none experienced —

107. — —
108. — I did not experience any anger. —
109. — I really only clicked on the links I liked. I was mildly angry at the Clemson university story, b.. —
110. — None —

—————————————————————————————

111. — I thought that some of the stories did not warrant the attention that they were getting. —
112. — Curiosity, I only read the SETI article. —
113. — No anger feelings —
114. — I did not experience any type of anger —
115. — —

—————————————————————————————

116. — While I read the one on the MIRI, many of the titles seemed to be written to aggravate the reader. —
117. — The article I read about was overall positive. There was no display of anger or indignation. —
118. — None —
119. — —
120. — pissed me off. Might as well try to sell the eponymous bridge too. —

—————————————————————————————

121. — I felt that this was very intrusive. —
122. — I disagreed with some of the news items. —
123. — Veterans looking for additional medical help for pain and suffering. Ponies being killed for food.. —
124. — I didn't feel any anger. At most, I felt mild bemusement. —
125. — I didn't feel any anger in the stories I had read. —

—————————————————————————————

126. — —
127. — The thought of eating ponies kind of made me angry —
128. — I have no anger at all. —
129. — Some stories' headlines made me feel mostly appalled rather than angry. I chose to ignore some of.. —
130. — Of all the thousands of nonprofits in the world that could use support and address issues that I .. —

—————————————————————————————

131. — I was very anger and upset over one of the articles. —
132. — Even though I detest rats and mice, it's wrong to mistreat them in that museum. Also, civilians .. —
133. — I thought that the Clemson survey was distasteful and I would have been very angry if I had been .. —
134. — —
135. — —

—————————————————————————————

136. — n/a —
137. — —
138. — —
139. — n/a —
140. — I felt a tiny bit angry at an organization advocating euthanasia. —

—————————————————————————————

141. — i had none —
142. — I felt just slightly upset that raising the lawsuit amount would increase medical costs for every.. —
143. — —
144. — I didn't understand why there would ever be a gun range for the blind. —

145. — Neutral —

—--—

146. — Very slight anger that social conventions and governmental laws are restricting the study of pote.. —
147. — I had no feelings of anger. —
148. — —
149. — None of these stories made me very angry. I was slightly annoyed at the vigilante cops, but not e.. —
150. — It really bothered me that we are a nation so completely obsessed with firearms that we are putti.. —

—--—

151. — I felt a little angry that we have to worry about doctors using illegal drugs and misusing prescr.. —
152. — —
153. — I didn't really experience any anger —
154. — I had no anger or any aggressive emotions during the study. —
155. — No anger-just thought the topic at hand was very intrusive to the students who participated. Majo.. —

—--—

156. — Moderately angry at the thought that there is an organization sharing suicide methods and tips to.. —
157. — U felt a little angry that some Dr. S don't feel they need to be regulated. —
158. — I felt joy learning that veterans can cure symptoms of ptsd by using marijuana —
159. — I believe that the group that is trying to contact extraterrestrial life is putting everyone on e.. —
160. — I felt anger over the mistreatment of animals and the stripping of rights from our veterans. —

—--—

161. — —
162. — I was partially angered at the fact that some of the organizations asking for money were ridiculo.. —
163. — The only thing that upset me was eating ponies to save the species. That seems silly. Also, I w.. —
164. — None —
165. — I did not feel any anger. It was just a medical article about LSD. —

—--—

166. — I didn't have any anger —
167. — I did not feel angry or feel any indignation over reading the news. It was not personal. —
168. — —
169. — The story I read had a small boy that is dying allowed to fulfill his hunting dream. Even though .. —
170. — I did not feel any anger. I picked topics that did not anger me. —

—--—

171. — I did not experience any anger during the study. —
172. — I felt almost no feelings of anger. The one time I felt a little uneasy maybe edging towards ange.. —
173. — Mainly about the story I read regarding physicians being under the influence of drugs while prefo.. —
174. — The feelings of anger an indignation were during the reading of the story about the school with t.. —
175. — —

—--—

176. — I did not feel angry. —
177. — and while I didn't agree with many of the stories (helping people kill themselves? could never l.. —
178. — I was very angry at the thought of doctors performing surgery while they are under the influence .. —
179. — —
180. — I did not like the use of Student ID numbers in the survey I read about in the article —

—--—

181. — I read about the horses being used for meat and the thought is angering but when you hear that it.. —

182. — none —
183. — Was upset over impropriety in an academic setting —
184. — I can see that some of the stories were meant to provoke different kind of anger. I had more curi.. —
185. — Only the general anger that happens when one realizes how weird and potentially terrible the worl.. —

---

186. — why do people not think ahead, animals are animals and humans are humans, there are so many probl.. —
187. — .. —
188. — —
189. — —
190. — I did not feel any anger. —

---

191. — I didn't feel any anger. —
192. — I felt angry that people are so judgmental concerning the articles. I even felt that I was fallin.. —
193. — I think some of the science articles were ridiculous, people playing god —
194. — I was moldy angry reading the story of vigilantes in new York city. It arouses my suspicions when.. —
195. — I was disturbed by the rat abuse story on a couple of levels, both the abusive treatment they rec.. —

---

196. — I find it a little maddening that vets aren't taken care of the way they should be. We send thes.. —
197. — —
198. — none —
199. — Anger at FAA regulation. —
200. — —

---

201. — The story of humanistic advance and playing God angered me. —
202. — I didn't feel too much anger. I didn't like the fact that the Clemson students needed to sign in.. —
203. — It was a ridiculous survey that the university did. It is none of their business and very intrusi.. —
204. — I experienced absolutely no anger or indignation. —
205. — —

---

206. — NO FEELINGS —
207. — I didn't feel anger at all while reading these stories. In fact, I felt more curious than anythin.. —
208. — angry at one of the articles —
209. — —
210. — I felt no anger —

---

211. — No real feelings of anger —
212. — I did not feel any emotions related to anger. —
213. — No anger, just boring headlines. —
214. — I did not experience any feelings of anger or indignation during the study, I enjoyed the stories.. —
215. — I got somewhat upset at the mandatory drug testing law —

---

216. — —
217. — —
218. — —
219. — I had no feeling sof anger, just sympathy for the veterans. —

220. — skeptical about the story about citizens patrols. —

—---—

221. — —

222. — —

223. — It amazes me that people have the right intentions, but go about it in a wrong way. That is just.. —

224. — None except for mild disagreement with Hawkings contention that contact with aliens might jeopard.. —

225. — Eating the horses was pretty gross and made me mad. —

—---—

226. — I didn't really feel any. The two stories I read were both very interesting. —

227. — Coming for a state that pot is legal I can see people trying to legalize the use of LSD for medic.. —

228. — I did not have any feelings of anger. —

229. — —

230. — I thought that the articles I read were interesting —

—---—

231. — —

232. — The reading about aliens was as annoying as articles I read in the news paper everyday. —

233. — I felt angry that citizens have to do the work that police are paid to do. What is even the point.. —

234. — Bear hunting story and Ukrainian plane story were a bit maddening. —

235. — —

—---—

236. — I did not feel any anger or indignation. —

237. — The story about the rats being abused filled me with anger. Animal abuse is ugly, ugly behavior. —

238. — I mostly felt anger at the idiotic students that somehow think killing animals is fun. And who w.. —

239. — I didn't like the celebration of an animal's death. —

240. — I was angry that some people are too focused on changing things don't need to be changed. —

—---—

241. — I felt indignation at the sex survey article. It was invasive and assumptive. —

242. — none. —

243. — It makes me a little bit upset when I read about animal being test subjects. —

244. — I never felt angry. —

245. — Reading the eating ponies angered me. —

—---—

246. — —

247. — —

248. — I experienced no anger. —

249. — I didn't see anything to get actually angry about. —

250. — i didnt really feel to much of anything during the study, neutral feelings. i wasnt angry or happ.. —

—---—

251. — —

252. — —

253. — some of the articles I wasnt interested in —

254. — The Dr. Death guy really made me angry. And also those presumptuous stoners handing out maryjane.. —

255. — Pets being made smarter is neutral with me. I read about doctors being drug tested. I think it's .. —

—---—

256. — I was not angry at all —

257. — —
258. — None experienced. —
259. — I felt anger when reading the story about the abused rats. It's disturbing that children, or anyb.. —
260. — None —

---

261. — did not experience anger —
262. — Some of the stories were quite biased and I did not agree with them. —
263. — I didn't experience any anger but I did experience slight indignation. —
264. — I read a story about blind people shooting guns. I got a little angry hearing that at least one .. —
265. — —

---

266. — I really didnt think eating the ponies was a good idea. It made me angry to read this. —
267. — I don't think it's right to allow kids who are terminally sick to go out hunting. It seems hypocr.. —
268. — I didn't experience any anger, I found the survey interesting and informative. —
269. — —
270. — I do not have any feelings of anger, but only feel more knowledgeable after reading such informat.. —

---

271. — I felt a little annoyed that there is a whole organization committed to saving Dartmoor ponies by.. —
272. — I was indignant at a few of the headlines. I also was disgusted by the article about eating ponie.. —
273. — frustrating about news on government and politics and heatlh etc. —
274. — I felt a little irritation, especially with regard to the articles about guns. —
275. — More than anger I felt outraged. These are public funds. Palladium is an important resource, and .. —

---

276. — but then I ended up giving then points for helping kids. —
277. — I didn't feel any of the articles I read were meant to invoke anger or indignation. They were al.. —
278. — I didn't feel any anger or indignation really, maybe more interest and perplexity —
279. — I usually don't trust the media. The story I read about weed was interesting but annoyed me a lit.. —
280. — I was somewhat angered about the lack of disclosure in the ad for robotic surgery. —

---

281. — n/a —
282. — I could not really understand why the blind need to shoot guns, did not get the point. —
283. — I got angry when I read the story about Clemson University because of the types of questions they.. —
284. — none —
285. — When reading about the drones, I felt angered at how much fuss people make over them. Seriously, .. —

---

286. — I did not experience any feelings of anger —
287. — The whole suicide club article got me pretty angry —
288. — —
289. — There was information within one of the articles I read that brought up anger from prior knowledg.. —
290. — —

---

291. — I was angered about the unjust deaths of animals, especially the rats. —
292. — while reading the stories about eating ponies i was horrified and angry. where do we draw the lin.. —
293. — A few made me angry like hunting trips for kids but overall it was more mild disapproval like the.. —
294. — I experienced no anger. —

295. — I was slightly indignant at the abrupt halt to lsd research since I believe it has benefits. —

—--—

296. — i read different story about people try to help other but the government get involve and miss thi.. —
297. — None —
298. — I felt anger concerning the vigilante/crime watch article in which a group beat a black gay man. —
299. — I had no anger. I read the article at first with no bias and then made my own decisions. —
300. — —

—--—

301. — no anger —
302. — response. —
303. — I felt that there was a lot of irresponsible behavior described in most of the articles I read, a.. —
304. — —
305. — —

—--—

306. — I did not like the idea of making meat out of the ponies. I also was angry at the treatment of t.. —
307. — I always feel upset when I learn that anyone is mistreated or treated unfairly. For the most part.. —
308. — N/A —
309. — I did not feel any feelings of anger. —
310. — I am vegetarian so stories about hurting animals made me unhappy. —

—--—

311. — The sex survey article was pretty ridiculous and sort of funny. —
312. — —
313. — —
314. — none —
315. — I was annoyed in particular at the Clemson story. There is a problem on campus, but I'm not sure .. —

—--—

316. — —
317. — Some of the organizations, in my opinion are just plain stupid. That they would actually garner .. —
318. — I was shocked that Clemson would publish a study that would allow confidential information to be .. —
319. — I felt angry after seeing an image of a pig that was hunted and killed by a little boy. —
320. — I did not really experience any anger or indignation. I thought the articles and organizations we.. —

—--—

321. — I was angry to see the meat market approach to saving Hill ponies. —
322. — I didn't really feel angry, although I disagreed with some of the ethics. —
323. — I was a bit angry with the LSD and marijuana legalization efforts, knowing how damaging these dru.. —
324. — I felt angry about the scientists at Colorado Boulder that may have been falsifying their results.. —
325. — There were little amounts of anger in reading the article about Dr.Death and his attempt to allow.. —

—--—

326. — I was a little angry about the article that said to save horses we should eat them. —
327. — I felt angry at a story about animal abuse reported at a school, as well as a story about researc.. —
328. — there was a story about hunting which made me a bit angry but i did not read the story after seei.. —
329. — —
330. — and on transhumanism, because I felt that they did not deal very well with the science involved. —

—--—

331. — no anger or indignation —

332. — Not very much anger. More like confusion. —
333. — There were a lot of stories that were not real news or beneficial. —
334. — I experienced some anger toward doctors who would treat patients while 'under the influence' ther.. —
335. — I was angry about one of the articles I read and how the study participants in it were treated. —

———————————————————————————————————

336. — I didn't experience any anger. —
337. — —
338. — Nothing made me angry. —
339. — Some of the stories made me angry at the organizations that were unethical and abused their powers. —
340. — I felt it was ridiculous to teach blind people to shoot guns, and wondered why the State of New H.. —

———————————————————————————————————

341. — none —
342. — I was perfectly at peace during the study. —
343. — I did not feel angry at all doing the readings —
344. — I got angry when I read about Drs. performing surgery with drug problems. —
345. — —

———————————————————————————————————

346. — No anger —
347. — —
348. — —
349. — —
350. — No anger at all —

———————————————————————————————————

351. — Did not have any anger. —
352. — —
353. — It made me slightly angry to read that Congress has mandated that a third of military forces are .. —
354. — I did not experience anger or indignation. —
355. — none really —

———————————————————————————————————

356. — I didn't feel any anger. I felt the Doctor's decision is justified. If someone wants to end their.. —
357. — The thought of eating horses is disgusting —
358. — I had some anger at Clemson University for their questions about how many times the students had .. —
359. — There were none. —
360. — —

———————————————————————————————————

361. — I felt indignation because none of the news stories were inspirational. None of the organizations.. —
362. — I did think the pony eating was a little disgusting, but I didn't feel anger or indignation. Also.. —
363. — I felt no real anger. I am not keen on some of the subjects, but I wouldn't call what I felt as a.. —
364. — —
365. — I don't recall feeling any anger. —

———————————————————————————————————

366. — most of the studys in these news story where just bull i just got mad about the waste of time and.. —
367. — —
368. — I felt some anger from the euthanasia article when at the end of the article they quoted someone .. —
369. — No anger...maybe a little fear. (the alien one) —

370. — N/A I don't get angry easily —

———————————————————————————————————

371. — Well, i was not angry, but there are a couple of stories that really make you re-think beliefs an.. —
372. — No anger felt. I had a position in my mind while reading but they didn't cause any agitation or a.. —
373. — Slight anger over those who can't see being allowed to use firearms. Moderate anger over eating h.. —
374. — —
375. — I fully understood each article and nothing anger me. —

———————————————————————————————————

376. — I thought it was a little strange that someone would think that everyone living forever would be .. —
377. — none —
378. — No feelings of anger. I did think there was a potential for the vigilante justice workers to go .. —
379. — none —
380. — No anger, but I didn't like the story about the vigilants in Howard Beach —

———————————————————————————————————

381. — I felt anger when I read the rat story. I hate animal abuse. I believe all animals should have ri.. —
382. — Just don't fancy horse eating. —
383. — —
384. — its bad that their are not enough cops —
385. — Not so sure about the vigilante's in NY. Are the police officers that overwhelmed there? I think .. —

———————————————————————————————————

386. — —
387. — —
388. — i dont like untrained people in squad cars —
389. — I didn't experience any anger. —
390. — I was a bit angered and disturbed by the story about the organization promoting selling and eatin.. —

———————————————————————————————————

391. — I didn't read any stories that would make me feel any anger. There is too much sad and disgustin.. —
392. — —
393. — no anger —
394. — That a group who is supposed to save horses says to eat them, that kind of makes me angry. A scho.. —
395. — —

———————————————————————————————————

396. — None —
397. — —
398. — Hunters kids? I don't like —
399. — I felt indignant at the irresponsible methods some Pro-Euthenasia groups go about things. —
400. — I had no anger I was just reading news stories who gets angry at the news? —

———————————————————————————————————

401. — I felt a little angry because I do not agree with vigilante groups and it brought up old feelings.. —
402. — I felt no feelings of anger toward the articles I read. —
403. — i felt very little anger because i had a level head —
404. — I did not like the story about ponies and eating them. I also did not like TOR because I felt th.. —
405. — Anger that children are taught to slaughter wild animals. Anger that ponies would be killed for m.. —

———————————————————————————————————

406. — I was angered just a tiny bit because the headlines weren't necessarily ones I wanted to donate to —

407. — none —
408. — —
409. — I had no feelings of anger —
410. — Calmness and interest —
—--—
411. — is a ridiculous notion to have more ponies....if you're having more for them to be eaten, I fail.. —
412. — Most of the causes seems to be fairly liberal with the goal of playing God or defeating tradition.. —
413. — I'm just kind of confused with the points system. —
414. — None —
415. — Reading some of the stories the topics made me angry. Like eating horses and abusing mice —
—--—
416. — —
417. — —
418. — Some of the articles presented ugly sides of humanity, such as dishonest physicians and the use o.. —
419. — I did not experience much anger. The only slight feeling of indignation was when I was reading a.. —
420. — No feelings of anger! —
—--—
421. — I didn't feel any. —
422. — I don't think people should have to disclose their personal lives unless help is granted afterward. —
423. — I was somewhat disappointed by this research. There were so many discrepancies in whether or not .. —
424. — The only story that upset me was about the assisted suicide. It has been a hot topic lately, so I.. —
425. — Some of those people are so stupid! —
—--—
426. — Slightly upset —
427. — —
428. — Hunting is one thing, but just torturing animals is another. —
429. — —
430. — I just disagreed with the two stories I read. I found them very absurd! —
—--—
431. — —
432. — I read the article on eating Dartmoor ponies to save them. As the article says, I see horses as c.. —
433. — —
434. — I was angry at eating the horses and helping people commit suicide. —
435. — I think it's pathetic how people think animals should be treated. I can't stand reading about peo.. —
—--—
436. — None of the stories that I chose made me feel angry. I avoided the ones that would have, like the.. —
437. — —
438. — n/a —
439. — I was very interested in some of the stores. —
440. — I did not feel any anger or indignation. Perhaps it was my choice of articles. —
—--—
441. — The stories about gun use were particularly bothersome to me. —
442. — —
443. — I did not experience anger. —
444. — —

445. — none —

—--—

446. — I am really feeling that the vets deserve medical marijuana. —
447. — I didn't really have any feelings of anger. Some disappointment in things a story reported, but n.. —
448. — —
449. — I think it's wrong —
450. — a hard time. I believe individuals who are suffering should be allowed to end their lives in a d.. —

—--—

451. — I was frustrated that humans would either want to experiment on animals or take it upon themselve.. —
452. — I had no angry feelings. —
453. — I felt angry that the rebels in the story tried to boast on min, then when they found out it was .. —
454. — I had no feelings of anger while doing this study. —
455. — I thought that people are stupid a lot which makes me mildly angry but not tremendously angry tho.. —

—--—

456. — Not angry as much disturb by the thought of trying to live forever from the article I read. —
457. — It made me a little angry when I read they ate the horses. —
458. — —
459. — I felt very angry about the story of the rats being abused —
460. — The story was more information and not an issue that would generate a lot of emotional response —

—--—

461. — I chose articles that were not the type that causes anger —
462. — I had no anger just curiosity in the subjects at hand. —
463. — I experienced anger when reading the stories related to the animals - the abuse and mistreatment .. —
464. — I think it is ridiculous that anyone would even consider training a blind person to shoot a gun. .. —
465. — none —

—--—

466. — No anger —
467. — I wasn't angry, I found the news article interesting. —
468. — Some of the stories just touch home to me and bother me quite a bit. Although Im not furiously an.. —
469. — I was slightly angry reading about the Clemson University story. It upset me that students were .. —
470. — Some of the titles of the articles I did not want to read. I chose not to read those...I think I .. —

—--—

471. — Not really anger. Just the stories I read seemed rather bizarre and foolish, if not outright dang.. —
472. — The sex survey story made me angry. nobody has any right to make people disclose this type of inf.. —
473. — Some of the stories made me cringe because of their beliefs within the stories. —
474. — The story I read was about MD's not diclosing financial ties to the DaVinci. It's angering becaus.. —
475. — I experienced anger while reading the story of the 7 year old boy whose bear hunt was sponsored b.. —

—--—

476. — I thought it was ridiculous that the Howard Beach Civilian Patrol was not appreciated by NYPD. —
477. — I was upset that no matter what you do on the internet, it can never really be kept secret. Altho.. —
478. — I can just imagine the rise of many George Zimmerman type crimes taking place with the vigilante .. —
479. — —
480. — —

+--+

1.7.6. Open-ended general comments (raw questionnaire data)

3. — I have a lot of confidence because you put it in writing. If you don't then your credibility is s.. —
7. — none —
12. — This was an interesting and unique study. I look forward to hearing about the results. —
16. — I enjoyed it. —
17. — none, thank you —

———————————————————————————————

18. — thanks —
20. — N/A —
24. — Good luck with your research! I have some good friends at CalTech. —
27. — Interesting articles, some I've heard of and some I will look up after survey completion. —
38. — It was interesting to have so much control over what to read and how to influence how much money .. —

———————————————————————————————

39. — program where you and a few people ellect to talk to some one i.e a sophmore would talk to a sen.. —
41. — This was a fun and interesting Hit. Thanks, have a great day!! —
51. — Thank you, this was very nicely put together and the topics were great. —
57. — I just started Mechanical Turk so the weekly amounts above are estimates. —
58. — NA —

———————————————————————————————

63. — none —
75. — I believe some of these stories are made up for research purposes. —
82. — None —
90. — I enjoyed participating in your study and reading the articles. —
92. — ty —

———————————————————————————————

96. — I like turtles —
98. — I just started using mechanical turk so my average time and amount earned are not entirely accura.. —
100. — It was a little confusing at first, but I soon got the hang of what was required. —
103. — Interesting survey! —
114. — thank you —

———————————————————————————————

120. — What a great study presentation. Low stress, made me want to read the articles. This is a better .. —
123. — I feel very fortunate that I am well. —
124. — n/a —
130. — Very interesting study, rather unique. I appreciate the opportunity to participate. —
134. — I enjoyed reading the articles. —

———————————————————————————————

137. — Some of the headlines seemed to not be real. —
146. — none. neat new survey tool —
148. — Interesting! —
149. — Excellent HIT, thanks a ton! —
151. — Interesting and fun study. —

———————————————————————————————

154. — n/a —
162. — I doubt there will be any actual money paid even though the 10 points are merely 30 cents. I sti.. —

165. — I stated that I didn't feel any anger but the next question was how you define your anger. —
167. — Fun study. I like to read the news, you had a nice selection of different topics. I'll look for.. —
170. — No problems. —

---

175. — thanks —
185. — Wish I could have read more articles. Interesting selections! —
186. — , so I don't really have a proper weekly hours time or earnings amount. —
192. — I really enjoyed this type of survey, very unique. —
193. — na —

---

196. — good luck with your research! —
201. — real purpose?? —
204. — The news stories were varied and interesting. —
214. — N/A, thank you very much. —
225. — Feel free give out bonuses to us sub-minimum wage folks. ;-) —

---

228. — Very interesting articles! —
235. — none —
241. — None. —
250. — no additional comments. —
254. — the articles were interesting and informative —

---

255. — This has been simple to understand. —
256. — There were a lot of editing errors in the articles on this site. —
259. — n/a —
270. — I really enjoyed taking this survey. —
276. — I'm not certain all those news articles are actually real. Just a comment, not a complaint. —

---

277. — The articles were not that interesting overall. They were regarding subjects that are very much .. —
278. — Interesting study! —
279. — thank you for the study, it was interesting —
281. — n/a —
284. — none —

---

285. — This was interesting. —
289. — This was an interesting survey. I did enjoy taking it. I love to read. —
294. — Thank you! —
296. — none —
299. — none —

---

302. — Good luck--good variety of articles and I learned something new today!!! That's a BIG plus when y.. —
309. — Very interesting, thank you! —
311. — It was a fun exercise. Thanks! —
318. — None, thanks! —
329. — thank you —

—---—

330. — Some of the articles seemed to have grammatical mistakes, although this may not be your fault. Al.. —
337. — Thank you! —
338. — I liked the variety of headlines that were available to choose from. —
340. — I thought it was interesting, and different (in a good way) —
346. — None —

—---—

347. — Some of the news articles were interesting to read. —
350. — No complaints or concerns... —
353. — n/a —
355. — none —
356. — Interesting choice of stories. —

—---—

359. — I liked this study. —
361. — not any —
364. — Thanks for the articles, there were a few I wasn't aware of. —
365. — Interesting survey! —
370. — none but thanks for the survey —

—---—

375. — interesting and different —
377. — was very interesting the two stories I read were very informative —
380. — Interesting survey. Thanks! —
390. — The stories were cool! A lot of random information and organizations I didn't know about. —
391. — I found this survey very interesting. This is a lot different than most hits on Mturk which is w.. —

—---—

394. — I wish I could read more or that it explains (and maybe it does) that the headlines do not need t.. —
398. — Interesting —
400. — I love the design of this site, wish I had more time to play around on it but I have to go to din.. —
410. — Thank you. —
423. — I have no other comments about this study except that I find that there are some disturbing thing.. —

—---—

431. — none —
439. — Vary insightful!! —
441. — I really like the way this survey was designed. It was fun and easy to use. I'd love to do some m.. —
451. — No other comments. —
452. — none —

—---—

453. — Thank You —
459. — have a nice day —
461. — No comment —
462. — N/A —
467. — None —

—---—

475. — It was interesting reading the articles but I feel like the instructions were a little hard to un.. —
476. — None. —

+--+

TABLE 1.27: The questionnaire

Variable	Question text
Anger level	Overall, while reading these news stories how much did you experience ANGER? Use this scale where 1=No anger at all and 10=Greatest anger you can imagine.
Gender	What is your gender?
Education	What is the highest level of education you completed?
Experienced anger	Please use this text box to describe any feelings of anger or indignation that you experienced during the study."
Hours/week on Mturk	How many hours per week do you USUALLY work on Mechanical Turk?
Dollars/week on Mturk	How much do you USUALLY earn per week on Mechanical Turk? Please answer in dollars.
Engaged in political protest	Have you ever done any of the following forms of political action? Joining in boycotts, attending peaceful demonstrations, joining strikes.
Has smartphone	Do you have a smartphone like an iPhone, Samsung Galaxy phone, or Windows Phone?
Uses Twitter	Do you use Twitter?
Uses Facebook	Do you have a Facebook account?
Belief in donation	How much confidence do you have that we will pay out the points as explained?
Low wealth	If you were to lose your main source of income (e.g., job, government benefits), could you cover your expenses for 3 months by borrowing money, using savings, selling assets, or borrowing from friends/family?

Chapter 2

Fear itself: The effects of distressing economic news on birth outcomes

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Abstract

I use new administrative data on mass layoffs and plant closings to study the effects of distressing economic news. Exposure to stressful events during pregnancy can impair fetal development. I find that announcement of impending job losses leads to a transient decrease in the mean birth weight within the firm's county one to four months before the job losses. A loss of 500 jobs corresponds roughly to a decrease of 15–20 grams and 16 percent greater risk of low birth weight. Layoffs announced late in pregnancy are most strongly linked to decreased birth outcomes.

2.1. Introduction

Each year in the United States some 20 million jobs are lost through layoffs or discharges. Workers who lose employment face serious problems, including long-term loss of earnings and damaged health, which have been documented in the economics literature.[1] However, each time someone

[1]Displaced workers' long-term earnings losses can range from 10 to 25 percent (Ruhm 1991; Jacobson et al. 1993; Couch and Placzek 2010). Recently displaced workers are likely to drop out of the labour force (Huttunen et al. 2011)

loses employment, he first receives the unfortunate news of the impending event. And each time a firm announces the decision to lay off workers or shut down, those residing nearby must consider how the change might affect the local economy and their future livelihoods. Many forms of distressing news appear as a part of the normal course of economic activity, but we know relatively little about the effects and costs associated with these messages.

Economic research on bad news and distress is nascent and promising. For example, Deaton (2012) reports a surprisingly strong relationship between negative hedonic experience and the S&P 500 index during the recent financial crisis. He conjectures that the index functioned as a highly salient channel for pessimistic news, which drove up mental and physical symptoms on a large scale. The present study investigates a similar relationship but focuses on many, localized news shocks rather than a nationwide crisis. These news shocks are announcements of mass layoffs and plant closings at specific sites. Such events are considered in light of the human capital literature which studies the damaging effects of stress experienced by women during pregnancy. Shocking events, such as terrorism and natural disasters, can decrease birth weights and shorten gestation (see, for example, Camacho 2008a or Simeonova 2011). These findings suggest that brief events can have serious health consequences that may even affect the next generation. However, these studies' quasi-experimental designs exploit very unusual events that may have effects different from those of common economic stressors.

The conjunction of stress and economic news is exceptionally appropriate. Perhaps the most common stressors are personal finances, jobs, and economic conditions (American Psychological Association 2012). To address the question of causal identification, I construct a novel data set containing the dates of major job loss events and information about the amount of forewarning given to the local community. These data allow me to analyze the particularly interesting period in which news of job losses is taking effect, but the job losses *themselves* have not yet occurred. My empirical model is constructed to rule out the direct consequences of job loss and isolate *anticipatory effects*. Such effects include the immediate, physiological effects of stress but also behavioral

and can experience enormously elevated mortality risk (Sullivan and von Wachter 2009). A parent's job loss can also reduce the health of subsequently born children (Lindo 2011).

responses in anticipation of economic change. For example, pregnant women who receive negative information about the future might decrease consumption of healthful goods, increase consumption of unhealthful goods, become more neglectful of their health in other ways, experience stress as a result of initiating a job search, or be burdened with extra responsibilities when another member of the household initiates a job search. Many of these channels cannot be isolated with the present data.

I study mass layoffs and plant closings using administrative data from Alabama, New York, Texas, and Washington. These data, derived from notices filed under the Worker Adjustment and Retraining Notification (WARN) Act, are merged with natality data at the county-by-month level to link each birth with job losses occurring in the mother's community. The results indicate that mean birth weights drop by 15–20 grams during a brief period *before* a large job loss event (defined as one at the 95th percentile or approximately 500 workers in an average U.S. county). However, the effect is almost entirely restricted to job losses where the firm in question provides a large amount of forewarning rather than little, suggesting that bad news is the driving factor. These negative effects also appear in analyses of individual-level birth data, which reveal that WARN notices occurring in the mother's county of residence just around the time of birth are linked to significant decreases in birth weight and gestational age. The strongest effects are associated with exposure to notices in the third trimester.

The study has several limitations. Births and dislocations are linked only by time and place, because the identities of the mothers and the people affected by WARN notices are unknown. Therefore like other studies of wide-scale stressors, the estimates represent only effects averaged over the affected community. In addition, the arrival time of the bad news about worker dislocations is only approximately known, which "blurs" the estimated form of the response. Finally, caution is required when interpreting the results and considering policy implications. One should not conclude that WARN notices or worker notification laws are harmful on the basis of this study. Others have found that displaced workers can find new jobs more quickly when protected by notification laws (Friesen 1997; Jones and Kuhn 1995). Compared to job losses without any notice, notices might have some negative effects upfront but provide benefits over a longer time span. Any policy is bound to have unintended consequences. My results should instead be considered evidence that announcements of common business decisions are associated with substantial psychological and physiological costs that occur before the decision takes effect.

2.2. Background

2.2.1. Stress due to economic conditions

Several studies report that workers' physical and mental conditions deteriorate in anticipation of job loss events. Evidence from quasi-experimental designs with large samples is reported by Hamilton et al. (1990a) and Ferrie et al. (1995, 2002). The latter presents strong evidence from longitudinal data on self-reported and physiological measures of health in about 3,500 workers. Earlier studies, although smaller and weaker in some design aspects, also report physiological effects before job loss events (Kasl and Cobb 1970a, 1980a). Overall, these studies cover a variety of workers, including both blue collar and white collar workers along with both sexes. However, a notable shortcoming is that each study considers only one employer.

Individuals who do not lose employment can still suffer from layoffs. Workers remaining at firms that have conducted layoffs can exhibit worsened health and increased absenteeism as evidenced by studies of survey data (Moore et al. 2004), administrative records (Vahtera et al. 1997), and interviews with managers (Maki et al. 2005). Although these studies also examine just one employer each, the link between pessimistic job expectations and poor health is broad enough to appear in representative survey data (Kalimo et al. 2003). Finally, numerous studies of a variety of sizes report that a wife may feel stress due to her husband's layoff or job troubles (Dew et al. 1987; Rook et al. 1991; Vinokur et al. 1996; Westman et al. 2001). For example, Dew et al. (1987) report that wives of laid-off steel workers experience stress increases which depend on the husband's mental health. However, direct, person-to-person transmission—sometimes called contagion—is generally difficult to isolate from empirically similar effects, such as the effect of a common environment. This caveat is important: A community's economic and social characteristics can have especially strong effects on mental health and subjective well-being, as found in the well-known Moving to Opportunity experiment (Ludwig et al. 2012). However, to support the hypothesis of this study it is enough to document that the effects of layoffs can spread broadly. In general, it is likely that news of layoffs will spread and induce stress through a variety of channels, which include being at a directly affected employer, having a household member or family member affected, word-of-mouth, or local media coverage.

2.2.2. Prenatal stress

Research from a large variety of approaches links stress experienced by the mother during pregnancy with effects on fetal development, birth outcomes, and health later in life. Causal effects of prenatal stress are well-supported by evidence from animal experiments (for a review, see Weinstock 2005). Ethics strongly constrain exogenous stress manipulations in human subjects, but there are converging lines of evidence to support effects in humans.

Birth weight and gestational age are the most commonly studied outcomes in this literature because they are easily available and indicative of health.[2] Several studies take a simple observational approach and document that stress reported by pregnant women is negatively associated with birth weight and gestational age (see Copper et al. 1996; Dole et al. 2003; and Rondó et al. 2003 for studies with relatively large samples). However, these generally provide weak evidence of causality because a third factor may cause both stress and low birth weight. Other clinical studies focus on the mechanisms linking stress and birth outcomes, which are thought to involve neuroendocrine processes, immune-inflammatory activity, and behavior (Wadhwa et al. 2001b,a; Wadhwa 2005; Dunkel Schetter 2011). Clinical research on birth outcomes and stress hormones—cortisol and corticotropin-releasing hormone (CRH)—typically report negative correlations between hormone

[2]The literature's narrow focus on these variables is a well-known limitation (Currie 2011). Nevertheless, birth weight and gestational age are important intermediate outcomes (Cunha and Heckman 2007) that consistently predict health and socioeconomic success (Behrman and Rosenzweig 2004; Black et al. 2007; Currie and Moretti 2007a; Royer 2009). A broad review of child health and socioeconomic status is provided by Currie (2009).

levels and birth outcomes (Hobel et al. 1999; Inder et al. 2001; Wadhwa et al. 2004). Such studies still lack exogenous variation,[3] but they provide additional evidence of causality by examining mechanisms that are well-supported by basic neuroendocrinology (Wadhwa et al. 2011; Welberg and Seckl 2001; Weinstock 2005; Seckl and Holmes 2007; Glover et al. 2010; Erickson et al. 2001; McLean et al. 1995; De Weerth and Buitelaar 2005). The role of infection as a link between stress and birth outcomes has received somewhat less study than endocrinology (Wadhwa et al. 2001b, 2011), but intrauterine infection is a heavily-studied risk factor for preterm birth (Goldenberg et al. 2008).

To address the lack of exogenous variation in the clinical research, some studies use quasi-experiments generated by plausibly exogenous, stressful events, for example, natural disasters or terrorist attacks. As an added advantage, such studies can use large administrative data sets. These studies typically report that prenatal exposure to the stressor decreases the expected birth weight and gestational age. Effects in terms of mean birth weight are typically a few grams up to about 30 grams while average effects on gestational age are roughly a day (Camacho 2008a; Simeonova 2011; Eccleston 2011; Brown 2014). However, one study reports no effects of a "near miss" by a hurricane on birth weight nor on gestational age (Currie and Rossin-Slater 2013a). Some studies only consider low birth weight as an outcome, making comparisons across the literature more difficult, but these studies consistently report elevated risk of low birth weight (Catalano and Hartig 2001; Lauderdale 2006; Eskenazi et al. 2007). The use of exogenous stressors is both the main advantage *and* disadvantage of this research.[4] The events are typically extraordinary,

[3] However, Aizer et al. (2009) investigate the role of cortisol using a novel design that exploits within-mother—between-pregnancy variation in cortisol measurements taken during the third trimester. They report that higher levels predict worse cognitive and health outcomes in the child. Aizer et al. (2009) do not find evidence of an effect on birth outcomes but attribute this to the way the sample was selected.

[4] An additional concern that applies to the entire literature is the inconsistency in the evidence related to the timing of stress during pregnancy. Laboratory studies using artificial stress tests report both decreasing (De Weerth and Buitelaar 2005) and increasing (Entringer et al. 2010) sensitivity. Associations between stress tests and pregnancy outcomes are also mixed (McCubbin et al. 1996; Vythilingum et al. 2010). These studies are small, and the relationship between laboratory stress tests and naturally-occurring stressors in this context is unknown. Large-scale studies based on administrative data and exogenous stressors are similarly inconclusive. These have reported that the strongest effects occur due to exposure during early and middle pregnancy (Eskenazi et al. 2007), middle and late pregnancy (Class et al. 2011; Catalano and Hartig 2001; Simeonova 2011), the first trimester (Torche 2011), and the second trimester (Camacho 2008b).

meaning the generalizability of the findings is questionable. In addition, the stress-related causal mechanism is often poorly isolated because the stressors involve violence and destruction that could have independent health effects or drive selective migration. Some studies do attempt to address these issues, for example, by using location instruments (Currie and Rossin-Slater 2013a) or analyzing birth rates (Quintana-Domeque and Rodenas 2014).

My study mitigates these concerns by examining a common stressor before there is likely to be a significant material effect. The empirical model used for this analysis is like that used by Currie and Schmieder (2009) to study pollution releases. Following their approach, I aggregate the data at the county level and estimate how births respond to within-county variation in a continuous treatment variable. This approach has the advantages of (1) isolating the period before a worker dislocation, (2) allowing for estimation of birth rates, and (3) describing the process that would occur in a community during the period before a dislocation. However, the county-level approach does not describe the effects of WARN notices appearing at different time points in pregnancy. That question involves some complications in defining the trimesters of pregnancy, which the county-level approach avoids. However, the question is still important, so I also analyze the natality micro-data by estimating models where birth outcomes depend on variables that represent exposure to notices during each trimester of pregnancy.

2.3. Data

This study examines births from 1999 to 2008 using a county-month panel data set that includes all 422 counties in Alabama, New York, Texas, and Washington. These states were selected based on the histories of WARN notices available from state-level agencies. I traded off geographic coverage against panel length while forming a balanced panel. The core of the dataset is composed of 7,113,083 births and 2,626 WARN notices. Extended summary statistics for all data are available in the appendix.

TABLE 2.1: Summary of dataset

	Births	% Births	Counties	Advance notices
Alabama	546,870	7.7	67	282
New York	2,327,954	32.7	62	891
Texas	3,476,622	48.9	254	1,142
Washington	761,637	10.7	39	311
Total	7,113,083	100.0	422	2,626

2.3.1. Layoffs and plant closings

2.3.1.1. Description

The United States Congress enacted the Worker Adjustment and Retraining Notification Act of 1988 in order to help individuals and communities anticipate worker dislocations. Under this law, private employers must notify workers and authorities (the chief elected official in the local government where the employer is located) at least 60 days in advance of a plant closing or mass layoff. Closings and layoffs trigger the law only if they meet certain criteria, which mainly specify thresholds for the number of workers involved. If a worksite with at least 50 workers will close, then the employer must provide notice. Similarly, the law applies to (1) layoffs of 500 or more workers and (2) layoffs of 50–499 workers when they constitute at least 33 percent of a site's workforce.[5] When one of these conditions applies, the employer must give detailed, written notices to (1) the affected workers, (2) the workers' representatives, (3) the local government, and (4) the state's dislocated worker unit (DWU). Upon receipt of a notice, the DWU's Rapid Response team contacts workers and the employer to arrange meetings and provide services to the affected workers. Typical services include guidance on unemployment insurance, career counseling, and job search assistance.

Several DWUs have provided electronic records of WARN notices for this study. Each notice specifies several key pieces of information: (1) the employer's name and address, (2) the number of

[5]For a complete description of the criteria in the Act, see the guide published by the Employment and Training Administration (2003).

workers affected, (3) the date on which the DWU was notified,[6] and (4) the date on which worker dislocations were to occur or begin. This combination of details allows me to identify distressing events that are locally salient, something not possible with other data. For example, the BLS Mass Layoff Statistics program lacks information about the amount of forewarning given by employers and provides coarser temporal and geographic resolution.

Based on the difference between the notice date and dislocation date, I categorize each notice into one of two types: *Advance notices* (ANs) provide at least 60 days' notice, while *short notices* (SNs) provide less. *The analysis here focuses on advance notices because they indicate situations where an anticipatory effect can be isolated. In the case of short notices it is difficult to isolate births that could be affected by an anticipatory effect but* not *by the job losses themselves.* The next paragraphs describe the two types of notices and argue that they are substantially similar except for the fact that advance notices provide much greater forewarning about an impending dislocation and appear to receive greater coverage in the media.

Advance notices make up about 40 percent of the total, a finding like those of U.S. government studies (U.S. General Accounting Office 1993, 2003). A small portion of notices, about 5 percent, arrived more than 30 days after the dislocation or more than 120 days early. These were regarded as erroneous or atypical situations and discarded. Nevertheless, the inclusion of these notices has no substantial effect on the results. Advance notices provide on average 70 days of forewarning, while short notices provide on average 31. Figure 2.1 shows a histogram of the distribution of amounts of forewarning given in the notices.

There are several reasons why notices giving less than 60 days' notice can appear. Employers are suspected to violate the WARN Act's requirements because of confusion, weak enforcement, and small penalties (U.S. General Accounting Office 1993). No government agency is directly responsible for enforcement of the Act. Following a violation, workers or local governments may file suit against employers in U.S. District Court to extract back pay. Such lawsuits can be costly

[6]The Alabama and New York records include just the date of the notification document, but the data from Washington include just the date on which the DWU received the notification. Only Texas includes both dates. I use the date of receipt in the analysis, but the results are not substantially changed by using the other date.

and uncertain because the criteria in the act are complicated and courts have interpreted the WARN Act inconsistently. These factors also contribute to underreporting. It is estimated that employers fail to file a notice for one-half to two-thirds of events that should be reported (U.S. General Accounting Office 1993, 2003). However, the WARN Act also encourages employers to file notices even if they are not obligated to do so, and these notices are not distinguished in the data. Finally, I contacted DWU staff to ask about differences between the two types. They denied that there is any clear difference (Faraone 2012; Jordan 2012).

A key feature of the notices is that the two types implicitly attribute different amounts of bad news to the months *before dislocations*. The notice date represents an important point in the process that disperses information about the impending job losses. However, the notice date should not be viewed as the single point at which full information about the job losses is instantaneously revealed to all. First, the notice date is generally the latest date at which workers would learn of the planned job losses. Employers want to break the news before Rapid Response makes contact with the workers. The U.S. General Accounting Office (1993) reports that workers generally receive somewhat more forewarning than DWUs. Second, workers also receive informal signals of the impending notice filing and dislocation. Previous studies of worker notification laws have considered this “spillover” problem (Friesen 1997; Jones and Kuhn 1995). Finally, the employer must also notify the community’s authorities, and local media often report WARN notices. Some state governments, including all four in the study, publish WARN notices online as they receive them, which provides another way for people to learn about them. Thus, when a dislocation follows an advance notice rather than a short one, the news of the impending job losses will have had a relatively long time to spread throughout the community and exert an effect.

An important factor that determines the effect of a notice is how much attention it receives. A notice that is reported by the media will have a greater potential to generate stress than a notice that is not reported and remains little known. The WARN notice data do not report whether a notice received media coverage. To determine the amount of coverage, a random sample of 220 notices was hand-coded according to whether media coverage relating to the notice could be found using a search engine.[7] Media coverage typically announces that the worker dislocation has just occurred or reports, shortly before the notice, that a company is preparing to lay off workers or shut down. The results of this analysis appear in Figure 2.1. Overall 45 percent of the notices have media coverage. The notices are grouped into bins (of width 1 day), and the points represent the proportion of notices in each bin having media coverage. The smoothed (LOWESS) plot indicates that notices with a medium amount of forewarning receive the least amount of coverage. The probability of coverage increases sharply as the amount of warning approaches 60 days, which suggests that notices with 60 days or more forewarning may be more consequential. The greater amount of news coverage could directly increase the effects of these notices by making them more widely known. Alternatively, the greater amount of news coverage may simply reflect a belief that such notices are likely to have a greater impact on economic activity.

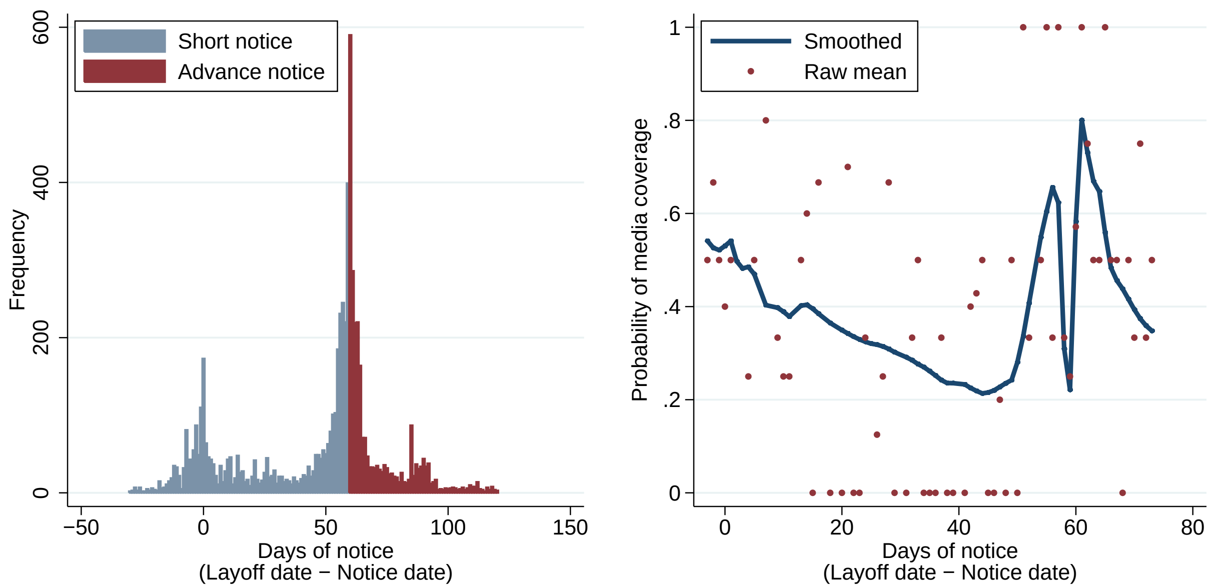

FIGURE 2.1: Distributions of days of advance notice and media coverage

A dislocation's potential impact depends in part on the number of workers affected and the size

[7]This method is imperfect but still reveals an important feature of the data. The random sample was stratified according to the amount of notice given, and the statistics were weighted by the actual sample frequencies. This method was chosen so that a sufficient amount of notices in the middle range would be included in the sample. The coding was done by searching for the company name along with the year of the notice and the location in Google News search. A notice was classified as having media coverage if any articles were found that directly mentioned the notice or that discussed plans for layoffs or shut down around the time of the notice. The coding was done blind to the amount of forewarning and with the notices sorted in a random order.

of the community. Thus, I operationalize this potential as the ratio of (a) the number of workers affected to (b) the population of working-age people in the county of the employer. In the set of county-month cells with at least one AN dislocation, the mean (median) potential is 0.124 (0.024) percent.[8]

WARN notices are rare in the sense that about 6 percent of county-month cells contain one. However, 67 percent of counties have at least one at some point in time. The second percentage would be above 90 if not for Texas, where 50 percent of the counties have no notices. The WARN Act applies only to employers with at least 100 employees in total, regardless of how many workers lose employment at a single worksite operated by the employer, and there may be few or no such large employers in the many small counties of Texas. Figure 2.2 shows that WARN notices are highly dispersed over the study period. No single year has more than 13 percent of the notices nor less than 7 percent.

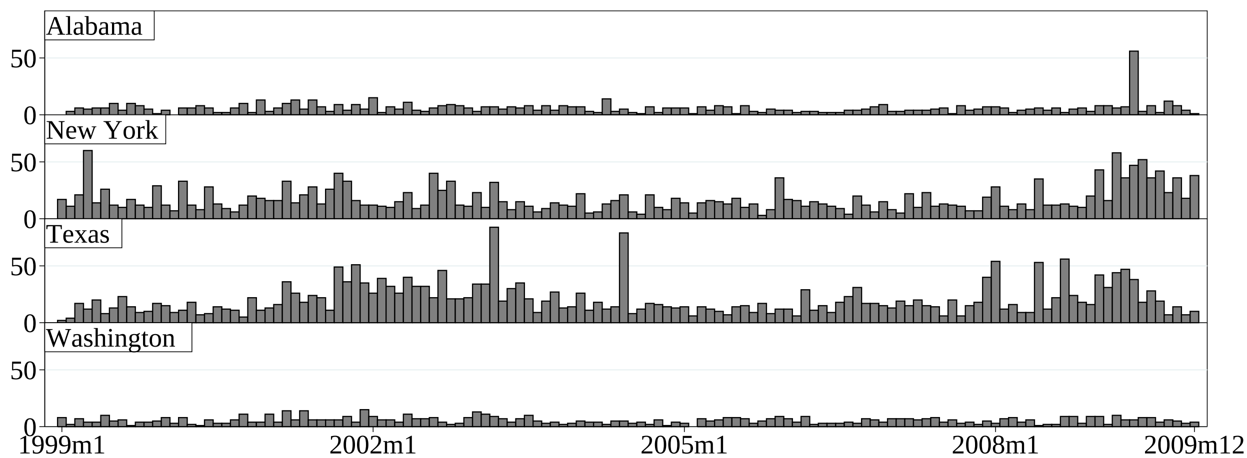

FIGURE 2.2: Monthly frequency of WARN notices by state, 1999–2009

2.3.2. Natality data

The National Center for Health Statistics at the Centers for Disease Control (CDC) has provided a confidential version of the Vital Statistics natality data that identifies each mother's county of

[8]AN and SN dislocations have very similar distributions of potential. The corresponding mean (median) for SN dislocations is 0.125 (0.026) percent. The first and third quartiles are also nearly equal across the two types. The mean number of workers affected by an AN notice is 114, while SN notices affect 117 on average.

residence. Practically all births in the United States appear in these data. The available variables include the birth weight (BWT) in grams, the gestational age (GA) in weeks, the month of birth, and the mother's demographic characteristics. Births were included based on these criteria: (1) the birth was a singleton, (2) the mother resided and gave birth within the same state, and (3) the mother was 18 to 45 years of age. If a record had a missing value in the birth weight, gestational age, plurality, or location variables, then it was dropped before analysis. The mean age of mothers at the time of birth is 27.6 years. Births to Hispanic, white, black, and "other" mothers make up 33, 47, 14, and 6 percent of the sample. The means of birth weight and gestational age are 3,318 grams and 38.7 weeks.

By convention, babies born at a weight below 2,500 grams are classified as low birth weight (LBW), and births before 37 weeks are classified as preterm (PTB). I also consider age-conditional z-scores and the proportion of babies that are both LBW and PTB. The z-scores indicate how much a baby's birth weight deviates from the median birth weight given the length of gestation, so they are sensitive to changes in fetal growth but not changes in gestational age. The z-scores are calculated using the tables and reference population provided by Oken et al. (2003). Z-scores are only defined for gestational ages between 22 and 44 weeks, so births outside this range, which make up less than one percent of the sample, are dropped when z-scores are analyzed.

2.3.3. Other data

The Bureau of Labor Statistics provides unemployment data at the county-by-month level through the Local Area Unemployment Statistics program. Finally, birth rates and working-age populations are calculated using intercensal population estimates from the U.S. Census and National Cancer Institute/SEER. The Census estimates July populations for each county, so linear interpolation provides figures for the remaining months.

2.4. Empirical model

2.4.1. County-level model

In order to estimate how birth outcomes respond to dislocations, I use a county fixed effects model that incorporates the following key features. First, each month's outcomes are allowed to depend on AN dislocations up to 6 months in the future. For a depiction of this structure consider the timeline in Table 2.2. This table gives an example of how the key variables relate to the timing of a notice and layoff. This example depicts a typical case in the sense that within a given county most layoffs will be isolated in time. In this example, a layoff of 250 workers occurs in October, the last column. However, the notice was given in July, and the period between the notice and the dislocation is shaded. Note that the row "# jobs lost in month t" has the value 250 in October, and the row "# layoffs announced in month t" has the value 250 in July. In addition, note that in July the distributed lead variable "# number jobs lost in month $t+3$" has the value 250. This variable along with the other distributed leads is the key to the anticipation effect because it allows outcomes in the earlier months to depend on the impending job losses. *The regression coefficients on these distributed leads quantify the deviations in birth outcomes occurring during the 6 months before the job loss.* For example, if the coefficient on the three-month lead is -20 grams, then it should be interpreted to mean that births occurring about 3 months before a unit-sized layoff are expected to be 20 grams lower than if there was no layoff (all else equal). Importantly, the model allows birth outcomes to respond to a dislocation up to six months before the job losses actually occur. As discussed above, the small number of WARN notices providing more than 120 days of advance notice were dropped from the data set. To demonstrate how this relates to the model, suppose that a notice was filed in June for a layoff occurring 120 days (4 months) later in October. It is possible that an anticipatory response appears in May, 5 months before the dislocation. This could occur if the firm made a pre-announcement in May stating that it was planning to formally announce layoffs in the next few weeks. By including the full set of 6 months of distributed leads of job loss variables, the model allows for such a situation. In principle, any number of distributed leads could

TABLE 2.2: Example timeline of WARN notice in July of 250 layoffs in October

Month	Apr	May	June	July	Aug	Sept	Oct
Months before layoff	6	5	4	3	2	1	0
Months after notice	-3	-2	-1	0	1	2	3
# layoffs announced in month t	.	.	.	250	.	.	.
# jobs lost in month t	.	.	.	.	.	.	250
# jobs lost in month $t+1$	.	.	.	.	.	250	.
# jobs lost in month $t+2$	.	.	.	.	250	.	.
# jobs lost in month $t+3$	.	.	.	250	.	.	.
# jobs lost in month $t+4$	.	.	250	.	.	.	.
# jobs lost in month $t+5$	.	250	.	.	.	.	.
# jobs lost in month $t+6$	250	.	.	.	.	.	.

be included, but 6 months is used because it is expected to encompass almost all anticipatory effects related to job losses. The second key feature is that dislocations enter the model as a proportion of the working-age population in the county.

The estimated equation is

$$Y_{i,t} = \sum_{\tau=0}^{6} \beta_\tau (\text{AN})_{i,(t+\tau)} + \mathbf{Z}_{i,t}\boldsymbol{\delta} + \epsilon_{i,t},$$

where for each county i and month t

- $Y_{i,t}$ is the mean of a birth outcome,
- $(\text{AN})_{i,t}$ is the proportion of the working-age population dislocated under an advance notice.[9] In the example above, if $t =$July, then

$$(\text{AN})_{i,t+3} = \frac{250}{\text{working-age population of county } i}$$

(up to a scaling factor). If the corresponding coefficient β_3 is negative, then the presence of job losses in October (month $t+3$) will have a negative effect on the mean birth outcome in July (month t). This relationship is precisely the "anticipatory effect" whereby women respond to impending job loss events. Since layoffs are rare, the $(\text{AN})_{i,t}$ are sparse. That is, it is unlikely for more than one of the $(\text{AN})_{i,t}$ to be non-zero in any given case (i,t).

[9]Summary statistics for the variable (AN) are printed in appendix Table 2.9. This variable corresponds to the row in the above table labelled "# jobs lost in month t" except that the population normalization is applied. For example, the mean value taken over county-month cells is 0.124 percent.

- $\mathbf{Z}_{i,t}$ is a vector that potentially includes the means of characteristics of women giving birth and county-specific quadratic time trends along with indicators for county of birth, year of birth, and calendar month of birth.[10]

Each observation is weighted by the number of births to increase efficiency, and standard errors are adjusted for clustering at the county level. Some specifications also include a window of unemployment rates, which is detailed in the results section. The dislocation effect variables are scaled so as to represent the effect of a 95th percentile layoff. The scaling factor was obtained by examining the 6% of county-month cells *in which any worker dislocations resulted from WARN notices*. From these cells, the 95th percentile of the dislocation potential (the percentage of the working-age population dislocated by a WARN notice) was extracted giving the number 0.0067. Note that since the percentile is determined by only a small subset of the cells, the use of the 95th percentile does *not* mean that a unit-sized or larger layoff occurs in 5% of county-month cells. During the period from 1999 through 2009, only 176 county-month cells, or about 0.32%, reach the threshold of 0.0067. Thus, the effects in this paper are scaled so as to represent very extreme dislocation events.

While it may seem counter-intuitive to use the month of the job losses as a reference point, this specification is a natural result of the identification strategy. The statistical model must be designed so as to clearly separate the effects of job loss from the anticipatory effects caused by bad news. If the model were to use the month of the notice as a reference point, the anticipatory effects could be confounded with the effects of job losses. For example, suppose the results showed that birth weight tends to be lower than normal about three months *after a notice*. There could be two explanations. One is that people are anticipating layoffs that had 3–4 months of notice. The other is that some layoffs, which had two months of notice, are generating effects one month after

[10] Mother characteristics enter as proportions of mothers in groups defined following Currie et al. (2011). The age groups are “Less than 20”, “20 to 34”, “Over 34”, and “Missing”. The race and ethnicity groups are “Hispanic”, “White”, “Black”, “Other”, and “Missing”. The educational attainment groups are “Less than high school”, “High school”, “Some college”, “College or higher”, and “Missing”. The marital status groups are “Married” and “Not married”. Finally, the categories for total birth order are 1, 2, 3, 4, 5 or more, and “Missing”. However, these maternal variables are omitted from birth rate models because they are undefined in months with no births. Smoking variables are omitted because of possible endogeneity, but their inclusion has minimal effect on the results. Birth rates are calculated as the number of births per 1,000 women aged 18–45.

the job losses—which is three months after the notice. Therefore, the county-level analysis uses the approach which sharply separates the pre- and post-job loss periods. However, the individual-level analysis (explained below) allows notices to directly enter the model, which provides the perspective of explicitly looking at responses to notices.

2.4.2. Individual-level analysis on natality micro-data

The second set of results uses birth-level micro-data to estimate the effects of exposure to notices at different points during the pregnancy. This approach presents some complications because exposure during the last trimester of pregnancy is partially determined by the length of pregnancy. A birth occurring at 40 weeks rather than 39 will have one extra week in which to accumulate exposure. This relation creates an upward (positive) bias in the effect of exposure in the third trimester even if the true effect is null or negative. Currie and Rossin-Slater (2013a) call this complication a "mechanical correlation" between the exposure and outcome, and they propose instrumenting for exposure using the potentially counterfactual exposure that would have resulted if the birth occurred at 39 weeks. That is, the instrument represents exposure as-if the pregnancy lasted 39 weeks. The instrument can be viewed as the result of making a small but critical adjustment to the endogenous exposure variable. I apply the "full-term instrument" method in this analysis. The individual-level results using this approach are consistent with results from the county-level data.

Each pregnancy is first divided into three trimesters corresponding to 13-week periods relative to conception. The date of conception is estimated by taking the midpoint of the birth month and subtracting the gestational age. Two weeks are added because conception normally occurs two weeks after the date of the last normal menstrual period, which is used to compute gestational age. Based on this conception date the trimesters of pregnancy are defined. In each trimester I compute the total amount of workers dislocated and affected by WARN notices by summing all those within the county of residence of the mother and falling within the appropriate time period. The third trimester ends at birth and may be longer than 13 weeks. The instrument is constructed using the same method except under the assumption that the birth occurred at 39 weeks gestational age. As a result, for births occurring at 39 weeks the instrument and the actual exposure are identical.

In addition to the three trimesters of pregnancy I define three additional trimesters for use in the analysis. Trimester 0 is the 13-week window just before conception. For births that occur at 39 weeks, trimester 4 is the 13 weeks after birth. Otherwise, trimester 4 is the window following trimester 3 up to 52 weeks after conception. [11] Finally, trimester 5 is weeks 53–65. This last period is included in the spirit of a placebo test, meaning it is expected to have relatively little effect.[12]

2.4.3. State-to-state heterogeneity of effects

Estimates using only data from individual states reveal heterogeneity in the effects of WARN notices. The two large states, New York and Texas, show the strongest evidence of anticipatory effects of notices on birth outcomes. However, the smaller states show more equivocal results. The estimates from Washington suggest moderate effects on birth outcomes, while Alabama shows no evidence of a decrease. State-by-state results from micro-data are in Table 2.11.

This heterogeneity demands some conceptual and methodological consideration before the analysis can proceed. Differences in anticipatory effects might arise from variation in regional labor market features or policies, locals' prior beliefs about job losses, spatial relationships among employers and employees, or numerous other factors. Alabama is the only state that shows no evidence of effects, and it is an outlier from the other states in several ways. First, the mean birth weight is substantially lower than in the other states. For example, it is 86 grams lower than the mean of New York. This difference dwarfs the estimated effects in this paper, which are already scaled to represent a very large loss of jobs. Second, Alabama is substantially more rural than the other three states.[13] Third, relative to these small counties, the worker dislocations in Alabama tend

[11]Trimester 4 ideally is the one following birth, but it will in reality slightly overlap with pregnancy in some cases because only the month of birth is known. In addition, because the exact timing at which people learn of an impending dislocation is not known, the inclusion of trimester 4 allows testing for anticipation of events occurring just after birth.

[12]Note that constructing the regressors requires 4 trimesters of notice data before the birth, so it is not possible to use births from 1999. Therefore, the individual-level analysis focuses on 2000–2008.

[13]The distribution in Alabama stands out with much fewer births in large metropolitan areas compared to the other states. However, the effects of notices does not appear to vary systematically between rural and urban areas. These results are available from the author.

to be large relative to the local population, although they are not unusually large in absolute terms. In the other three states about 0.1 percent of the working age population is affected by a WARN notice in an average month. However in Alabama, this value is over 0.3 percent, which suggests that the effects of dislocations are likely to be dispersed outside the county. Future research should investigate how people in different environments vary in their reaction to economic news.

Heterogeneity of treatment effects presents some complications when estimating average treatment effects. Angrist (1998) shows that when treatment effects are heterogeneous across groups an OLS estimator which allows just one treatment effect will not in general estimate the average treatment effect. Instead, the estimator will estimate a weighted average of the group-specific treatment effects where each weight is proportional to the group-specific variance of the treatment variable. The estimator then depends on the marginal distribution of the treatment variable within each group. In this case the estimator overweights Alabama because the notice variables have high variance relative to the other states. Morgan and Winship (2007) propose addressing this problem by a simple stratification method which estimates the group-specific treatment effects and then averages them using weights proportional to the number of observations in each group. I implement this estimator by computing one regression which allows each state to have a separate set of effects. A single treatment effect is then estimated by averaging these four state-specific effects using the proportion of births in each state as weights. This approach is applied to both the county-level estimates and the individual-level estimates. Estimates from models that estimate single, unstratified effects are available in appendix Table 2.12 along with estimates based on raw counts of workers affected by notices.

2.5. Results

2.5.1. County-level results for birth weight and gestational age

Table 2.3 presents the main results for birth weight and gestational age. The estimates show that both variables selectively decrease in anticipation of dislocations. During the period four months to one month before dislocations, the mean birth weight is depressed by 15–20 grams and the mean gestational age by roughly one half to one day.

TABLE 2.3: Estimated anticipation effects on birth weight and gestational age

	Birth weight (grams)				Gestational age (days)			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Timing of birth								
(relative to job losses)								
6 mos. before	0.26	-1.89	-3.55	-3.54	-0.17	-0.19	-0.04	-0.06
	(4.77)	(4.45)	(5.12)	(5.17)	(0.21)	(0.21)	(0.22)	(0.22)
5 mos. before	0.83	-2.73	-4.74	-4.98	-0.18	-0.23	-0.10	-0.10
	(6.52)	(5.75)	(5.86)	(5.87)	(0.21)	(0.20)	(0.23)	(0.23)
4 mos. before	-17.32**	-18.24**	-18.81**	-19.15**	-0.86**	-0.93**	-0.77**	-0.79**
	(6.06)	(5.89)	(5.93)	(5.91)	(0.20)	(0.21)	(0.20)	(0.21)
3 mos. before	-16.71*	-18.89*	-20.18**	-20.81**	-0.29	-0.33	-0.18	-0.21
	(7.58)	(7.48)	(7.45)	(7.43)	(0.22)	(0.22)	(0.20)	(0.19)
2 mos. before	-10.66+	-14.63**	-16.00**	-16.94**	-0.57*	-0.61*	-0.48*	-0.53*
	(5.88)	(5.44)	(5.61)	(5.65)	(0.24)	(0.24)	(0.22)	(0.22)
1 mo. before	-11.83+	-14.30*	-16.07*	-17.27*	-0.23	-0.25	-0.12	-0.16
	(7.06)	(6.87)	(6.81)	(6.75)	(0.27)	(0.28)	(0.24)	(0.24)
Same month	4.46	1.42	1.05	-0.08	-0.04	-0.09	0.08	0.06
	(7.55)	(7.62)	(7.38)	(7.59)	(0.26)	(0.28)	(0.26)	(0.26)
County, yr., mo. FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Moms' characteristics	.	Yes	Yes	Yes	.	Yes	Yes	Yes
County quad. trends	.	.	Yes	Yes	.	.	Yes	Yes
Unemp. rates	.	.	.	Yes	.	.	.	Yes
Cells	49,123	49,123	49,123	49,123	49,123	49,123	49,123	49,123
Adj. R-sq.	0.655	0.665	0.674	0.674	0.536	0.539	0.561	0.561

Notes. Average treatment effects of a 95$^{\text{th}}$ percentile layoff printed with standard errors in parentheses. FE=Fixed effects. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Estimates from four specifications allow us to consider the potential influence of omitted variables. The most basic specification includes just county fixed effects and state-specific year and calendar month fixed effects. This relatively simple model is sufficient to show the basic form of the anticipatory response to dislocations. The addition of the mothers' demographic characteristics increases the magnitude of the birth weight coefficients associated with advance notice and tends to increase the precision of the estimates. These additional covariates also slightly increase the magnitude of the gestational age coefficients. This result suggests that the effects are not driven by changes in the demographic composition of mothers.

Since the study period spans ten years, one might be concerned that the counties underwent heterogeneous drift in some unobserved factors related to birth outcomes and job loss activity, which could bias the estimates. This problem appears unlikely because the birth outcomes show transient responses. Nevertheless, the third specification includes county-specific quadratic time trends. These trends should capture the effects of unobserved influences that vary in a smooth way within each county. This change adds over 800 parameters to the model, but has little effect on the estimated coefficients and standard errors. The birth weight effects slightly increase in magnitude, while the gestational age effects slightly decrease in magnitude.

Finally, it might also be suspected that dislocation activity simply reflects more general economic conditions. The state-by-year fixed effects in the basic model should control for economic activity varying at the state level. However, county-level heterogeneity might still be a source of bias. The quadratic trends might not properly account for varying economic activity since two recessions occured during the study period. So, the last specification includes a 19-month window of county-level unemployment rates. Twelve months of lags are included to capture effects occuring around the time of conception and during pregnancy. Six months of leads are included to allow for pregnant women to react to expectations about future economic activity. Inclusion of unemployment rates slightly increases the magnitude of the birth weight and gestational age effects, suggesting that local fluctations in general economic activity are not a serious problem.

2.5.2. Effects over broader geographic regions

The effects of job losses and their announcements might extend beyond the county where they occur. This subsection shows that such effects are unlikely to be large, which justifies the paper's focus on within-county effects. To check for the possibility of such inter-county spillover effects the model is augmented with variables that represent, for each county, the job losses in a group of nearby counties. The model retains variables representing same-county notices. Therefore, the model allows us to answer the question, 'How do birth outcomes in a particular county i respond to increasing the amount of WARN notices in i's group (holding constant notices within i itself)?'

For the moment, assume that all counties have been partitioned into contiguous groups. The variables that represent inter-county influences are constructed analogously to those that represent within-county influences and use the same scaling factor. The variable representing the inter-county influences on i is

$$\frac{\text{\# of workers being laid off by advance notices in } i\text{'s group (but not in } i \text{ itself)}}{\text{the working-age population of the group (excluding } i\text{'s population)}}.$$

Just like the baseline specification, these inter-county variables enter the model as distributed leads.

Four methods of grouping counties are analyzed: Metro- and micro-politan areas, commuter zones (as defined by the US Department of Agriculture's Economic Research Service), component economic areas, and economic areas. The last two groupings are defined by the U.S. Bureau of Economic Analysis. Some counties are not part of a metro- or micro-politan area, so these counties have zeros for all inter-county influence variables.

The results of this approach appear in Table 2.4. Same-county effects from the previous table are reprinted in the first column for the purpose of comparison. In contrast to the same-county estimates, which show a consistent, statistically significant negative effect, the inter-county estimates have widely varying signs and do not reach significance at conventional levels. Based on these results the remainder of the analysis will consider only within-county effects.

2.5.3. Dynamic features of the birth weight response

The four-month period before dislocations shows a fairly stable depression in birth weights, but this stability hides interesting dynamics. In particular, the initial response is marked by a precipitation of babies born too early and too small. Later parts of the response show decreased fetal growth and possibly some selection. With these two mechanisms in mind, I now consider several additional variables.

The main physiological contributors to birth weight are the length of gestation and the rate of intrauterine growth. These contributions can be partially disentangled by considering the joint distribution of birth weight and gestational age. Decreased birth weights caused by shorter gestation should be reflected in the *proportion of babies born both preterm and with low weight*. However,

TABLE 2.4: Inter-county effects of dislocations over wider geographic areas

	Same county	Surrounding area (inter-county effects only) Metro area	Commuter zone	Component economic area	Economic area
Timing of birth (relative to job losses)					
6 mos. before	-3.55	-4.87	-3.63	1.13	12.33
	(5.12)	(10.01)	(8.15)	(10.80)	(9.39)
5 mos. before	-4.74	2.33	-1.04	10.07	7.12
	(5.86)	(7.11)	(7.99)	(10.65)	(18.40)
4 mos. before	-18.81**	19.73+	-6.50	20.84	8.47
	(5.93)	(11.36)	(9.35)	(16.86)	(17.02)
3 mos. before	-20.18**	-0.68	-1.69	-3.41	-15.19
	(7.45)	(10.55)	(10.70)	(13.96)	(14.25)
2 mos. before	-16.00**	-16.16	-19.45	-12.86	-23.00
	(5.61)	(15.62)	(12.57)	(18.97)	(21.07)
1 mo. before	-16.07*	-0.87	-5.95	-11.74	15.74
	(6.81)	(10.31)	(9.84)	(11.69)	(15.08)
Same month	1.05	-9.09	-11.96	-1.78	13.23
	(7.38)	(6.43)	(9.75)	(9.43)	(13.54)
# geographic units	422	323	108	57	36

Notes. Average treatment effect of a 95$^{\text{th}}$ percentile layoff printed with standard errors (clustered at the level of geographic grouping) in parentheses. All models include county, year, and month fixed effects along with county-specific trends. Only advance notices included. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

this variable is not sensitive to a change merely in fetal growth rates. Alternatively, birth weights caused by lower growth rates can be detected in *age-conditional z-scores*, which are sensitive to changes in birth weight conditional on gestational age.

Table 2.5 displays the estimated effects of a dislocation on the (1) mean birth weight, (2) birth rate, (3) proportion low birth weight, (4) proportion both low birth weight and preterm, and (5) mean age-conditional z-score of birth weight. Several results suggest that the initial decrease in birth weight at month four can be attributed to reduced gestational age. First, at this point the proportion LBW is increased by one percentage point, or 16 percent of the overall rate, and the proportion LBW & PTB increases by 0.76 percentage points, or 20 percent of the overall rate, revealing that the additional low weight births are largely also additional preterm births.[14] Second, the contemporaneous effects ("four months before") in the main results above show that the decreases in birth weight and gestational age are consistent with each other. To a first-order approximation, a 0.8 day decrease in gestational age implies a decrease of 18.9 grams, which is

[14]The LBW and PTB cutoffs are somewhat arbitrary, so it is reasonable to vary the cutoffs and recalculate the effects. That analysis is presented in Appendix Figure 2.5 and is consistent with the results in this subsection.

almost exactly the estimated change in mean birth weight.[15]

TABLE 2.5: Dynamics of the anticipatory response

	BWT		BR		LBW		LBW & PTB		Z-score	
	ATE	SE	ATE	SE	ATE	SE	ATE	SE	ATE	SE
Timing of birth (relative to job losses)										
6 mos. before	-3.55	(5.12)	-0.03	(0.07)	0.19	(0.29)	0.01	(0.19)	-0.19	(1.04)
5 mos. before	-4.74	(5.86)	-0.09	(0.07)	0.06	(0.23)	-0.01	(0.20)	-1.05	(1.09)
4 mos. before	-18.81^{**}	(5.93)	-0.01	(0.06)	1.01^{**}	(0.28)	0.76^{**}	(0.23)	-0.76	(1.06)
3 mos. before	-20.18^{**}	(7.45)	-0.14^{+}	(0.07)	0.88^{**}	(0.28)	0.34^{+}	(0.18)	-3.61^{**}	(1.39)
2 mos. before	-16.00^{**}	(5.61)	0.01	(0.05)	0.20	(0.29)	0.01	(0.21)	-2.02^{+}	(1.09)
1 mo. before	-16.07^{*}	(6.81)	-0.22^{**}	(0.07)	0.28	(0.28)	0.16	(0.19)	-3.24^{**}	(1.13)
Mo. of layoff	1.05	(7.38)	-0.09	(0.07)	-0.19	(0.35)	-0.24	(0.27)	-0.52	(1.26)
Cells	49,123		50,640		49,123		49,123		49,108	
Adj. R-sq.	0.674		0.840		0.254		0.177		0.572	

Notes. Average treatment effect of a 95$^{\text{th}}$ percentile layoff printed with standard errors in parentheses. Outcome models include county, year, and month fixed effects along with county-specific trends. BR model is a Poisson model with coefficients reported. BWT=Birth weight (grams). BR=Monthly birth rate: Births per 1,000 women of child bearing age. LBW=Proportion low birth weight $\times$ 100. PTB=Proportion preterm birth $\times$ 100. Z-score represents BWT conditional on gestational age and is multiplied by 100 for display. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Closer to the dislocation month, there appear changes in intrauterine growth. The effect on the proportion LBW progressively diminishes from month four onwards while the decrease in mean birth weight is about 16 grams. Months three through one show rescaled z-scores decreased by 2.02–3.61, suggesting that retarded intrauterine growth is reducing birth weights. Converting the z-score effect to grams gives a decrease of 11–17 grams.[16] This result indicates that decreased growth, due to physiological stress responses or increased levels of unhealthful behaviour by pregnant women, can account for most or all of the estimated change in birth weight.

One concern is that average birth outcomes could change if women migrated out of areas where job losses are about to occur. Immigration is also possible but less likely in this context. Estimates of changes in birth rates before job losses suggest that selection is unlikely to account for the estimated effects on birth outcomes. Table 2.5 also shows changes in the monthly birth rate using a model like those used to estimate changes in average birth outcomes. These results show that the

[15] Oken et al.'s (2003) supplemental tables indicate that a median baby born at 38 weeks should weigh 165 grams more than one born at 37 weeks. The calculation $(3,301 - 3,136) \times \frac{1}{7} \times 0.8 \approx 18.9$ suggests that around this point in pregnancy a decrease of 0.8 days in GA should result in a 18.9 gram decrease in birth weight.

[16] For a birth at 39 weeks, Oken et al.'s (2003) supplemental tables associate a loss of 11 grams with a decrease of 0.03 in z-score. Alternatively, the standard deviation of birth weight in the data is 562 grams. So, a change of 0.03 standard deviations corresponds to 17 grams.

changes in birth outcomes do not coincide with changes in the birth rate. Recall that birth weight is consistently depressed from 4 months to 1 month before the job loss event. However, the birth rate is decreased only in months 3 and 1 and by less than 5%. This discrepancy in timing indicates that a simple pattern of selection cannot account for the decrease in birth weight.[17]

2.5.4. Individual-level results using birth micro-data

The last analysis approaches the problem differently by modeling individual-level birth outcomes in the manner commonly used to study prenatal exposures in the economics literature. Linear models are estimated which allow the expected outcome of *each* birth to depend on exposure—notices occurring in the mother's county—during each trimester of the pregnancy. This approach provides the advantage of explicitly allowing the effect of an exposure to depend on when in pregnancy it occurred. However, this approach also complicates the identification of the effects of notices themselves because a given pregnancy is often exposed to both the notice and the subsequent pregnancy. Nevertheless, these new results are consistent with anticipatory effects. Exposure to notices late in pregnancy is associated with significant decreases in both birth weight and gestational age.

The estimated individual-level effects of exposure to WARN notices are displayed in Table 2.6. Like the previous set of results these include only advance notices.[18] Each IV column shows estimates from a model using the full-term instruments, while the OLS columns show reduced-form estimates. The notice exposure variables are scaled so that they are comparable to the county-level results. That is, a unit of exposure in a particular trimester corresponds to WARN notices directly affecting approximately 0.67% of the county's working-age population during that trimester. In the birth weight columns, the negative coefficients on third and fourth trimester exposure indicate that the occurrence of notices in those periods—just around birth—is associated with a strong decrease in birth weight. The point estimates for these two trimesters are around -30 and -21 grams,

[17] Additional analysis that consider subgroups of mothers and mothers' characteristics also fail to show clear evidence of selection. These results are available from the author.

[18] The analysis in this section gives similar results if all notices are included. However, the estimates are smaller in magnitude because the short notices "dilute" the effect. These results are available from the author.

respectively. These estimates are somewhat more negative than the county-level results, which are about -20 grams. This difference likely occurs because the county-level effects isolate births that are before layoffs, but the individual-level results include many births occurring after a layoff. For example, the third trimester coefficients may be picking up additional negative effects of layoffs that are both announced and carried out during the third trimester. However, the differences between the county-level and individual-level models are not especially large compared to the standard errors of about 6. Additionally, this same concern applies to all of the trimester coefficients, which may be contributing to a small downward bias across all trimesters. Taking the evidence all together, the effects of notices are likely closer to 20 grams.

These results provide additional evidence that birth outcomes deteriorate in anticipation of job losses. The effects are concentrated in the third and fourth trimesters rather than earlier in pregnancy. If the effects were actually driven by the consequences of job losses themselves, for example, lost income or migration, then we should expect to see the strongest effects from notices early in pregnancy. For example, if women tend to selectively migrate away from areas that are losing jobs, then the effect of selection should increase as time passes, which would result in larger effects for earlier notices rather than later ones. In addition, significant effects are estimated from notices occurring in the fourth trimester, which covers the period just following pregnancy and possibly the very end of pregnancy.[19] There is little opportunity for job losses associated with fourth trimester notices to directly affect the pregnancy. These effects are likely due to both the brief overlap between the fourth trimester and pregnancy along with anticipation of the notice itself. Finally, the fourth trimester estimates are uniformly smaller than the third trimester estimates, suggesting that the strongest effects result from notices occurring at the end of pregnancy.

[19] Separate analyses were run where job losses enter the individual-level models instead of notices. The results show that birth weight and gestational age are significantly lower among births that occur just *before a job loss event*, which also suggests anticipatory effects. These results are available upon request.

TABLE 2.6: Individual-level effects of advance notices during pregnancy

Time of exposure relative to conception	Birth weight		Gestational age		Low birth weight		Preterm birth	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Trimester 0	-7.99	-8.02	-0.59*	-0.59*	0.33	0.33	0.83+	0.83+
[1–13 weeks *before*]	(5.83)	(5.80)	(0.29)	(0.29)	(0.24)	(0.24)	(0.50)	(0.50)
Trimester 1	-10.07	-10.10	-0.47	-0.47	0.21	0.21	0.37	0.37
[weeks 1–13]	(7.92)	(7.88)	(0.30)	(0.30)	(0.34)	(0.34)	(0.50)	(0.50)
Trimester 2	-6.94	-6.93	-0.78**	-0.78**	0.29+	0.29+	0.59	0.59
[weeks 14–26]	(4.73)	(4.70)	(0.28)	(0.28)	(0.16)	(0.16)	(0.39)	(0.39)
Trimester 3	-29.62**	-30.32**	-1.16**	-1.17**	0.92**	0.94**	1.29**	1.30*
[weeks 27–39]	(5.70)	(6.00)	(0.36)	(0.37)	(0.21)	(0.23)	(0.49)	(0.51)
Trimester 4	-21.47**	-21.07**	-1.08**	-1.07**	0.68**	0.67**	1.04**	1.02*
[weeks 40–52]	(4.35)	(4.51)	(0.26)	(0.26)	(0.17)	(0.18)	(0.39)	(0.40)
Trimester 5	-4.62	-4.62	-0.53*	-0.53*	0.28	0.28+	0.71*	0.71*
[weeks 53–65]	(4.30)	(4.27)	(0.22)	(0.22)	(0.17)	(0.17)	(0.35)	(0.35)

Notes. Estimates from micro-data. OLS column shows reduced-form estimates. IV column shows instrumental variables estimates. Coefficients for LBW and PTB models are multiplied by 100 so as to represent percentage point effects. All models have 6,332,304 observations. Average treatment effect of a 95$^{\text{th}}$ percentile layoff printed with standard errors (clustered by county) in parentheses. All models include county, year, and month fixed effects along with mother covariates. Statistical significance symbols: $^{+}\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$.

2.6. Conclusion

Analyzed in variety of ways, the data reveal that birth outcomes deteriorate in anticipation of job loss events that are announced with substantial forewarning. That is, several months before mass layoffs and plant closings, birth weights transiently decrease by 15–20 grams within the affected community. Results from individual-level data show that birth outcomes are affected by announcements of worker dislocations made late in pregnancy or around the time of birth as well as by worker dislocations occurring just after birth. These effects are largely restricted to exposures that occur late in pregnancy rather than early in pregnancy. The results are robust to the instrumental variables approach of Currie and Rossin-Slater (2013b). Thus, both approaches to the data indicate significant anticipatory effects of job losses on birth outcomes in the local community.

These results suggest that the arrival of commonplace bad news about the local economy can generate a wave of stress or unhealthful coping behaviors that in turn decrease birth weights. In addition, the response of birth weights has an intuitive dynamic. The curtailment of pregnancies can affect birth weights abruptly, while changes in growth rates require time to integrate up to a significant effect. This pattern appears in the results. The initial response to bad news appears to be babies born too early and too small, while later decreases are associated mostly with slower intrauterine growth. Nevertheless further research should probe the mechanisms related to the anticipation of negative economic events. This study cannot disentangle direct physiological responses to stress from behavioral responses that might also generate deterioration of birth outcomes. Future research should look at data with fine-grained temporal resolution to determine exactly when and how behavior and physiology begin to anticipate shocks. Mobile and wearable technology or social media may provide an especially useful source of such data (see, e.g., Antenucci et al. 2014). In general, one important matter is how disclosure policy, media coverage, and social networks influence the anticipatory response to economic events.

FIGURE 2.3: Birth weight decreases in context with other studies

The results in this study are similar in magnitude to those reported in studies of prenatal stress and destructive events. This similarity may seem surprising, but recall that the effects in this study are scaled to represent very extreme job loss events. The unconditional probability of such a job loss event occurring within the county-month sample is 0.0032. Figure 2.3 displays reported effects on birth outcomes from a wide variety of studies. The effect attributed to the current study was obtained by averaging the county-level anticipatory effects 1–4 months before a dislocation event. An important takeaway from this figure is that the effects of even fairly extreme stressors, such as terrorist attacks, tend to be small, typically in the single- or low double-digits. These effects are quite small compared to the effect of smoking, race-related differences, the difference between Alabama and New York, or even the decrease in the mean U.S. birth weight since the 1990s—which is largely unexplained (Donahue et al. 2010). Of particular interest are the findings of Lindo (2011), which show that babies fathered by men who have lost employment are 142 grams lighter. This treatment effect is probably the largest one attributed to a specific event but is likely to include substantial income effects along with stress and other mechanisms. If one supposes that stress makes up a small but significant fraction of that decrease, then the present results on layoff notices would require that a fairly large portion of a county's population be stressed. Since most layoff events dislocate only a very small portion of the local population, it is likely that many people are indirectly affected by dislocations.

Although the mean effects on birth weight and gestational age associated with this study and others on extreme events are fairly small, the risk of falling below critical thresholds can be substantial. For example, the individual-level results above indicate that large layoffs can increase the risk of preterm birth by 10 percent. Future research might try to determine who is most at risk of a strong negative reaction to bad news. Finally, policy makers might consider the possibility of offering health assessments or counselling to workers facing imminent job loss or others who are at significant risk.

2.7. Appendix of chapter 2

2.7.1. Data description

TABLE 2.7: Natality data summary statistics

	Alabama	New York	Texas	Washington	All
Mother's age (years)	26.27	28.94	26.85	27.99	27.61
	(5.56)	(5.96)	(5.76)	(5.83)	(5.90)
Married	0.664	0.628	0.668	0.711	0.659
	(0.47)	(0.48)	(0.47)	(0.45)	(0.47)
Hispanic	0.0565	0.224	0.479	0.165	0.330
	(0.23)	(0.42)	(0.50)	(0.37)	(0.47)
Non-Hispanic white	0.629	0.501	0.372	0.663	0.465
	(0.48)	(0.50)	(0.48)	(0.47)	(0.50)
Non-Hispanic black	0.300	0.173	0.108	0.0401	0.137
	(0.46)	(0.38)	(0.31)	(0.20)	(0.34)
Non-Hispanic other	0.0143	0.0873	0.0382	0.107	0.0598
	(0.12)	(0.28)	(0.19)	(0.31)	(0.24)
Less than high school	0.193	0.180	0.281	0.160	0.228
	(0.39)	(0.38)	(0.45)	(0.37)	(0.42)
High school	0.328	0.287	0.304	0.260	0.296
	(0.47)	(0.45)	(0.46)	(0.44)	(0.46)
Some college	0.252	0.233	0.208	0.282	0.227
	(0.43)	(0.42)	(0.41)	(0.45)	(0.42)
College	0.225	0.292	0.197	0.260	0.237
	(0.42)	(0.45)	(0.40)	(0.44)	(0.43)
Female	0.489	0.488	0.489	0.487	0.488
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Birthweight (grams)	3244.0	3329.7	3298.8	3422.1	3317.9
	(588.4)	(568.9)	(553.1)	(551.6)	(562.5)
Low birthweight	0.0802	0.0602	0.0618	0.0447	0.0609
	(0.27)	(0.24)	(0.24)	(0.21)	(0.24)
Gestational age (weeks)	38.43	38.90	38.63	39.02	38.74
	(2.66)	(2.40)	(2.41)	(2.23)	(2.42)
Preterm birth	0.138	0.0980	0.116	0.0847	0.108
	(0.34)	(0.30)	(0.32)	(0.28)	(0.31)
Low weight and preterm	0.0507	0.0373	0.0382	0.0280	0.0377
	(0.22)	(0.19)	(0.19)	(0.17)	(0.19)
Births	546,870	2,327,954	3,476,622	761,637	7,113,083

Standard deviations in parentheses. Categories specified as proportions.

TABLE 2.8: Account of WARN notices, 1999–2009

	Alabama No.	%	*New York* No.	%	*Texas* No.	%	*Washington* No.	%	*Total* No.	%
Completeness										
Complete	857	99.8	2,443	99.0	2,840	99.8	760	96.9	6,900	99.2
Incomplete	2	0.2	25	1.0	6	0.2	24	3.1	57	0.8
Total	859	100.0	2,468	100.0	2,846	100.0	784	100.0	6,957	100.0
Type										
>30 days late	37	4.3	73	3.0	56	2.0	9	1.1	175	2.5
Short	488	56.8	1,428	57.9	1,576	55.4	441	56.2	3,933	56.5
Advance	282	32.8	899	36.4	1,144	40.2	324	41.3	2,649	38.1
>120 days early	52	6.1	68	2.8	70	2.5	10	1.3	200	2.9
Total	859	100.0	2,468	100.0	2,846	100.0	784	100.0	6,957	100.0
In analysis?										
Included	768	89.4	2,302	93.3	2,715	95.4	741	94.5	6,526	93.8
Excluded	91	10.6	166	6.7	131	4.6	43	5.5	431	6.2
Total	859	100.0	2,468	100.0	2,846	100.0	784	100.0	6,957	100.0

TABLE 2.9: Potential of dislocation-months as a percentage of working-age population

	Mean		Max		Q1		Median		Q3	
State	AN	SN	AN	SN	AN	SN	AN	SN	AN	SN
Alabama	0.345	0.343	8.205	5.028	0.037	0.045	0.121	0.135	0.424	0.411
New York	0.065	0.061	1.333	1.936	0.008	0.008	0.020	0.019	0.069	0.053
Texas	0.111	0.085	3.440	5.350	0.007	0.004	0.018	0.015	0.067	0.052
Washington	0.085	0.097	1.431	1.543	0.009	0.011	0.021	0.031	0.065	0.096
Total	0.124	0.125	8.205	5.350	0.009	0.008	0.024	0.026	0.091	0.092

Note: Each statistic is calculated separately on the set of county-month cells with a positive number of AN dislocations and the set of cells with a positive number of SN dislocations.
Abbreviations: AN=Advance notice, SN=Short notice, Q#=Quartile #

TABLE 2.10: Previously estimated effects of prenatal exposures

Study	Location	Exposure	Timing†	BWT effect	GA effect
Camacho (2008b)	Colombia	Landmine	Trimester 3	-0.4g ns	.
Camacho (2008b)	Colombia	Landmine	Trimester 2	-0.8g ns	.
Camacho (2008b)	Colombia	Landmine	Trimester 1	-2.0g	.
Catalano & Hartig (2001)	Sweden	*Estonia* sunk	Trimester 3	-18 vLWBs ns	.
Catalano & Hartig (2001)	Sweden	*Estonia* sunk	Trimester 2	+26 vLWBs	.
Catalano & Hartig (2001)	Sweden	*Estonia* sunk	Trimester 1	-15 vLWBs ns	.
Catalano & Hartig (2001)	Sweden	Palme murder	Trimester 3	+44 vLWBs	.
Catalano & Hartig (2001)	Sweden	Palme murder	Trimester 2	+13 vLWBs ns	.
Catalano & Hartig (2001)	Sweden	Palme murder	Trimester 1	+16 vLWBs ns	.
Deschênes et al. (2009)	USA	Temp.>85F	Trimester 3	-0.009 %	.
Deschênes et al. (2009)	USA	Temp.>85F	Trimester 2	-0.008 %	.
Deschênes et al. (2009)	USA	Temp.>85F	Trimester 1	-0.003 %	.
Eccleston (2011)	NYC	9/11 attack	Trimester 3	ns	ns
Eccleston (2011)	NYC	9/11 attack	Trimester 2	-14.3g	-0.12 ws.
Eccleston (2011)	NYC	9/11 attack	Trimester 1	-11.9g	-0.22 ws.
Eskenazi et al. (2007)	NYC	9/11 attack	+0–1 ws.	1.44[a], vLWB	1.30[a], vPTB ns
Eskenazi et al. (2007)	NYC	9/11 attack	+0–1 ws.	1.67[a], mLBW	.
Eskenazi et al. (2007)	NYC	9/11 attack	+13–16 ws.	1.36[a], vLWB	ns
Eskenazi et al. (2007)	NYC	9/11 attack	+17–20 ws.	1.28[a], vLWB	∼1.20[a], vPTB
Eskenazi et al. (2007)	NYC	9/11 attack	+33–36 ws.	1.29[a], vLWB	ns
Eskenazi et al. (2007)	Upstate NY	9/11 attack	+17–20 ws.	1.46[a], vLWB	∼1.10[a], mPTB
Eskenazi et al. (2007)	Upstate NY	9/11 attack	+33–36 ws.	1.32[a], vLWB	ns
Eskenazi et al. (2007)	NYC	9/11 attack	+0–8 ws.	ns	0.87[a], mPTB
Eskenazi et al. (2007)	Upstate NY	9/11 attack	+0–4 ws.	ns	0.89[a], mPTB
Khashan et al. (2008)	Denmark	Relative dies	Trimester 3	-29g	.
Khashan et al. (2008)	Denmark	Relative dies	Trimester 2	-47g	.
Khashan et al. (2008)	Denmark	Relative dies	Trimester 1	-27g	.
Khashan et al. (2008)	Denmark	Relative ill	Trimester 3	-10g	.
Khashan et al. (2008)	Denmark	Relative ill	Trimester 2	-13g	.
Khashan et al. (2008)	Denmark	Relative ill	Trimester 1	-15g	.
Simeonova (2011)	USA	Ext. weather	Birth−1 mo.	-0.7g ns	ns
Simeonova (2011)	USA	Ext. weather	Birth−2 mo.	-1.6g	ns
Simeonova (2011)	USA	Ext. weather	Birth−3 mo.	-1.6g	-0.01 ws.
Simeonova (2011)	USA	Strong storm	Birth−1 mo.	-1.8g	-0.01 ws. ns
Simeonova (2011)	USA	Strong storm	Birth−2 mo.	-2.2g	-0.20 ws.
Simeonova (2011)	USA	Strong storm	Birth−3 mo.	-1.1g ns	-0.01 ws. ns
Smits et al. (2006)	Netherlands	9/11 attack	Trimester 3	-71g	-0.5 days ns
Smits et al. (2006)	Netherlands	9/11 attack	Trimester 2	-67g	-1.1 days
Smits et al. (2006)	Netherlands	9/11 attack	Trimester 1	+2g ns	+0.7 days
Torche (2011)	Chile	Earthquake	Trimester 3	+2.6g ns	+0.03 ws. ns
Torche (2011)	Chile	Earthquake	Trimester 2	+17g ns	+0.01 ws. ns
Torche (2011)	Chile	Earthquake	Trimester 1	-51g	-0.19 ws.

† Large plus symbol indicates timing of observed effect relative to exposure. Notation: BWT=Birthweight; (v,m)LBW=(very,moderately)Low birthweight; GA=Gestational age; (v,m)PTB=(very,moderately)Preterm birth; [a] adjusted odds ratio; ns=Reported as statisically non-significant; ws.=weeks; g=grams

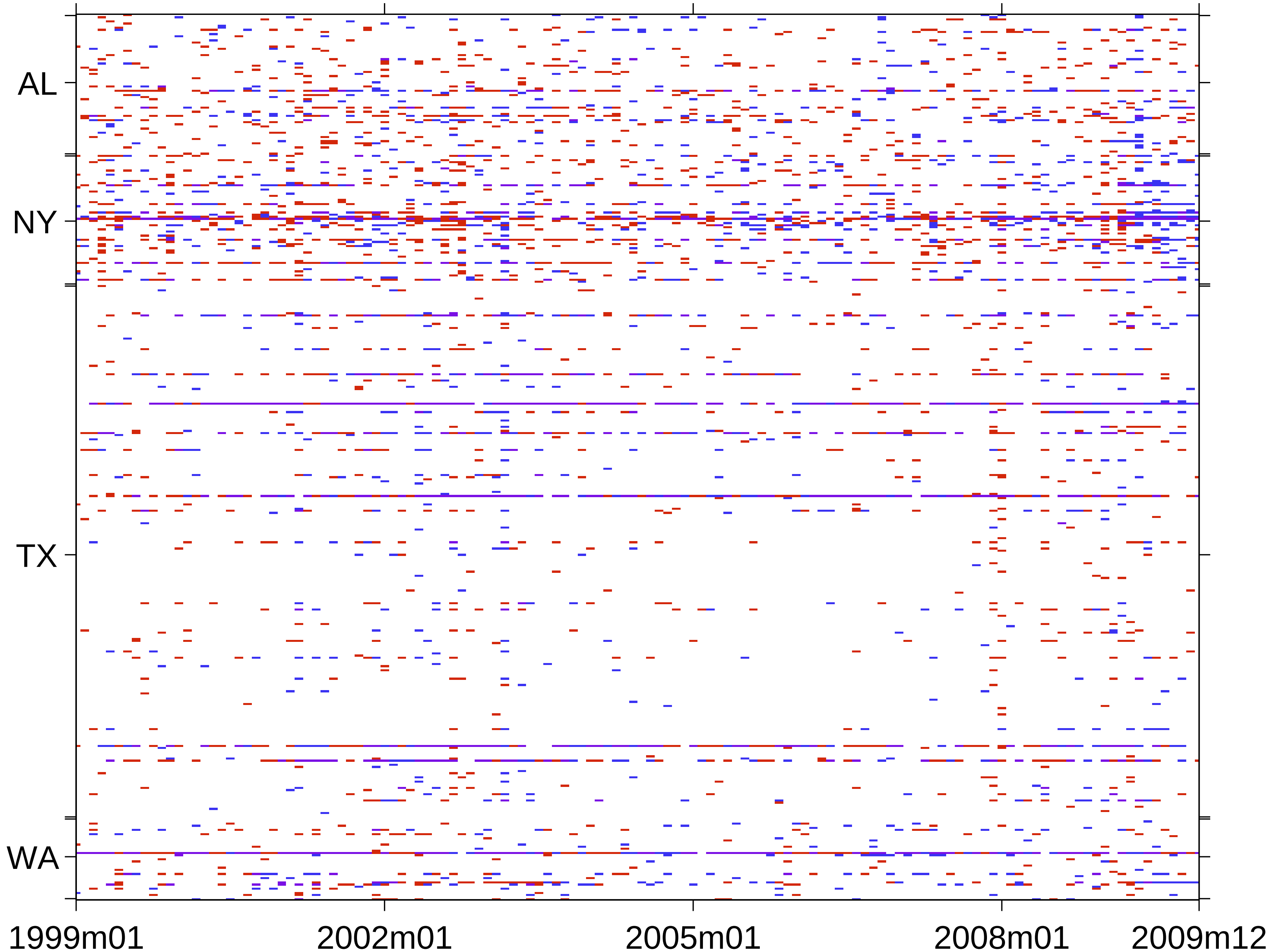

FIGURE 2.4: County-month carpet plot of WARN dislocations; blue=AN, red=SN, purple=both

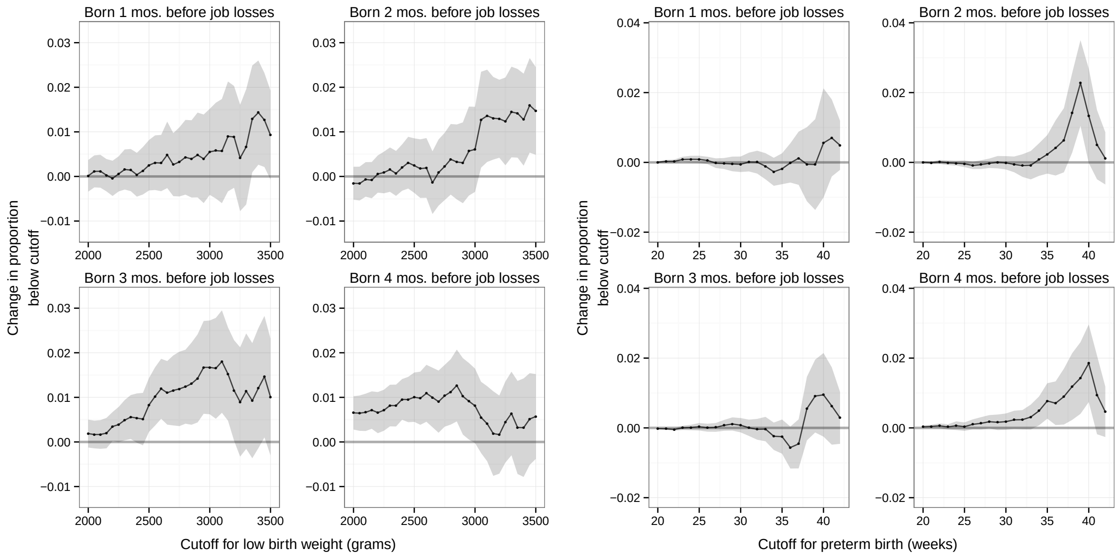

FIGURE 2.5: Anticipatory effects for varying LBW & PTB cutoffs (95% CIs plotted)

TABLE 2.11: Micro-data IV estimates of effects of advance notices in each state

	Birth weight (grams)				Gestational age (days)			
	AL	NY	TX	WA	AL	NY	TX	WA
Trimester 0	-2.39	-12.50	-6.97	-3.00	-0.09	-1.12	-0.36	-0.35
	(2.32)	(11.77)	(8.74)	(8.88)	(0.09)	(0.71)	(0.36)	(0.34)
Trimester 1	3.64*	-11.42	-14.71	5.49	0.11	-0.76	-0.48	0.01
	(1.75)	(19.39)	(9.69)	(5.80)	(0.07)	(0.74)	(0.36)	(0.27)
Trimester 2	-0.46	-6.69	-6.86	-12.68+	-0.00	-1.48*	-0.48+	-0.60
	(1.81)	(9.22)	(7.30)	(6.43)	(0.08)	(0.73)	(0.29)	(0.36)
Trimester 3	-1.09	-42.92**	-26.13**	-25.64	0.06	-2.66**	-0.33	-1.34
	(2.29)	(12.94)	(6.92)	(16.79)	(0.10)	(0.92)	(0.35)	(0.83)
Trimester 4	0.95	-45.63**	-13.74*	0.27	-0.10	-2.62**	-0.39	-0.24
	(2.14)	(9.99)	(5.57)	(8.74)	(0.11)	(0.69)	(0.24)	(0.29)
Trimester 5	-4.63*	-8.82	-2.71	-0.64	-0.10	-1.57**	-0.02	0.01
	(2.04)	(10.45)	(5.09)	(7.58)	(0.08)	(0.57)	(0.22)	(0.22)
Births	483,908	2,056,626	3,112,455	679,315	483,908	2,056,626	3,112,455	679,315
Adj. R-sq.	0.057	0.036	0.035	0.028	0.017	0.015	0.013	0.011

Notes. Effects of exposure a 95$^{\text{th}}$ percentile notice with at least 60 days forewarning. Standard errors (clustered by county) in parentheses. All models include county, year, and month fixed effects along with county-specific trends. Estimates are multiplied by 100 so as to represent percentage point changes. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 2.12: Alternative specifications for micro-data models

	Birth weight (grams)		Gestational age (days)	
	Proportion	Levels	Proportion	Levels
Trimester 0	-2.91	-0.23	-0.24+	-0.08
	(2.46)	(0.70)	(0.14)	(0.06)
Trimester 1	-0.24	-2.05	-0.10	-0.11*
	(2.92)	(1.44)	(0.13)	(0.05)
Trimester 2	-2.70	-0.33	-0.29	-0.11**
	(2.37)	(0.70)	(0.18)	(0.04)
Trimester 3	-10.42+	-2.58*	-0.35	-0.10
	(5.45)	(1.05)	(0.25)	(0.08)
Trimester 4	-4.57	-0.41	-0.35*	-0.06
	(3.51)	(0.79)	(0.18)	(0.04)
Trimester 5	-2.85	0.65	-0.19+	-0.01
	(2.27)	(0.58)	(0.11)	(0.03)
Births	6,332,304	6,332,304	6,332,304	6,332,304
Adj. R-sq.	0.041	0.041	0.019	0.019

Notes. All treatment variables included without stratification by state. Proportion column uses the standard treatment variable used throughout the paper: the proportion of the working-age population affected by WARN notices. The levels column uses the number of workers affected by WARN notices and is scaled to represent the effect of 500 workers. All models include county, year, and month fixed effects. Standard errors clustered by county are in parentheses. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Chapter 3

Red alert: Prenatal stress and plans to close military bases

Abstract

In May 2005 the U.S. military announced a restructuring plan called Base Realignment and Closure (BRAC). Some areas were projected to lose 20 percent of employment, sparking much distress. Previous research shows that stress affects pregnancy and fetal development. I find that immediately following the announcement, the mean gestational age in the most-affected areas dropped by 1.5 days for a period of 1–2 months. Births shifted from 39+ to 37–38 weeks, a period linked to health risks. Similar changes appear in birth weight. Local changes in employment and mothers' characteristics do not account for these effects.

3.1. Introduction

The effects of government policies are the subject of much economics research. However, less attention focuses on the effects of the decision process during which a potential policy is publicly discussed and announced. When the costs of a potential policy change will be highly concentrated in specific areas, their residents may react to the decision process with a great deal of distress. This stress may affect people's mental or physical well-being before any policy changes are implemented. The relevant situations typically involve large, lumpy, localized projects, for example, U.S. domestic military activities. In many communities military operations make up a major portion of economic activity, which makes these communities especially sensitive to actual or planned changes in military policy.[1] For example, as a result of the 2013 budget sequestration, the Department of Defense announced tentative plans to furlough almost all civilian employees for 22 days (Carroll 2013).[2] These plans threatened to have extremely concentrated, negative effects at places like Newport News, Virginia, where the shipyard for nuclear-powered aircraft carriers employs some 21,000 people. In a high-profile speech at the shipyard, President Obama attributed significant harm to the mere threat of furloughs and spending cuts (Shear and Shanker 2005).

To study these announcement effects in a specific and tractable way, I focus on a major military reorganization process called Base Realignment and Closure 2005 (BRAC 2005). It is the most recent and best documented reorganization of the U.S. military. Another round of BRAC may occur in the next few years. BRAC 2005 involved a secretive decision process surrounded by much speculation and anxiety, during which the Department of Defense (DoD) announced a preliminary plan that involved hundreds of military sites throughout the United States. Some major bases were designated for closure or reduction in size, and projected losses of local employment reached as high as 20 percent. However, the implementation of this plan was to begin only long after the announcement and was not certain. An independent commission reviewed the DoD's plan, heard communities' concerns, and ultimately canceled several parts of the plan. This study focuses on May–August 2005, the period starting with the DoD's announcement of its plans and ending with the BRAC commission's final revisions. This period was marked by highly salient uncertainty and much public distress.

The announcement's effects are examined using the human capital literature that considers the effects of stress experienced by women during pregnancy. The condition of the baby at birth is important because it predicts the offspring's later life outcomes such as earnings, education, IQ, and pregnancy outcomes in adulthood (Behrman and Rosenzweig 2004; Black et al. 2007; Royer

[1] Military spending, Medicare-Medicaid, and Social Security each consume similar portions of the federal budget. However, military spending is relatively concentrated in specific areas, a characteristic which is the subject of its own literature (see Braddon 1995).

[2] The initial plans called for 22 days of furlough, which was later cut to 11.

2009).[3] We can better understand the intergenerational evolution of outcomes by studying how conditions during pregnancy affect the offspring (Currie and Moretti 2007b). Several studies have shown that disasters and other extreme events can decrease birth weights and shorten pregnancies in the surrounding area (see, for example, Camacho 2008a or Simeonova 2011). Such findings raise the possibility that stressful events experienced during a critical phase of development might have long-lasting effects on human capital. However, it remains unclear how stress related to violent and destructive events compares to stress caused by changes in economic conditions. This study helps to fill that gap by relating a stress-inducing announcement about economic policy to birth outcomes.

The results presented here show that, in some areas, the BRAC 2005 announcement had effects quite similar to those reported in studies of prenatal stress and natural or man-made disasters. In a few communities that were projected to lose 10–20 percent of employment due to BRAC, the DoD announcement was associated with a significant decrease in mean gestational age. Gestational age trended downward in the months preceding the announcement, and the effect reached a peak just after the announcement of the BRAC list. The month of the announcement and the following month show a brief, sharp drop in gestational age of about 1.5 days in magnitude. The drop is characterized by a shift in births from 39 weeks and above to 37–38 weeks, a period called early-term and associated with long-lasting, negative effects on health and cognitive function. This result is supported by auxiliary, individual-level analyses using a full-term exposure instrument (Currie and Rossin-Slater 2013a), which reveal a strong negative link between expected gestational age and BRAC exposure during the third trimester. Over the key period between the DoD announcement and the commission's decision, the mean gestational age was about half a day lower. These results are robust to many alternative specifications of the control group. The effects on the mean birth weight are more difficult to estimate but are consistent with the effects seen in gestational age. These results suggest that researchers and officials should pay greater attention to the negative psychological and health effects of major policy announcements.

[3] However, the degree of importance of birth outcomes is still a subject of debate (Almond et al. 2005).

Further investigation shows that these effects are unlikely to be explained by economic changes or selection by potential mothers. Unemployment rates show little evidence of a BRAC effect on local economic conditions during 2004–2005. The BRAC-affected areas show some evidence of changes in the characteristics of women giving birth, but these changes are generally small in size, in a direction that cannot account for the effect, or temporally inconsistent with the changes in gestational age. Finally, the main results for gestational age and birth weight are affected little by the inclusion or exclusion of demographic control variables and by highly flexible specifications of the control variables.

The next section of the paper explains the BRAC process and findings from previous studies of stress. The following section covers the data and empirical model. Afterwards the results are presented along with a variety of robustness checks.

3.2. Background

3.2.1. Statutory requirements of BRAC

The BRAC 2005 process was a major restructuring of the United States military in which many military sites were closed or "realigned," that is, had personnel and functions removed and relocated to other sites. The overall effect was mainly a consolidation of operations rather than a reduction in total military size. The procedures for BRAC are specified in the Defense Base Closure and Realignment Act of 1990. Previous rounds of BRAC were carried out in 1988, 1991, 1993, and 1995, which reduced the military's installation inventory by about 21 percent (Base Realignment and Closure Commission 2005a, p. 314).

The requirements of the Act are as follows. The Department of Defense must prepare an inventory of military installations and develop a 20-year plan for national security as a part of the fiscal year 2005 budget process (Base Realignment and Closure Commission 2005b, Appendix C. Section 2912). The Secretary of Defense must then decide whether the military needs to close or realign military installations in order to follow the 20-year plan.

If so, the Secretary must publicly certify the need for BRAC and publish a list of recommended closures and realignments by May 16, 2005 (Base Realignment and Closure Commission 2005b, Appendix C. Section 2914(a)). These *recommendations* must be made according to two sets of criteria. The *military value criteria* have four components: (1) the current and future mission capabilities of the Department of Defense for fighting and training, (2) the availability of land, facilities, and airspace at military sites, (3) the ability to accommodate contingencies and mobilization, and (4) the cost of operations and personnel implications. The *other criteria* are: (1) the extent and timing of costs and savings from closures and realignments, (2) the economic impact of changes on communities near military installations, (3) the ability of community infrastructure to support operational changes, and (4) environmental impact, including restoration and waste management. The Act instructs the Secretary to place higher priority on the military value criteria.

The Act also requires the President to nominate, by March 15, 2005, nine members for the Defense Base Closure and Realignment Commission, which are subject to appointment by the Senate (Base Realignment and Closure Commission 2005b, Appendix C. Section 2912(d)). The Commission is required to review the list of recommendations published by the DoD and prepare a report on its findings and conclusions, which must be sent to the President by September 8, 2005. The review process must include public hearings on the recommendations. In addition, the Government Accountability Office (GAO) must assist in the review process by publishing a report analyzing the recommendations by July 1, 2005. The Commission's report must include a revised set of recommendations. The Commission can modify or strike recommendations that are judged to deviate from the 20-year plan and final criteria. However, new closures and realignments or expansions of realignments require additional procedures, including public hearings and the support of 7 of the 9 commissioners. The President must approve or disapprove the Commission's recommendations by September 23, 2005.

3.2.2. The BRAC list and results

For this study, the two key BRAC events are the public announcement of the Secretary's recommended closings and realignments—commonly called "the BRAC list"—on May 13, 2005 and the final deliberations of the BRAC Commission, broadcast live on August 24–27, 2005. The May 13 announcement was very salient and the subject of great anticipation because the communities near military installations feared reductions in employment following realignment or closure. On May 14, 2005 the *New York Times* covered the list in a front page story (Schmidt 2005). Figure 3.1 shows the intensity of Google searches for "BRAC 2005", which indicates that attention began to increase in late 2004 and built up to a sharp spike in May 2005. A smaller peak occurs in August 2005, the month of the final vote. During this May–August period, the communities subject to closures and realignments lobbied the BRAC Commission fiercely, arguing that their base should be removed from the list.

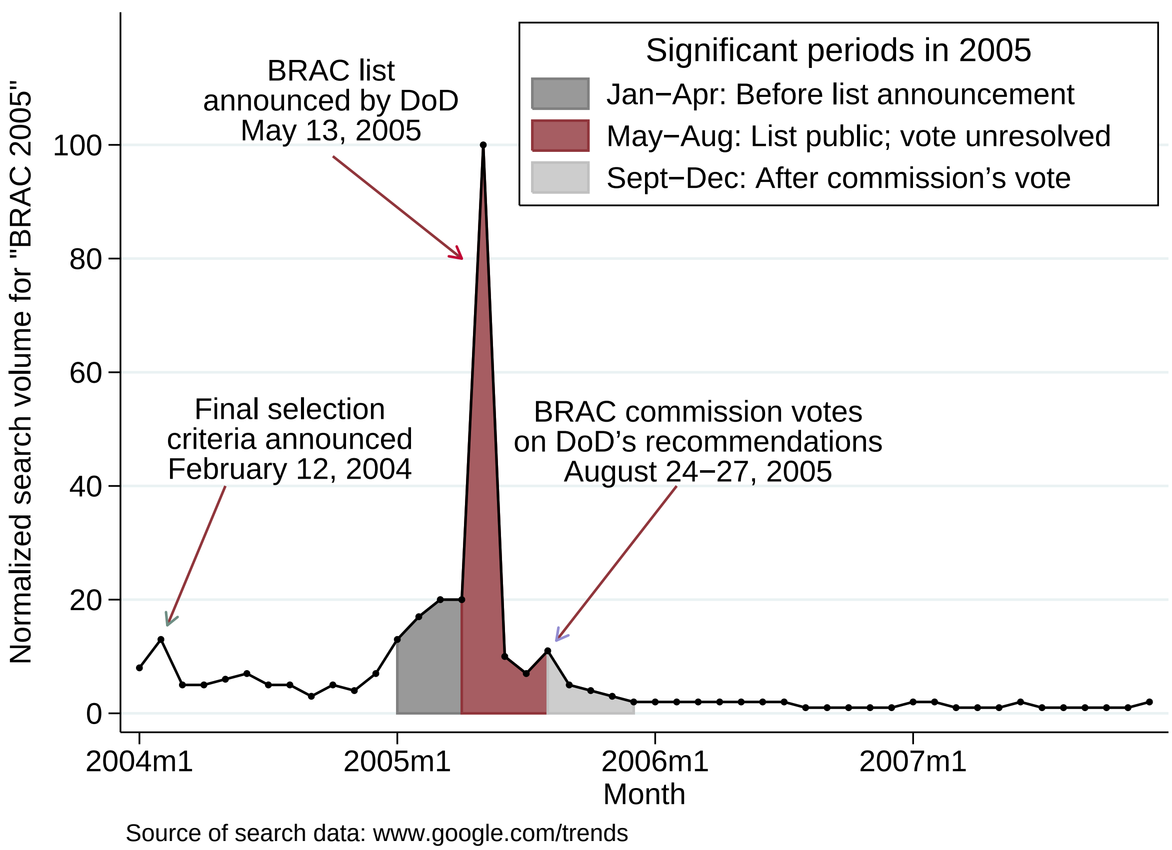

FIGURE 3.1: Public attention to the BRAC process

The DoD's BRAC list proposed 837 closures and realignment actions. Included were 33 major closures and 30 major realignments. The DoD designates a closure as "major" if the facility has a plant replacement value of at least $100 million.[4] A realignment is designated as "major" if it will result in a net reduction of at least 400 military or civilian personnel at the installation (Government Accountability Office 2005). The DoD's designations are not ideal indicators of the local economic impact of BRAC. First, major closures are defined only by the value of physical plant and in some cases affect a small number of personnel. Second, they do not take into account the size of the installation relative to the local economy.

Instead, I focus on six actions singled out by the GAO, which publishes many studies to support the BRAC decision process and audits the results of BRAC implementation. For the statutorily-mandated report released on July 1, 2005, the Government Accountability Office analyzed the potential negative economic impact of the BRAC actions and found that six areas faced "fairly significant impact" (Government Accountability Office 2005, p. 48). These are the communities associated with (1) Cannon Air Force Base, New Mexico; (2) Hawthorne Army Depot, Nevada; (3) Naval Support Activity Crane, Indiana; (4) Submarine Base New London, Connecticut; (5) Eielson Air Force Base, Alaska; and (6) Ellsworth Air Force Base, South Dakota. The estimated, combined direct and indirect losses of area employment ranged from 8.5 percent to 20.5 percent. These areas ("the GAO-6") are listed in table 3.1.

In five of these cases, the Commission modified the recommendation. The Commission canceled the realignment of Hawthorne Army Depot and the closure of Ellsworth Air Force Base. Cannon Air Force base was assigned to enclave status, in which it would remain open while a new use was found for it. The closure of Submarine Base New London was reduced to a realignment, and the realignment of Eielson Air Force Base was partially canceled. Only the realignment of Naval Support Activity Crane stood without modification.

[4]Plant replacement value of a facility is the cost, in current year dollars, of designing and constructing a replacement of the facility at the same site (Office of the Deputy Under Secretary of Defense 2003).

TABLE 3.1: Sites facing significant negative effects from BRAC 2005 (GAO-6)[1]

Military installation	DoD's recommended action	Result[2]	County	Percentage employment loss projected	Population Y2004
Cannon AFB	Close	Enclave	Curry, NM	20.5	45,951
Hawthorne Army Depot	Realign	Struck	Mineral, NV	13.6	4,827
NSA Crane	Realign	Realign	Martin, IN	11.6	10,220
SUBASE New London	Close	Struck/R	New London, CT	9.4	266,890
Eielson AFB	Realign	Struck/R	FNSB, AK	8.6	92,301
Ellsworth AFB	Close	Struck	Pennington, SD	8.5	92,890

[1] "Of those communities facing potential negative economic impact, six communities face the potential for a fairly significant impact. They include the communities surrounding Cannon Air Force Base, New Mexico; Hawthorne Army Depot, Nevada; Naval Support Activity Crane, Indiana; Submarine Base New London, Connecticut; Eielson Air Force Base, Alaska; and Ellsworth Air Force Base, South Dakota, where the negative impact on employment as a percent of area employment ranges from 8.5 percent to 20.5 percent." (GAO-05-785, pp. 48–49)
[2] The BRAC commission spared five of the installations by voting to strike or modify the DoD's recommended action.
Abbreviations: AFB=Air Force Base, NSA=Naval Support Activity, SUBASE=Submarine Base, FNSB=Fairbanks North Star Borough

3.2.3. Public reactions to BRAC as reported by the news media

News coverage of the BRAC process frequently reported anxiety, fear, and local efforts to prepare to lobby the Commission. Before the announcement of the list, local papers ran headlines like "A Good Navy Town, Submarines Run Deep In Groton's Soul, And So Does Fear For The Naval Base's Future" (Hamilton 2005). The local paper in Clovis, New Mexico reported that closure of Cannon Air Force Base could "devastate" the community with over 7,000 jobs lost (Irvin 2005). Hoax emails claiming to leak the list were circulating by late 2004 (Linn 2004). Some community groups sought to retain law firms for assistance in lobbying the Commission (Gargulinski 2005). Economic impact analyses were also commissioned in order to challenge BRAC decisions on the economic impact criterion (see, e.g., State of Connecticut 2005).

The *New York Times*, in a front page story, characterized the day of the BRAC announcement by saying, "On military bases across the country and in the communities that depend on them, all the dread and anxiety comes into high focus today, when the Pentagon plans to release a new list of recommended base closings and consolidations" (Semple 2005). Civilian employees at military sites crowded around televisions waiting for the press conference in which the list would be announced (Hartz 2004).[5] Residents in the GAO-6 were variously reported to react with "engaged

[5] "When defense officials announced Cannon was recommended for closure, a sense of unbelief crashed through the crowd [watching the press conference]. 'We were all very upset and shocked,' said Wike, a manager of dining

anger," "denial", and "shock" (Semple 2005; Hartz 2004).

3.2.4. Previous research on stress

This study's hypothesis depends on two key relationships. First, the announcement of the BRAC recommendations should cause a broad increase in stress levels among residents of the GAO-6. Media reports and data on community lobbying both support this point, and studies discussed below report evidence that people are sensitive to anticipated changes in economic conditions. Second, stress experienced by a woman during pregnancy tends to cause an earlier birth and lower birth weight.

Economic matters are important sources of stress according to representative survey data. In recent surveys by the American Psychological Association (2012), "the economy" is the third most commonly reported stressor, after "money" and "work." Anxiety levels reported by workers are highly responsive to cyclical changes in labor market conditions (Davis and von Wachter 2011). Finally, self-reported negative hedonic experience is sensitive to short-term changes in the S&P 500 index (Deaton 2012). At a finer level numerous studies by health researchers link deterioration of workers' physical and mental health to anticipation of negative workplace events, for example, layoffs, plant closures, and restructuring (Kasl and Cobb 1970b, 1980b; Hamilton et al. 1990b; Ferrie et al. 1995; Grunberg et al. 2001).[6]

A wide variety of research links stress experienced during pregnancy to effects on fetal development and birth outcomes. The risks of low birth weight and preterm birth are positively associated with self-reported stress (see Copper et al. 1996; Dole et al. 2003; and Rondó et al. 2003 for clinical studies with samples sizes on the order of 1,000). The biological mechanisms underlying these links involve neuroendocrine processes, immune/inflammatory activity, and behavior (Wadhwa et al. 2001b,a; Wadhwa 2005; Dunkel Schetter 2011). Two commonly studied hormones in this context are cortisol and corticotropin-releasing hormone (CRH), which are involved

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facilities and clubs at Cannon" (Hartz 2004).

[6] Carlson (2015) provides a more detailed discussion of stress in this context.

in the physiological stress response and pregnancy. Cortisol is implicated in "programming" of the hypothalamic-pituitary-adrenal (HPA) axis, a proposed prenatal mechanism that permanently alters physiology with consequences for cognitive, emotional, and physiological health (Welberg and Seckl 2001; Weinstock 2005; Seckl and Holmes 2007; Glover et al. 2010). Aizer et al. (2009) investigate cortisol using within-mother—between-child variation and report that higher levels predict worse cognitive and health outcomes in the child. Corticotropin-releasing hormone plays a critical role in both the stress response and determining the timing of birth (Erickson et al. 2001). Some researchers propose that CRH functions as a "clock" that determines the onset of birth (McLean et al. 1995). Numerous studies positively associate levels of maternal CRH with the risk of preterm birth (see, for example, Hobel et al. 1999; Inder et al. 2001; Wadhwa et al. 2004; Sandman et al. 2006). Stress can also raise the mother's risk of infection or level of inflammatory processes, which in turn increase the possibility of preterm birth (Wadhwa et al. 2001b). Finally, mothers experiencing stress may cope by using substances or altering diet (Dunkel Schetter 2011). These studies are critically important for establishing the biological plausibility of a causal link between stressful experiences and effects on fetal development. The main shortcoming of this line of research in humans is a lack of exogenous variation in stress.

That shortcoming is partially addressed by the research that studies pregnant women who were exposed to an exogenous, stressful event, typically a disaster or similar extreme situation. A variety of disasters have been studied including *earthquakes* (Glynn et al. 2001; Tan et al. 2009; Torche 2011), *extreme weather* (Xiong et al. 2008; Deschênes et al. 2009; Simeonova 2011; Currie and Rossin-Slater 2013a), *armed conflict* (Catalano and Hartig 2001; Camacho 2008a; Mansour and Rees 2012; Quintana-Domeque and Rodenas 2014), a *nation-wide blackout* (Burlando 2012), and the *September 11, 2001 attacks* (Berkowitz et al. 2003; Lederman et al. 2004; Lauderdale 2006; Smits et al. 2006; Eskenazi et al. 2007; Lipkind et al. 2010; Eccleston 2011; Brown 2014). The designs of these studies are highly variable in terms of the sophistication of statistical methods and origin of data, with some using small, convenience samples and others using comprehensive administrative data. Studies typically report that exposure to extreme events modestly decreases mean birth weight or gestational age and increases the risk of low birth weight or preterm birth.[7] Studies using large administrative data sets have reported birth weight decreases such as 8.7g (Camacho 2008a, landmine exposure), 8–19g (Eccleston 2011, 2001-09-11 in NYC), 5–15g (Brown 2012, 2001-09-11 outside NYC/DC), $\sim$2g and $\sim$30g (Simeonova 2011, storms and floods), 51g (Torche 2011, earthquake), and $\sim$0.3g (Deschênes et al. 2009, per day $>$85F). Reported decreases in mean gestational age are typically on the order of a day (see, for example, Eccleston 2011; Simeonova 2011; Torche 2011). However, even in careful studies there are still concerns. The physical destruction caused by extreme disasters could impede access to food, water, or medical services. Women may leave or be forced from disaster-affected areas or may experience an abortion. These influences could introduce selection bias.

To study some of these concerns Currie and Rossin-Slater (2013a) utilize a dataset that links births to mother identifiers and precise location data. With this data they can use a mother's lagged location to instrument for hurricane exposure during subsequent pregnancies. An instrumental variables approach is also used to correct for the fact that longer pregnancies are more likely to be exposed to a hurricane. The authors report no effect of hurricane exposure on gestational age nor birth weight. However, they do find exposure to increase the risk of abnormal conditions.

This paper also relates to research on economic changes and birth outcomes. Dehejia and Lleras-Muney (2004) report a countercyclical relationship between economic conditions and birth outcomes in the United States. Babies conceived when unemployment is high are at lower risk of low birth weight. However, Bozzoli and Quintana-Domeque (2014) study Argentina during 2001–2003 and report that low economic activity in the month of birth predicts a lower birth weight. However, the effects of economic conditions are strongest in low-education women. The high-education women appear to be sensitive only to economic conditions in early pregnancy. Finally, Lindo (2011) finds evidence that a man's displacement from work can negatively impact the birth weight of his wife's babies.

[7] However, Lipkind et al. (2010) reports no difference in mean birth weight and gestational age between pregnant women in the immediate vicinity of the World Trade Center on 2001-09-11 and other pregnant women in New York City more than 5 miles from the towers.

3.3. Data and model

3.3.1. BRAC data

The BRAC process is heavily documented. The DoD's main report is a volume of about 650 pages, while eleven additional volumes provide supporting documentation (United States Department of Defense 2005a,b). Appendix B details the 449 sites where BRAC actions would have a net, non-zero effect on area employment.[8] The BRAC Commission's report is over 700 pages and indicates all changes to the DoD's original list (Base Realignment and Closure Commission 2005a,b). The last key document is GAO report 05-785 (Government Accountability Office 2005), which identifies the major closures, major realignments, and the GAO-6 sites.

3.3.2. Community lobbying data

There are two separate sets of data on community lobbying: web comments and postal letters. The web comments were obtained from the site BRAC.gov, which was created by the BRAC Commission. Members of the public were able to submit anonymous, public comments at this site without charge. The comments were submitted in 2005 between May 21 and September 16. A total of 13,249 comments are posted at the site. Commenters were able to fill in a field specifying the base on which they were commenting, and about 89 percent of the comments contain this information. The analysis focuses on a subset of 5,884 messages that meet the following criteria: (1) the comment was associated with one of the 444 domestic installations on which the BRAC list had a net personnel effect, (2) the comment was submitted before the Commission's final votes on August 24–27, and (3) the comment was not associated with a set of nine bases that were *added* to the list by the Commission in July.[9]

[8]Each site was assigned an area according to the following rule. If the site was in a metropolitan division (MD), then it was assigned to that MD. Otherwise, if the site was in a metropolitan or micropolitan statistical area, then it was assigned to that area. If neither of the above applied, then the site was assigned to its county.

[9]These bases were actually on the original list, but the Commission voted on July 19 to make the effects more negative. For example, Naval Air Station Oceana was changed from a minor realignment to a major closure (Defense Base Closure and Realignment Commission 2005a).

Data on lobbying via postal letter was derived from an archive of BRAC documents. The Commission scanned and archived all documentation, including letters sent from community members. Metadata on these files is maintained by the University of North Texas Digital Library. These metadata files were used to estimate the number of letters sent by members of each community.[10] The analysis includes the 157,661 letters that met the same selection criteria used for the web comments.

3.3.3. Natality data

The data on births come from the National Center for Health Statistics at the Centers for Disease Control (CDC). Almost all births in the United States are recorded in this data set. The key variables are the birth weight (BWT) in grams, the gestational age (GA) in weeks, the month of birth, and the mother's county of residence. For the purpose of this study, births are assigned to the county in which the mother resides, rather than the county of birth, so as to capture the effect of the mother's environment. Births meeting the following criteria were included: (1) The birth was a singleton, (2) the mother resided and gave birth within the same state, and (3) the mother was 18 to 45 years of age. If a record had a missing value in the birth weight, gestational age, plurality, or location variables, then it was dropped before analysis.

Summary statistics of the birth data appear in table 3.2. Several differences appear between the GAO-6 and control areas. Births in the GAO-6 are more likely to be to non-Hispanic white mothers. These mothers are slightly younger and more likely to be married. The education distribution of mothers in the GAO-6 is more tightly clustered around high school and some college. Finally, births in the GAO-6 have a higher mean weight, by about 44 grams, and gestational age, by about 0.12 weeks.

[10] Standard text processing techniques were used to process metadata entries like: "'Community Correspondence - 214 Individual Letters from Cannon AFB', '2005 BRAC Commission"'. However, in some cases the exact number of letters could not be determined. These cases were counted as one letter. The responses likely contain some measurement error, but this appears to be small. For example, the data contain 125,699 letters from the Niagara Falls area, and local news coverage on the letter campaign reported that more than 123,000 letters had been sent by August 16, 2005 (Buckley 2005).

TABLE 3.2: Summary statistics for birth data, 2000–2005

	All	GAO-6	Control groups				
			Baseline	Minor	Major	Military	States
Mother characteristics:							
Age < 20	7.20	7.81	7.63	7.18	6.18	7.50	7.95
Age $\in [20, 24]$	25.76	29.96	26.96	25.08	23.34	28.51	27.65
Age $\in [25, 34]$	52.58	49.22	51.83	52.56	54.29	49.89	51.24
Age > 34	14.46	13.00	13.58	15.18	16.19	14.10	13.16
Hispanic	21.70	11.81	22.63	35.54	22.64	24.85	19.58
Non-Hispanic white	57.98	71.86	59.54	44.05	51.98	45.64	64.13
Non-Hispanic black	12.20	5.33	10.15	10.99	15.19	12.38	8.39
Non-Hispanic other	7.34	9.39	7.09	8.93	8.94	15.56	6.93
Less than high school	18.42	11.73	19.67	22.89	16.70	15.42	18.48
High school	30.63	37.19	31.46	29.06	28.53	34.00	32.67
Some college	22.14	26.03	22.34	20.58	21.97	24.56	22.82
College graduate	27.45	23.96	25.47	26.24	30.60	23.86	24.59
Married	67.77	69.69	66.95	65.50	70.49	69.11	64.31
Gained < 16 lbs.	12.18	11.11	12.44	11.33	11.64	11.57	11.38
Gained > 60 lbs.	2.03	1.74	2.04	1.94	1.94	2.24	2.25
Smoked while preg.	9.49	14.42	10.53	6.19	6.01	5.87	13.72
Cigs. per day	1.08	0.80	1.21	0.78	0.79	0.82	0.43
Prenatal visits	11.64	11.51	11.64	11.76	11.77	11.54	11.27
C-section	24.71	23.08	24.40	24.47	25.00	24.20	23.60
Induction	20.30	17.37	20.55	16.94	18.94	14.09	22.29
Child characteristics:							
Female	48.80	49.00	48.80	48.80	48.81	48.69	48.78
Birth weight (grams)	3347.28	3391.48	3346.90	3334.23	3355.79	3343.11	3337.69
Gestational age (weeks)	38.84	38.96	38.85	38.83	38.85	38.89	38.81
Low weight (< 2500g)	5.73	5.04	5.71	5.79	5.55	5.75	5.82
Preterm (< 37 wks.)	9.86	8.38	9.89	9.97	9.49	9.79	10.14
Births	13,698,648	38,755	9,082,906	3,884,247	3,098,584	702,046	1,104,693

Notes. Variables are binary and expressed as percentages unless otherwise specified. Standard deviations in appendix.

3.3.4. Additional data

The county-by-month level unemployment data comes from the Local Area Unemployment Statistics program. County-level military employment data is provided by the Bureau of Economic Analysis. Population estimates for July 1 come from the U.S. Census and National Cancer Institute/SEER.

3.3.5. Empirical model

The analysis estimates how county-level mean birth outcomes respond to the BRAC process rather than how individual-level outcomes respond to exposure in different trimesters of pregnancy. Thus the approach here is more like that of Currie and Schmieder (2009). The main reason for adopting this approach is that the BRAC effect is not necessarily confined to the announcement on May 13, 2005. That event is of primary interest, but there is also anticipatory stress before the announcement and the committee's decision in August. These various influences are difficult to disentangle. For example, pregnancies exposed to the May announcement in the second or third trimester were also exposed to anticipatory stress before the announcement. However, for the sake of comparison with the related literature, the appendix does include results from individual-level exposures in the three different trimesters. Those results include estimates using the counterfactual full-term pregnancy instrument proposed by Currie and Rossin-Slater (2013a).

To estimate how birth outcomes respond to the BRAC announcement, I use a linear model with county fixed effects. The mean birth outcome $Y_{i,t}$ for each county i and month t is modeled by

$$Y_{i,t} = \alpha_i + \left(\sum_{\tau=1}^{6} \beta_\tau T^\tau_{i,t} + \gamma^\tau_t \right) + \boldsymbol{Z}'_{i,t}\boldsymbol{\delta} + \epsilon_{i,t},$$

where

- the six treatment variables $T^\tau_{i,t}$ indicate if county i is in the GAO-6 and month t is in the period τ = (1) Jan–Apr 2004, (2) May–Aug 2004, (3) Sept–Dec 2004, (4) Jan–Apr 2005, (5) May–Aug 2005, and (6) Sept–Dec 2005;
- γ^τ_t indicates when t is in period τ as defined above; and
- $\boldsymbol{Z}_{i,t}$ is a vector of other covariates, including year indicators, seasonality indicators, and means of mothers' characteristics.[11] The seasonality indicators are interacted with the treatment group so that the treatment effect estimates are not biased by differences in seasonal patterns between the groups. This specification is strongly supported by tests of the restriction that the seasonal patterns are equal in the two groups.[12]

The key parameter is β_5, which gives the average effect of the BRAC announcement on birth outcomes in the GAO-6 during May–August 2005. The corresponding estimate along with those for the other five periods of interest are reported in the results section. The regressions weight each

[11] The mothers' characteristics are proportions of mothers falling in various groups. The age categories are "Less than 20", "20 to 24", "25 to 34", "Over 34", and "Missing". The race and ethnicity categories are "Hispanic", "White", "Black", "Other", and "Missing". The educational attainment categories are "Less than high school", "High school", "Some college", "College or higher", and "Missing". The marital status categories are "Married" and "Not married". The categories for total birth order are 1, 2, 3, 4, 5 or more, and "Missing". Tobacco use variables are not included in the main results because they are reported irregularly across states and over time. These self-reported variables are also generally suspected to contain significant measurement error. Nevertheless additional results reported in the appendix show that inclusion of tobacco use variables has no substantial effect on the results. The variables included are "non-smoker", "smokes 1–5 cigarettes per day", "smokes 6–10 cigarettes per day", "smokes 11-20 cigarettes per day", "smokes 21 or more cigarettes per day", "smokes an unknown amount", "smoking status unknown", and "smoking variables not reported."

[12] The p value from the seasonality interaction test (SIT) is reported in the regression tables.

cell by the number of births it contains. The cluster-robust covariance estimator is used to allow for within-county correlation in $\epsilon_{i,t}$.

The model focuses only on short-term effects through 2005 because an analysis over a longer term would require an investigation of much greater scope. BRAC actions began to be implemented in 2006, so analyzing longer-term effects would require considering additional changes in military operations, economic conditions, and demographics. However, in the interest of completeness, results for 2006 are presented in the appendix (see table 3.16).

3.4. Results

3.4.1. Lobbying intensity in the GAO-6

I use data on community lobbying to independently check whether the GAO-6 areas reacted unusually strongly to the BRAC list announcement. Both measures of lobbying intensity are far more intense in the GAO-6 than in other areas. Incidence-rate ratios from negative binomial models appear in Table 3.3. These results show that the rate of web comments, where area population is the denominator, was about 600–900 times higher in the GAO-6 relative to areas with minor BRAC actions, while the rate of postal letters was 10,000–50,000 times higher. Compared to areas with a major loss, the GAO-6 areas sent roughly 100 times more web comments and 50–100 times more postal letters. The GAO-6 effects increase after adding controls for urban population, military employment, and area income. The estimated effects of these controls show a striking consistency across both web comments and postal letters. More urban areas are associated with less lobbying as are areas with higher military employment.[13]

[13]This result may seem counterintuitive, but members of the military have relatively little stake in BRAC. They frequently relocate to new bases regardless of BRAC. Thus, they have little reason to lobby.

TABLE 3.3: Estimated citizen lobbying intensities from BRAC-affected areas

	Web comments		Postal letters	
Base category:				
- GAO-6	682.99**	839.52**	10,768.72**	49,859.67**
- Major loss	4.88**	11.12**	243.76**	459.33**
- Minor action	1.00	1.00	1.00	1.00
- Major gain	0.65	1.47	0.61	1.88
Percent urban		0.96**		0.95
Percent military		0.89*		0.84*
Per cap. income		1.00		1.00
Observations	435	435	435	435
Pseudo R^2	0.090	0.116	0.094	0.103

Notes. Incidence-rate ratios from negative binomial models that include population as an exposure term. Statistical significance symbols: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

3.4.2. Birth outcomes

3.4.2.1. Pre-treatment trends

Figure 3.2 shows that the pre-treatment trends in gestational age provide a suitable basis for a difference-in-differences strategy. The mean gestational age decreases by about one day over the period 2000–2004 in both the GAO-6 and the control region.[14] To provide support for the difference-in-differences identifying assumption, I tested and could not reject the hypothesis of a common trend during the periods 2000–2003 and 2000–2004.[15] The evidence of a common trend becomes somewhat stronger after adjusting the means for variation related to mother covariates.[16] However, the mean birth weights in the two areas diverge over the period 2000–2004. This makes estimation of the effect on birth weight more problematic, which is discussed further below. This problem is even more severe for other outcomes such as the proportion low birth weight or preterm, so these auxiliary outcomes are not included in the analysis.

[14] The downward trends in gestational age and birth weight are well-known but cannot be fully explained by changes in demographics or obstetric practices (Donahue et al. 2010).

[15] The test uses the null hypothesis that the five differences in means, $\{\text{MeanGA}_{\text{GAO-6},y} - \text{MeanGA}_{\text{control},y} : y = 2000, 2001, \ldots, 2004\}$, are all equal. These Wald tests use coefficients estimated using only data for the relevant time period.

[16] Adjusted means are plotted in the appendix. Adjustments are made by computing the estimated conditional expectation with all mother covariates held at the sample mean. The estimates are obtained from the same models used in the main analysis but omitting year 2005 data.

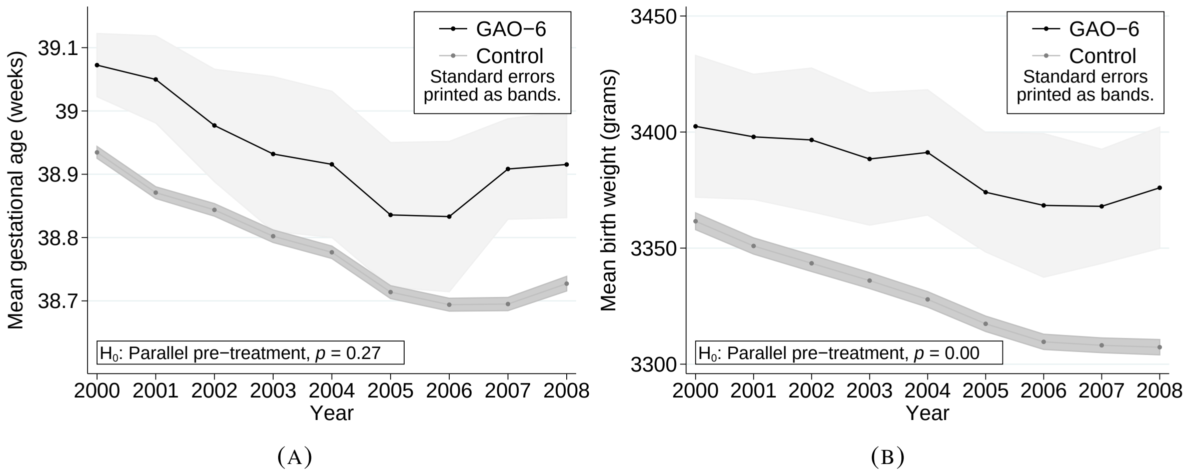

FIGURE 3.2: Year-to-year trends in gestational age and birth weight

3.4.2.2. Main results for gestational age

The main regression results for gestational age appear in Table 3.4. In each of the five models, there are six estimates, which correspond to the six four-month periods of 2004–2005. Across all models the pattern is a decrease in gestational age that is strongest in May–August 2005 followed by an insignificantly positive effect after August 2005. The first column shows results using the baseline control group. In 2005, the January–April and May–August effects reach -0.42 (SE=0.27) and -0.60 (SE=0.20) days, respectively. However, the September–December effect is +0.31 (SE=0.24) days. Below the regression estimates is the p value of the *equality test*, which has the null hypothesis that the three effects in 2005 are equal but not necessarily equal to zero. Thus, the test has some robustness to a violation of parallel trends. The test strongly rejects that the three coefficients are equal. In addition, note that the coefficient estimates do not change monotonically over time. The mean deviation test is designed to test for a linear relationship among the three coefficients. The null hypothesis is that the May–August 2005 effect is equal to the average of the January–April 2005 and September–December 2005 effects. This test also rejects strongly as seen in bottom of Table 3.4.

The other four columns show that the pattern of results is robust to the use of alternative control groups. The *minor control group* is the set of 144 counties that were affected by a minor BRAC

TABLE 3.4: Estimated effects of BRAC list announcement on gestational age and birth weight in the GAO-6

	Gestational age (days)					Birth weight (grams)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Jan.–Apr. '04	0.252	0.065	0.151	0.178	0.473	9.052	7.626	11.496	5.751	10.025
	(0.410)	(0.467)	(0.402)	(0.461)	(0.405)	(13.609)	(14.206)	(14.600)	(14.532)	(13.922)
May–Aug. '04	-0.186	-0.333	-0.294	-0.340	-0.172	4.395	4.196	8.739	1.414	4.623
	(0.441)	(0.456)	(0.456)	(0.506)	(0.446)	(9.730)	(9.930)	(11.455)	(12.506)	(10.530)
Sept.–Dec. '04	-0.452*	-0.477*	-0.486*	-0.254	-0.459*	-24.676**	-28.084**	-20.899*	-22.512*	-19.896*
	(0.189)	(0.207)	(0.204)	(0.257)	(0.209)	(6.982)	(6.882)	(8.095)	(10.425)	(8.576)
Jan.–Apr. '05	-0.421	-0.595+	-0.594*	-0.404	-0.306	-17.829	-21.381	-20.024	-23.781	-9.470
	(0.271)	(0.313)	(0.298)	(0.341)	(0.277)	(16.505)	(16.019)	(16.970)	(17.746)	(17.034)
May–Aug. '05	-0.603**	-0.831**	-0.804**	-0.739**	-0.590*	-35.164*	-37.267**	-44.666**	-44.729*	-29.975+
	(0.204)	(0.183)	(0.206)	(0.272)	(0.246)	(15.163)	(13.941)	(13.717)	(17.719)	(16.899)
Sept.–Dec. '05	0.306	0.236	0.217	0.353	0.391	-19.596	-26.137	-28.800	-33.438	-8.532
	(0.235)	(0.268)	(0.234)	(0.276)	(0.262)	(22.386)	(21.711)	(23.678)	(27.880)	(23.220)
Control group	Baseline	Minor	Major	Military	States	Baseline	Minor	Major	Military	States
Quadratic trend	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
PTT, 2000–2003: p	0.664	0.781	0.699	0.803	0.721	0.039	0.121	0.177	0.045	0.044
SIT: p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.012	0.015
Equality test: p	0.000	0.000	0.000	0.000	0.000	0.000	0.008	0.000	0.013	0.001
Mean deviation test: p	0.000	0.000	0.000	0.001	0.000	0.044	0.091	0.013	0.080	0.023
Cells	115,708	6,764	9,991	3,236	16,524	115,708	6,764	9,991	3,236	16,524
Clusters	1,646	94	139	45	240	1,646	94	139	45	240
Adj. R^2	0.341	0.601	0.626	0.478	0.273	0.514	0.792	0.759	0.777	0.468

Notes. Difference-in-differences estimates displayed. Standard errors, in parentheses, are clustered by BRAC-defined economic areas. All models include year and calendar month-by-treatment group indicators. Symbols of significance at level p: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

action.[17] They satisfied these criteria: (1) A military installation in the county was put on the DoD's BRAC list, (2) no major closure, major realignment, nor major gain occurred in the county, and (3) the employment changes associated with the county's BRAC activities were less than one percent of the county's employment. The *major control group* is the set of 171 counties that fell in an economic area that was slated to have a major closure or major realignment (where economic area is defined as described in the background section above). The minor and major control groups are disjoint. The *military control group* is the set of 56 counties where military employment made up 5–25% of employment in 2004. The military control group is not a strict subset of the minor control group nor of the major control group. Finally, the *states control group* includes only counties in the same states as the GAO-6 areas. The alternate control groups give statistically significant May–August 2005 estimates in the range of -0.59 to -0.83, an interval which includes the estimate obtained from the baseline model. In addition the equality and deviation tests are rejected in all cases.

Only the May–August 2005 estimate is statistically robust across all models, but the results suggest that gestational age was already trending downward before May 2005. The trend can be seen to start in late 2004, which suggests that something was influencing gestational age in the GAO-6 areas before the announcement in May 2005. One possibility is a change in economic conditions. This topic is considered further below, and the unemployment rate data do not reveal any significant changes over 2004–2005. Another possibility is selection or migration by potential parents, also considered in more detail below. The results do not show unusual changes in the characteristics of mothers giving birth during 2004–2005 nor the number of births. Finally, there is the possibility that the decrease in gestational age is due to a process of anticipatory stress that built up and peaked just after the BRAC list was announced. The coefficient estimates on the gestational age effects steadily decrease and appear to be part of a single process that reaches a peak and terminates in May–Aug. 2005. Gestational age then abruptly returned to a baseline level. An anticipatory stress process is consistent with the local press reports discussed above, which indicate

[17]The number of counties is not equal to the number of clusters in the table because some counties are combined according the military's definition of an economic area.

a significant amount of anxiety before the BRAC announcement. However, this explanation also requires that the GAO-6 areas were affected more severely than other military areas. Unfortunately there is no data on beliefs about the relative likelihood of different military sites being assigned to the BRAC list. Research on BRAC or other policy changes should consider the possibility that medical or physiological data might reflect aggregate beliefs about the likelihood of stressful future events.

3.4.2.3. Main results for birth weight

The results for birth weight must be interpreted with caution because, as shown in Figure 3.2, the pre-treatment trends diverge. Table 3.4 displays regression results analogous to those for gestational age, but these birth weight models include treatment group-specific quadratic time trends. This functional form appears most plausible in appendix Figure 3.6. Nevertheless, regression results for alternative specifications are presented in appendix table 3.10. The birth weight results are in qualitative agreement with the gestational age results and suggest a decrease in growth during May–August 2005 relative to the other parts of the year. The May–August 2005 effect estimates range from -30 to -45 grams, but the estimates are somewhat imprecise with standard errors in the range of 14 to 18. Across all the control group specifications the equality tests and deviation tests are rejected, indicating an unusual change in birth weights over the course of the 2005. The results from alternative specifications of the time trend also indicate a decrease in birth weight during May–August 2005 and rejection of the equality test. Ultimately, the results here suggest the same pattern of changes seen in gestational age, but the lack of well-behaved pre-treatment trends precludes a sharp conclusion about the size of the birth weight effects.

3.4.2.4. Month-by-month results for gestational age

Examining the effects on a monthly basis provides additional evidence that the BRAC announcement decreased the mean gestational age in the GAO-6. To investigate the effects of the announcement in finer detail, I estimated a model with separate effects for each month of 2005 instead of only three periods. The model is otherwise identical to that in column 1 of table 3.4. The estimated coefficients are plotted in Figure 3.3. These estimates reveal that the decreased gestational age in the May–August period is actually concentrated in just May and June. The mean gestational age was decreased by about 1.5 days in the month of announcement and the following month. The acute effect of the announcement must have occurred within about two weeks of May 13 because no effect is evident in April. Further, the effects in May and June are almost equal. This strong effect precisely in the period just after the announcement provides strong support for a stress-induced decrease in gestational age. Finally, at the end of August a substantial portion of the BRAC actions affecting the GAO-6 areas were canceled. This “good news” may have had a brief protective effect on birth outcomes as suggested by the positive September estimate with a magnitude of one day. However, there is little research on the effects of positive information, so this result should be viewed tentatively.

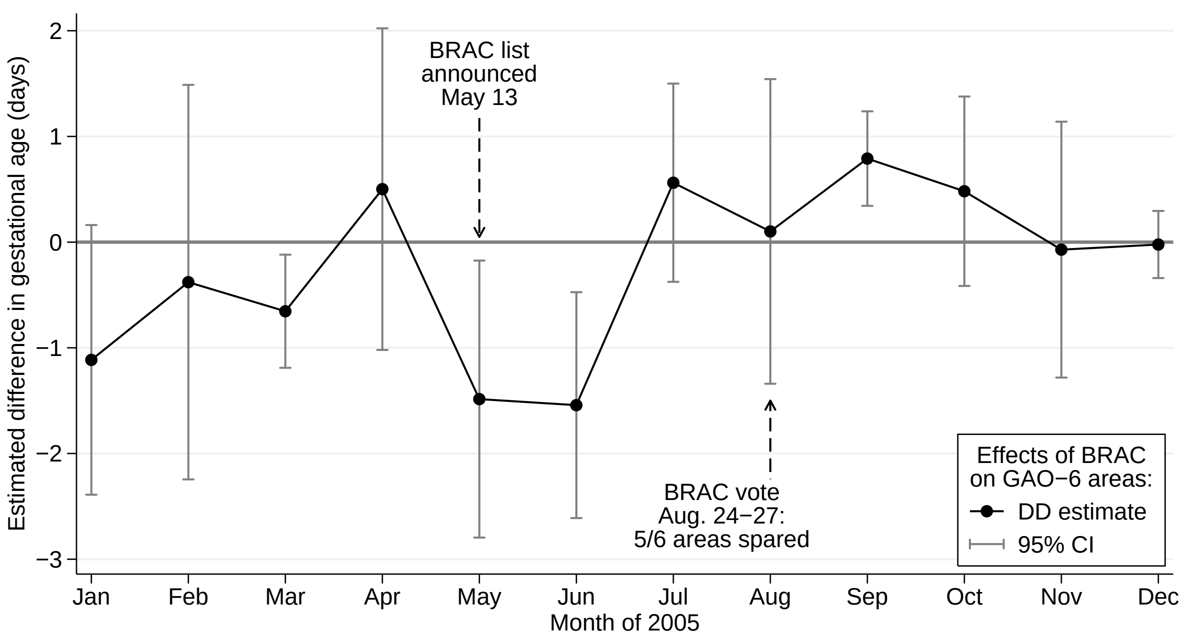

FIGURE 3.3: Estimated gestational age effects by month

3.4.2.5. Distributional effects on gestational age

May through June 2005 appears to be the period where the BRAC effects were most concentrated, so the last level of analysis focuses there. Figure 3.4 depicts how the distribution of gestational age in the GAO-6 areas changed during the May–June 2005 period, which contained 1,147 births. Each plotted series represents the change in the empirical probability mass function (histogram) of gestational age from March–April to May–June within a given year. A positive value at a particular gestational age bin indicates that a larger percentage of births fell into that bin during May–June than during March–April. The 2005 series shows large, positive values at 37 and 38 weeks but negative values at 39–42 weeks. This pattern suggests that the 1.5-day decrease in the May–June average represents a shift in births from 39+ weeks gestation to 37 and 38 weeks, which are regarded as early-term births. If zero change is taken as the baseline, then this result implies that the early-term bins gained about 48 births as a result of the BRAC announcement.[18] Although these births did not fall below the conventional threshold for preterm birth, recent research links early-term births to health and developmental risks. Crump et al. (2013) report that early-term birth predicts greater risk of childhood and adult mortality relative to birth at 39–42 weeks. For those who reached 18 years of age, early-term birth was associated with a 20 percent increase in the risk of death during young adulthood. Yang et al. (2010) report that, relative to children born at 39 weeks, children who were born at 38 or 37 weeks had IQ scores 0.4 or 1.7 points lower, respectively.

The 2005 series represents a change that is opposite to what tends to occur in the other years. The years 2000–2003 show instead large negative values around 38 weeks and large positive values around 39 and 40 weeks. This indicates that the usual change from March–April to May–June is actually towards fewer births around 38 weeks and more around 40 weeks. Finally, in 2004 the distributions in March–April and May–June were very similar to each other.

[18] The calculations are $0.0275 \times 1{,}147 = 31.5425$, for 38 weeks, and $0.0145 \times 1{,}147 = 16.6315$, for 37 weeks. The total is 48.174.

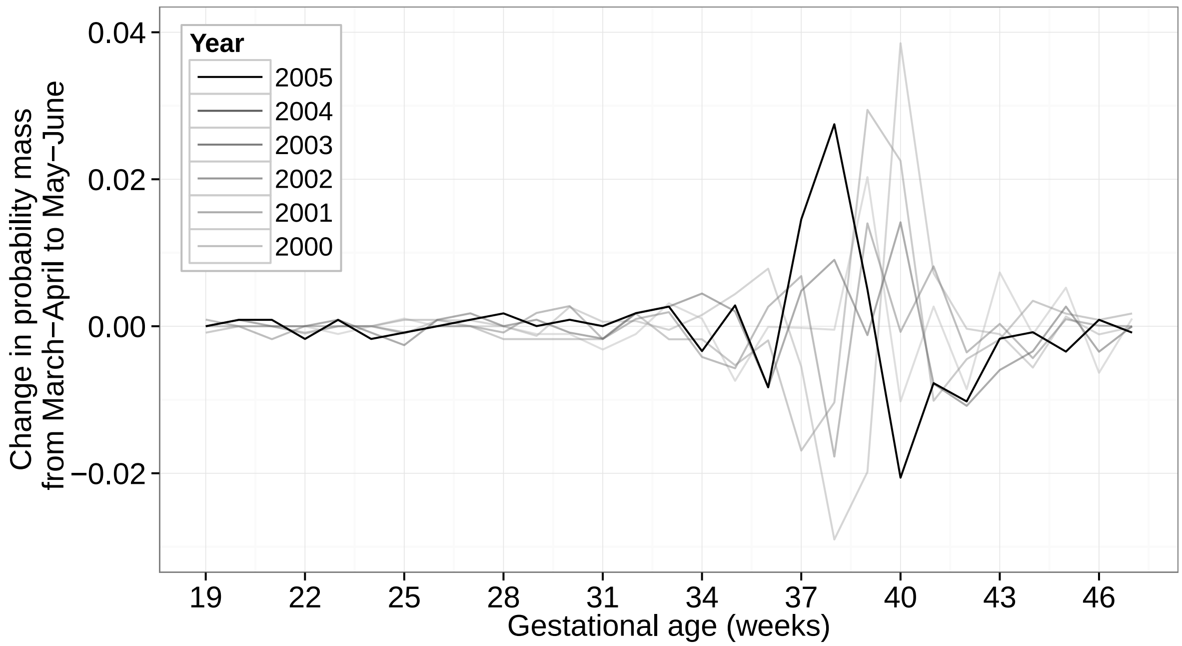

FIGURE 3.4: Effects on the gestational age distribution in the GAO-6

3.4.2.6. Randomization inference for gestational age effect

Additional analysis shows that the changes in birth weights in the GAO-6 are unusual relative to those found in other areas. I conducted a randomization inference test similar to that discussed by Bertrand et al. (2002) and Donohue and Ho (2007). This test holds all covariates constant except for the BRAC treatment variables, which are randomly assigned to other areas. After random assignment, the effects of the randomly assigned treatments are estimated. Under the sharp null hypothesis of no treatment effect in every unit, repetition of this procedure N times delivers an estimate of the randomization distribution of the test statistic of interest, $\{t_n : n = 1, \ldots, N\}$.[19] Observing an extreme test statistic relative to the randomization distribution provides evidence against the null hypothesis.

This test is constructed such that it parallels the research design and incorporates information about the treatment assignment mechanism. Intuitively it answers the question, “If many researchers conducted studies of BRAC by randomly choosing six counties with a large military presence and then conducted the usual analysis, what would be the distribution of results?” In

[19] Calculation of all possible permutations is infeasible.

each realization of the test, the placebo treatments are randomly assigned so as to create a group of six counties that are similar to the GAO-6. These random groups are drawn from the set of 62 counties where military employment made up 5–25 percent of local employment.[20] The test statistic used is the t statistic on the May–August 2005 effect, the same statistic used for inference in the conventional analysis. If the BRAC list announcement truly has no effect, then the observed t statistic is unlikely to be unusual relative to the randomization distribution. Finally, one additional step is included to address the problem of pre-treatment trends and the difference-in-differences identifying assumption. Some randomly formed treatment groups do not share a common trend with the control group. Ignoring this problem would cause potentially severely biased estimates to enter the randomization distribution. To address this problem, the random groups are filtered through the parallel trends test used elsewhere in the paper. Each realization is only included if this test has $p > 0.05$ over 2000–2004.[21]

A total of 5,000 randomizations were attempted, which yielded 922 statistics after filtering. The p-value on the May–August 2005 estimate had a 95 percent confidence interval which fell entirely below 0.05, thus satisfying a simple stopping rule. A histogram of these realizations appears in figure 3.5. The observed $t = -2.91$ has a one-tailed p-value of 0.027. Thus, the real result was unlikely to be generated under the null hypothesis of no BRAC effect. The equality test of the three key coefficients is also rejected under the randomization implementation with a p-value of 0.014. The January–April and September–December coefficients however are not significant at conventional levels. These additional results are plotted in the appendix.

3.4.2.7. Individual-level estimates

For comparison with the literature, estimates on individual-level natality data are presented in the appendix. These models treat the BRAC announcement on May 13, 2005 as a point source of stress like studies of disaster exposure. Exposures are treated both in the conventional way and

[20] This includes the GAO-6 counties. Two of the GAO-6 sites, Martin, Indiana, and Mineral, Nevada, were reported to have less than 5 percent military employment in the Bureau of Economic Analysis data. But these data conflict with military figures and appear to be unreliable due to the small populations.

[21] The trends are tested over 2000-2004 and only the treatment variables for 2005 are included.

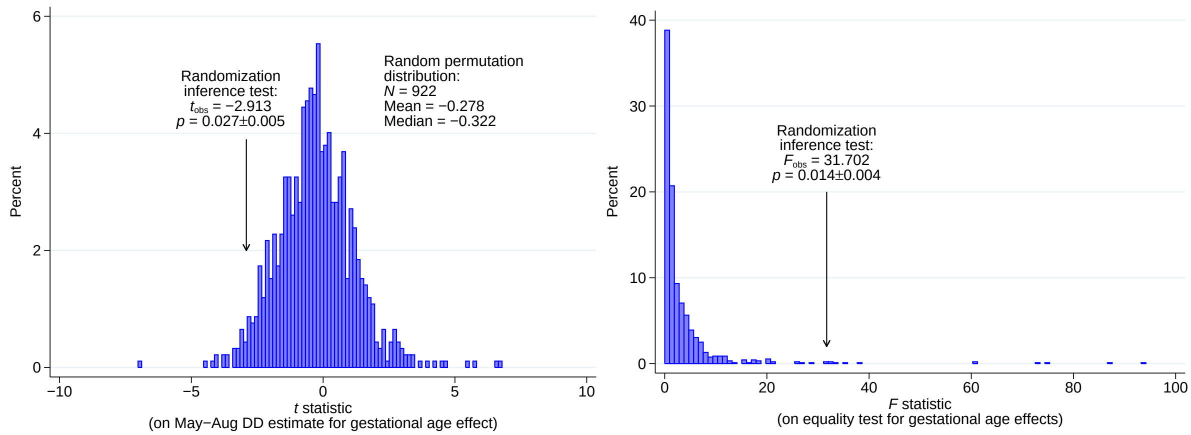

FIGURE 3.5: Histograms of randomization test results, May–Aug. coefficients and equality test

using the full-term instrument proposed by Currie and Rossin-Slater (2013a). These results are consistent with the analysis presented above. Exposure in the third trimester is associated with a decrease in gestational age of about one day (see Table 3.17). The corresponding effect on the probability of preterm birth is positive but does not reach statistical significance in the instrumented model (see Table 3.18). This finding is consistent with the month-by-month results above where the decreases in gestational age tended to shift births downwards to 37–38 weeks, which is above the conventional preterm threshold. The estimated effects on birth weight are negative but not significant at conventional levels. However, the models do show a link between third trimester exposure and increased risk of low birth weight (see Table 3.18). Estimates of selection effects on mothers' characteristics are also consistent with the selection results discussed below, showing a shift towards relatively more white mothers. No significant effects are found on obstetric procedures, mother's age, or reported tobacco use. However, exposure to the BRAC announcement in the second trimester is associated with an increase in the risk of meconium staining (see Table 3.19) of about 2 percentage points. This effect is close to that reported by Currie and Rossin-Slater (2013a). However, unlike the present study, they also found an effect of exposure in the third trimester. Overall, the individual-level results tell a story that agrees with the aggregate results. The mean gestational age in the GAO-6 decreased sharply among pregnancies that were far along at the time of the announcement. There is some evidence of a decrease in birth weights and little evidence of other changes that would account for the decreased gestational age.

3.4.3. Additional mechanisms

3.4.3.1. Unemployment rates

Employers in the GAO-6 areas might have reacted to BRAC by slowing hiring or laying off workers. The unemployment data provide little evidence of this effect. Appendix figure 3.7 plots the unemployment rates in the two areas, which shows that unemployment in the GAO-6 was lower throughout the study period. However, the gap between the two areas grew after the 2001 recession and then steadily shrank.[22] To account for that approximately linear change in the gap from 2002 onwards, the unemployment models in Table 3.5 are estimated on the 2002–2005 data and include group-specific linear trends. The negative coefficients reflect that unemployment in the GAO-6 is lower even after taking into account the linear drift in the gap, although the coefficients are not significant at conventional levels. If anticipation or the consequences of the BRAC announcement had increased unemployment in the GAO-6, then we would expect an upward trend in the coefficients. However, the coefficients show a downward trend with time. In addition, this trend is fairly steady, whereas the changes in birth outcomes show a sharp reversal within 2005. Overall, these results show little evidence of unusual changes in employment activity in the GAO-6.

3.4.3.2. Selection

The means of gestational age and birth weight are also influenced by the composition of mothers. Several additional checks show that changes in the composition of mothers are unlikely to explain the decrease in gestational age and birth weight. The first check is to estimate a model of the birth rate. The estimates from a Poisson conditional fixed-effects model are displayed in the last column of Table 3.6. The results show little evidence of changes in the birth rate, and the usual equality test does not reject the hypothesis that the Jan.–Apr., May–Aug., and Sept.–Dec. coefficients from 2005 are equal. The same results hold for the alternative control groups (see appendix Table 3.29). These results leave little room for women to select out of giving birth in the GAO-6 on net. The second check simply allows the demographic control variables to enter the models of

[22] The militarized areas may have been insulated from the business cycle.

TABLE 3.5: Estimated effects of BRAC list announcement on unemployment in the GAO-6

	(1)	(2)	(3)	(4)	(5)
Jan.–Apr. 2004	-0.012	-0.063	-0.115	-0.169	-0.092
	(0.175)	(0.180)	(0.178)	(0.184)	(0.197)
May–Aug. 2004	-0.150	-0.134	-0.247	-0.315	-0.196
	(0.339)	(0.346)	(0.347)	(0.350)	(0.359)
Sept.–Dec. 2004	-0.421	-0.345	-0.495	-0.594	-0.522
	(0.499)	(0.506)	(0.508)	(0.517)	(0.529)
Jan.–Apr. 2005	-0.204	-0.176	-0.438	-0.578	-0.607
	(0.493)	(0.506)	(0.502)	(0.511)	(0.535)
May–Aug. 2005	-0.181	-0.130	-0.401	-0.630	-0.577
	(0.546)	(0.564)	(0.553)	(0.564)	(0.593)
Sept.–Dec. 2005	-0.292	-0.268	-0.539	-0.778	-0.657
	(0.616)	(0.629)	(0.626)	(0.636)	(0.671)
Control group	Baseline	Minor	Major	Military	States
Equality, p	0.674	0.572	0.479	0.450	0.783
Deviation, p	0.381	0.299	0.227	0.529	0.485
Cells	79,008	7,200	8,496	2,976	11,520
Adj. R^2	0.836	0.864	0.859	0.884	0.794

Notes. All models include year fixed effects, calendar month indicator variables interacted with treatment group, and group-specific linear trends. Cells are weighted by the size of the labor force. The equality test has the null hypothesis that all three coefficients in 2005 are equal. Deviation test explained in main text. Standard errors, in parentheses, are clustered by county. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

mean gestational age and birth weight more flexibly. The estimated decreases in gestational age and birth weight are robust to excluding the demographic controls or simultaneously interacting them with treatment group, calendar month, and year indicator variables. The May–Aug. 2005 estimate of the decrease in gestational age is -0.517 (SE=0.25) when demographic controls are omitted. Including the controls results in an estimate of -0.602 (SE=0.204). Finally, interacting the demographic controls with indicators for treatment group, calendar month, and year results in an estimate of -0.567 (SE=0.226). These robustness results are available for both the baseline and military control groups in the appendix (Tables 3.11 and 3.12).

Further checks for selection are implemented by estimating models of the proportions of mothers falling into various demographic categories. These models are like those used for the main results above but exclude mothers' demographics as covariates. This check was run for a variety of characteristics including age categories, educational attainment, marital status, and smoking status.

The results are presented in Table 3.6 and the appendix. The mothers' characteristics show some evidence of changes over 2004–2005, but these changes appear unlikely to explain the changes in gestational age and birth weight. The magnitudes of the demographic changes are small, and the period-to-period timing of the changes in demographics does not match well with changes in gestational age. Finally, these demographic results are sensitive to the specification of the control group, whereas the main results are quite robust.

For example, during 2005 and especially the crucial May–August period, there is evidence of a moderate increase in the percentages of mothers who are white and married. Comparing the three race/ethnicity columns suggests that births shifted from Hispanic and black mothers to white mothers. This shift appears unlikely to account for the gestational age results because on average births to white mothers and Hispanic mothers have similar gestational ages. Births to black mothers have significantly lower gestational age than the two other groups, and births to married women tend to have higher gestational ages (see appendix Table 3.14). Finally, marriage tends to have a positive effect on gestational age.

The education results in Table 3.6 suggest that the mean educational attainment of mothers in the GAO-6 decreased during 2005 with a shift from college education to high school and partial college. This change would tend to decrease the mean gestational age, but the effects are quite small at about 1–2 percentage points. More importantly the pattern of education changes does not match the pattern of gestational age changes. The proportion of college-educated mothers remains depressed throughout 2005, but the gestational age was depressed only during the first two periods of the year and elevated in the last period. In addition, the decrease in college-education is greatest during Jan.–Apr. 2005, with an estimate of -1.87 percentage points, and then rises to -1.03 during May–Aug. 2005. However, the gestational age decrease was greatest during the May–Aug. period. Furthermore, if the model is estimated using the military control group, then the decrease in the proportion of college-educated mothers is substantially smaller and not statistically-significant. In contrast, the gestational age effect of BRAC appears larger when switching from the baseline to military control group. Finally, in the individual-level models there is no significant association between the mother's education and third trimester BRAC exposure (see appendix Table 3.20). Estimates for additional variables, including weight gain, prenatal visits, birth method, tobacco use, and birth order, are available in the appendix (see Table 3.23 and the following tables). Like the results discussed above, these additional variables overall do not exhibit patterns of changes that can explain the decreases in gestational age and birth weight.

3.4.4. Effects beyond the GAO-6

The GAO-6 areas represent a small portion of those affected by BRAC. Other areas that were targeted by major closures or realignments may have experienced similar stress-related effects. A supplementary analysis here suggests the possibility of small negative effects on gestational age. To examine other areas, I use the same models as in the main analysis (see Table 3.4). However, treatment is redefined to be any major closure or realignment. More precisely, a county was defined as treated if it contained a military site that was assigned to undergo a "major closure" or "major realignment" as defined by the DoD. The GAO-6 areas were entirely excluded from this analysis. The results appear in appendix Table 3.13. First, in some specifications the parallel trends tests reject or nearly reject, so the results are subject to bias. However, there is still some evidence of an effect in May–August 2005 because, like the main GAO-6 results, that period shows lower gestational ages than preceding and following periods. However, the size of the difference is small relative to the GAO-6 results and generally not significant at conventional levels. Thus, the results here are qualitatively consistent with the stress hypothesis, but the effects, if any, are small and strong conclusions cannot be reached.

3.5. Conclusion

In May 2005 the Department of Defense announced plans to close or shrink military sites across the United States. In six communities that were expected to experience serious losses of employment as a result of the policy change, the announcement was met with much anxiety and distress. In

TABLE 3.6: Effects on mothers' characteristics: Age, race, education, marital status

	Age			Race/ethnicity			Education			Married	Birth rate
	< 20	20–24	25–34	Hispanic	White	Black	HS	< college	College		
Jan.–Apr. '04	0.95	0.94*	-0.72+	0.35	1.57	-0.15	-0.36	1.22+	-0.65	1.23	0.04+
	(0.78)	(0.40)	(0.39)	(1.06)	(1.92)	(0.60)	(1.74)	(0.65)	(0.78)	(1.15)	(0.02)
May–Aug. '04	0.30	3.04**	-1.66+	-1.11	1.14	0.68	0.34	1.17	-0.02	1.76**	0.02
	(0.42)	(1.10)	(0.93)	(1.06)	(2.08)	(0.61)	(1.33)	(1.27)	(0.72)	(0.22)	(0.02)
Sept.–Dec. '04	-0.41	1.61+	-0.21	1.57	-1.14	-0.57**	1.63**	-0.14	-0.76	1.40**	0.03
	(0.85)	(0.84)	(0.88)	(1.38)	(1.32)	(0.14)	(0.24)	(0.67)	(0.73)	(0.48)	(0.02)
Jan.–Apr. '05	0.37	1.20*	-0.88	1.07	1.09	-0.06	1.87	-0.05	-1.87**	-0.68	0.04
	(0.88)	(0.61)	(0.81)	(0.86)	(2.64)	(0.75)	(1.21)	(0.89)	(0.50)	(1.81)	(0.02)
May–Aug. '05	0.53	0.46	-0.64	-1.77**	4.36	-0.84**	-0.50	1.29	-1.03*	1.58	-0.01
	(0.80)	(1.14)	(1.14)	(0.46)	(2.83)	(0.22)	(1.28)	(0.79)	(0.47)	(1.17)	(0.02)
Sept.–Dec. '05	0.79	1.72	-1.94	-1.11	0.76	-0.62	1.76	-1.80**	-1.13**	0.92	0.03
	(0.56)	(1.08)	(2.23)	(0.77)	(0.69)	(0.44)	(1.33)	(0.60)	(0.33)	(0.82)	(0.02)
Control group	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
PTT, 2000–03: p	0.000	0.050	0.063	0.083	0.001	0.072	0.207	0.928	0.146	0.000	
SIT: p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	
Equality test: p	0.742	0.769	0.723	0.000	0.131	0.538	0.002	0.004	0.391	0.002	0.329
Deviation test: p	0.929	0.529	0.526	0.105	0.048	0.275	0.006	0.006	0.241	0.001	0.142
Cells	115,708	115,708	115,708	115,708	115,708	115,708	115,708	115,708	115,708	115,708	118,512
Clusters	1,646	1,646	1,646	1,646	1,646	1,646	1,646	1,646	1,646	1,646	1,646
Adj. R^2	0.385	0.629	0.460	0.981	0.972	0.958	0.625	0.451	0.852	0.756	

Notes. Coefficient estimates*100 displayed except for birth rate. Birth rate column shows coefficient estimates from a Poisson conditional fixed-effects model. Standard errors, in parentheses, are clustered by county. All models include year and group-interacted calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

these areas, the mean gestational age decreased by 1.5 days for a period of 1–2 months following the announcement. This decrease was driven by a greater risk of early-term birth, a factor which predicts decreased cognitive function and higher risk of childhood and adult mortality. The mean birth weight showed changes that are consistent with the decreases in gestational age. A smaller decreasing trend in gestational age appeared in the months before the announcement. Alternative models were estimated that treat the BRAC announcement as a point source of stress exposure for individual pregnancies. These results are consistent with the aggregate results and are robust to the use of the full-term instrument proposed by Currie and Rossin-Slater (2013a). The effects on birth outcomes are not accounted for by changes in unemployment rates or demographic features of the mothers in the BRAC-affected areas during 2004–2005.

This result suggests that policy announcements can have substantial effects in areas where many people expect to suffer losses from the policy change. In addition, anxiety about impending economic changes may be revealed by medical or physiological changes in the relevant population. This study focused on areas where effects were concentrated, but there are also people working in relatively small sites who may fear being furloughed or having their work place closed. It is unclear how these isolated workers will differ. They might be less distressed by announcements if they believe that they can easily find another job in their area. They might also be less affected if the results of this study depend on social interactions to amplify distress. However, social interactions might also provide a protective effect, which would make isolated workers even worse off. These questions can only be addressed with additional research using individual-level data.

Nevertheless, these results are highly relevant because many policy changes and business decisions have strongly concentrated effects. As shown here, the mere announcement of a policy is followed by responses similar to those seen following disasters. In addition, the health effects are unlikely to be restricted to just birth outcomes, so further research on additional outcomes would be valuable. Cardiovascular and mental health variables are likely candidates. Officials in areas facing strong impacts from policy changes should be cognizant of health effects. They may want to provide interventions to help employees or citizens manage stress levels and maintain healthful behaviors during uncertain times.

3.6. Appendix of chapter 3

3.6.1. Background

TABLE 3.7: Key dates in BRAC 2005 process

Date	Event
November 15, 2002	BRAC process initiated by SECDEF.[1]
February 12, 2004	Final base selection criteria published by SECDEF.[2]
March 23, 2004	Need for BRAC 2005 certified by SECDEF.[3]
April 1, 2005	BRAC commissioners appointed by President Bush.[4]
May 13, 2005	Recommendations (BRAC list) announced by SECDEF.
July 1, 2005	GAO reported analysis of DoD's recommendations.[5]
August 24–27, 2005	Final deliberations and vote by BRAC commission[6]
2006–	BRAC actions implemented

SECDEF=Secretary of Defense Donald Rumsfeld [1] Government Accountability Office (2005, p. 2) [2] United States Department of Defense (2005a, p. 18) [3] United States Department of Defense (2005a, p. 2) [4] United States Department of Defense [5] Government Accountability Office (2005) [6] Defense Base Closure and Realignment Commission (2005b)

TABLE 3.8: Control groups used in estimation

Control group	Definition
Baseline	All non-GAO-6 areas where no major closure, realignment, or gain occurred.
Minor BRAC	All non-GAO-6 areas affected by BRAC where (1) no major closure, realignment, or gain occurred; and (2) projected changes in local employment (due to BRAC) were less than one percent.
Major BRAC	All non-GAO-6 areas where a major closure or realignment occurred.
Military	All non-GAO-6 areas where military employment is 5–25% of total employment.
States	Alaska, Connecticut, Indiana, Nevada, New Mexico, South Dakota

Notes. Major closures are closures of installations of at least \$100 million in plant replacement value. Major realignments (gains) are actions that would remove (add) at least 400 jobs at an installation.

3.6.2. Data

TABLE 3.9: Summary statistics of natality data

	All	GAO-6	Control groups				
			Baseline	Minor	Major	Military	States
Age < 20	7.20	7.81	7.63	7.18	6.18	7.50	7.95
	(25.84)	(26.83)	(26.54)	(25.81)	(24.09)	(26.34)	(27.05)
Age $\in [20, 24]$	25.76	29.96	26.96	25.08	23.34	28.51	27.65
	(43.73)	(45.81)	(44.38)	(43.35)	(42.30)	(45.15)	(44.72)
Age $\in [25, 34]$	52.58	49.22	51.83	52.56	54.29	49.89	51.24
	(49.93)	(49.99)	(49.97)	(49.93)	(49.82)	(50.00)	(49.98)
Age > 34	14.46	13.00	13.58	15.18	16.19	14.10	13.16
	(35.17)	(33.63)	(34.26)	(35.88)	(36.84)	(34.80)	(33.81)
Hispanic	21.70	11.81	22.63	35.54	22.64	24.85	19.58
	(41.22)	(32.27)	(41.84)	(47.86)	(41.85)	(43.21)	(39.68)
Non-Hispanic white	57.98	71.86	59.54	44.05	51.98	45.64	64.13
	(49.36)	(44.97)	(49.08)	(49.64)	(49.96)	(49.81)	(47.96)
Non-Hispanic black	12.20	5.33	10.15	10.99	15.19	12.38	8.39
	(32.73)	(22.47)	(30.20)	(31.28)	(35.89)	(32.94)	(27.72)
Non-Hispanic other	7.34	9.39	7.09	8.93	8.94	15.56	6.93
	(26.08)	(29.17)	(25.66)	(28.52)	(28.53)	(36.24)	(25.39)
Less than high school	18.42	11.73	19.67	22.89	16.70	15.42	18.48
	(38.76)	(32.18)	(39.75)	(42.01)	(37.30)	(36.11)	(38.82)
High school	30.63	37.19	31.46	29.06	28.53	34.00	32.67
	(46.10)	(48.33)	(46.44)	(45.40)	(45.15)	(47.37)	(46.90)
Some college	22.14	26.03	22.34	20.58	21.97	24.56	22.82
	(41.52)	(43.88)	(41.65)	(40.43)	(41.40)	(43.04)	(41.97)
College graduate	27.45	23.96	25.47	26.24	30.60	23.86	24.59
	(44.63)	(42.68)	(43.57)	(44.00)	(46.08)	(42.62)	(43.06)
Married	67.77	69.69	66.95	65.50	70.49	69.11	64.31
	(46.73)	(45.96)	(47.04)	(47.54)	(45.61)	(46.20)	(47.91)
Gained < 16 lbs.	12.18	11.11	12.44	11.33	11.64	11.57	11.38
	(32.71)	(31.42)	(33.00)	(31.69)	(32.07)	(31.99)	(31.76)
Gained > 60 lbs.	2.03	1.74	2.04	1.94	1.94	2.24	2.25
	(14.10)	(13.09)	(14.12)	(13.80)	(13.78)	(14.80)	(14.82)
Smoked while preg.	9.49	14.42	10.53	6.19	6.01	5.87	13.72
	(29.31)	(35.13)	(30.70)	(24.10)	(23.77)	(23.50)	(34.41)
Cigs. per day	1.08	0.80	1.21	0.78	0.79	0.82	0.43
	(3.81)	(3.05)	(4.04)	(3.21)	(3.22)	(3.21)	(2.36)
Prenatal visits	11.64	11.51	11.64	11.76	11.77	11.54	11.27
	(3.85)	(3.26)	(3.80)	(3.91)	(3.86)	(3.94)	(4.05)
Induction	20.30	17.37	20.55	16.94	18.94	14.09	22.29
	(40.22)	(37.89)	(40.40)	(37.51)	(39.18)	(34.80)	(41.62)
C-section	24.71	23.08	24.40	24.47	25.00	24.20	23.60
	(43.13)	(42.13)	(42.95)	(42.99)	(43.30)	(42.83)	(42.46)
Female	48.80	49.00	48.80	48.80	48.81	48.69	48.78
	(49.99)	(49.99)	(49.99)	(49.99)	(49.99)	(49.98)	(49.99)
Birth weight (grams)	3347.28	3391.48	3346.90	3334.23	3355.79	3343.11	3337.69
	(566.19)	(561.60)	(564.56)	(560.75)	(565.74)	(566.39)	(561.03)
Gestational age (weeks)	38.84	38.96	38.85	38.83	38.85	38.89	38.81
	(2.37)	(2.21)	(2.37)	(2.37)	(2.33)	(2.40)	(2.37)
Births	13,698,648	38,755	9,082,906	3,884,247	3,098,584	702,046	1,104,693

Notes. Variables are binary and expressed as percentages unless otherwise specified. Standard deviations in parentheses.

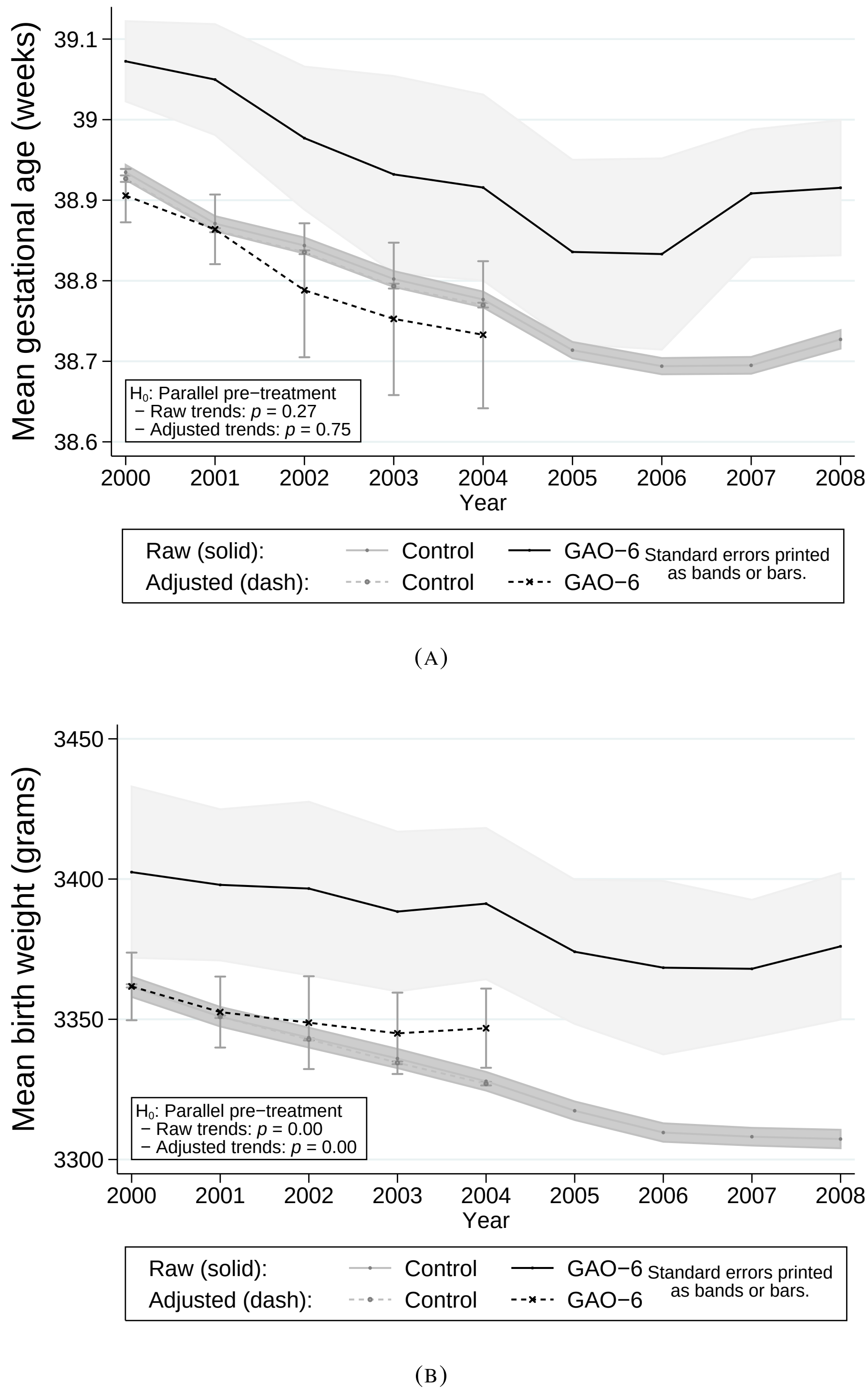

FIGURE 3.6: Adjusted year-to-year trends in gestational age and birth weight

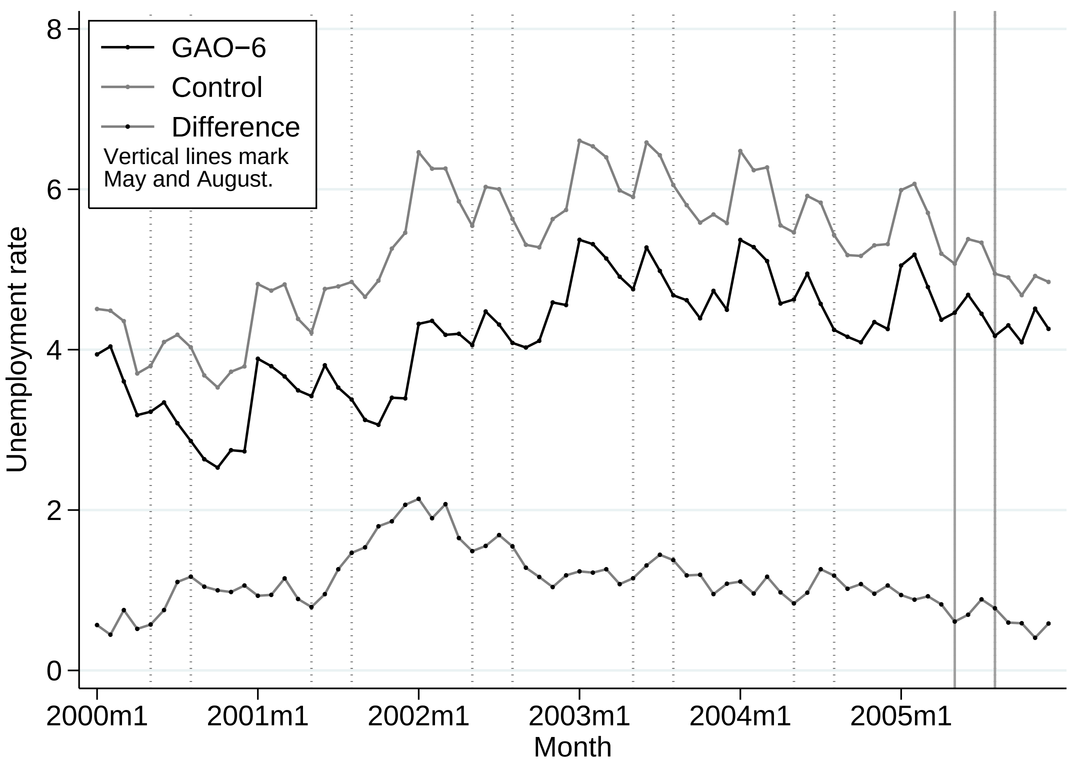

FIGURE 3.7: Unemployment rates by area type, 2000–2005

3.6.3. Results

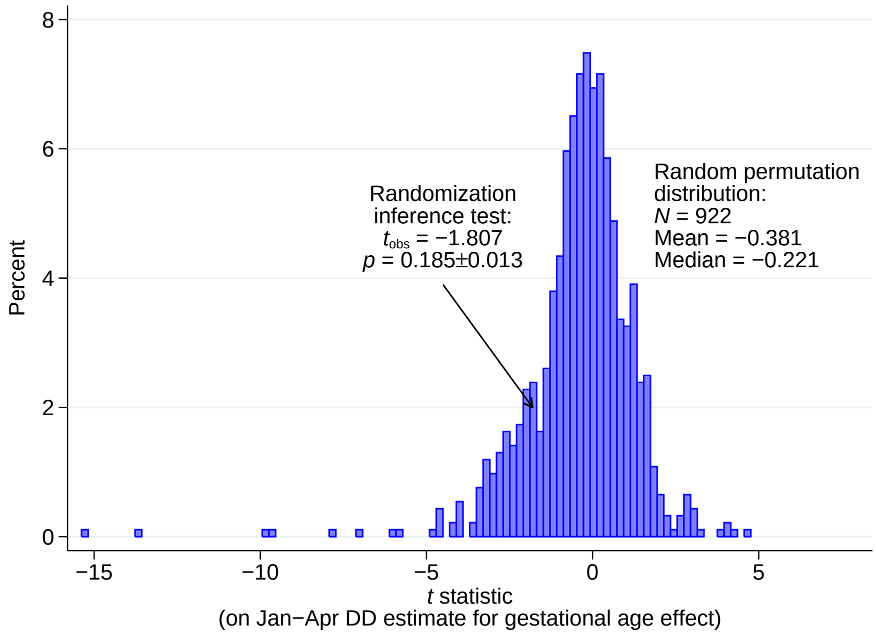

FIGURE 3.8: Histogram of randomization test results, Jan.–Apr. '05

TABLE 3.10: Estimated effects of BRAC list on mean birth weight (grams), alternate trend specifications

	1	2	3
Jan.–Apr. '04	24.657^{**}	15.641	9.052
	(7.62)	(13.95)	(13.61)
May–Aug. '04	23.474^{**}	14.404^{**}	4.395
	(6.87)	(4.63)	(9.73)
Sept.–Dec. '04	-2.315	-11.339	-24.676^{**}
	(10.83)	(9.12)	(6.98)
Jan.–Apr. '05	12.276	-0.461	-17.829
	(18.52)	(16.56)	(16.51)
May–Aug. '05	-0.226	-13.010	-35.164^{*}
	(16.01)	(15.25)	(15.16)
Sept.–Dec. '05	19.967^{*}	7.237	-19.596
	(7.96)	(9.90)	(22.39)
County FE	Yes	Yes	Yes
Linear trend	.	Yes	Yes
Quad. trend	.	.	Yes
Trend test: p	0.000	0.000	0.007
Equality test, p	0.0236	0.0243	0.0004
Cells	115, 708	115, 708	115, 708
Adj. R-sq.	0.514	0.514	0.514

Notes. All models include year and calendar month fixed effects. Standard errors in parentheses. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

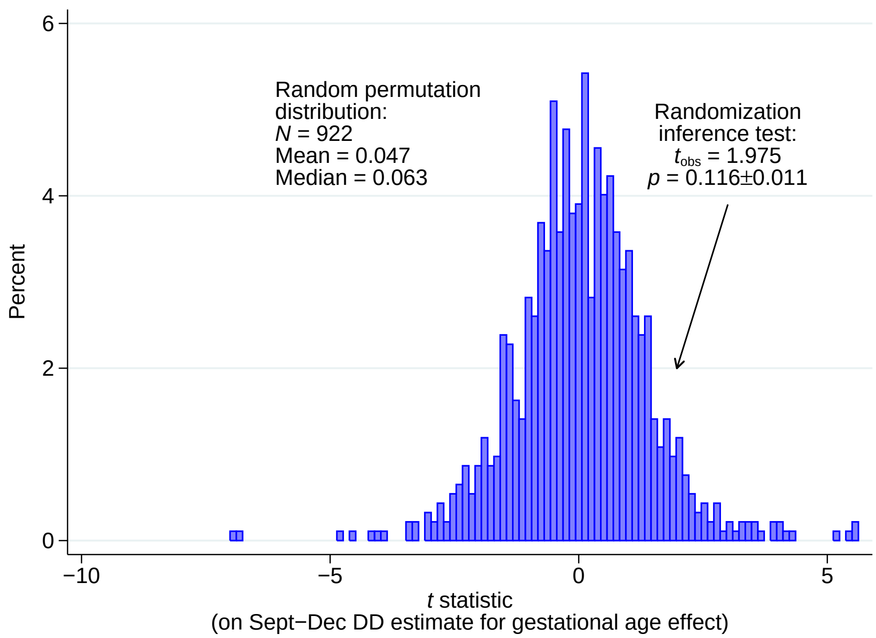

FIGURE 3.9: Histogram of randomization test results, Sept.–Dec. ’05

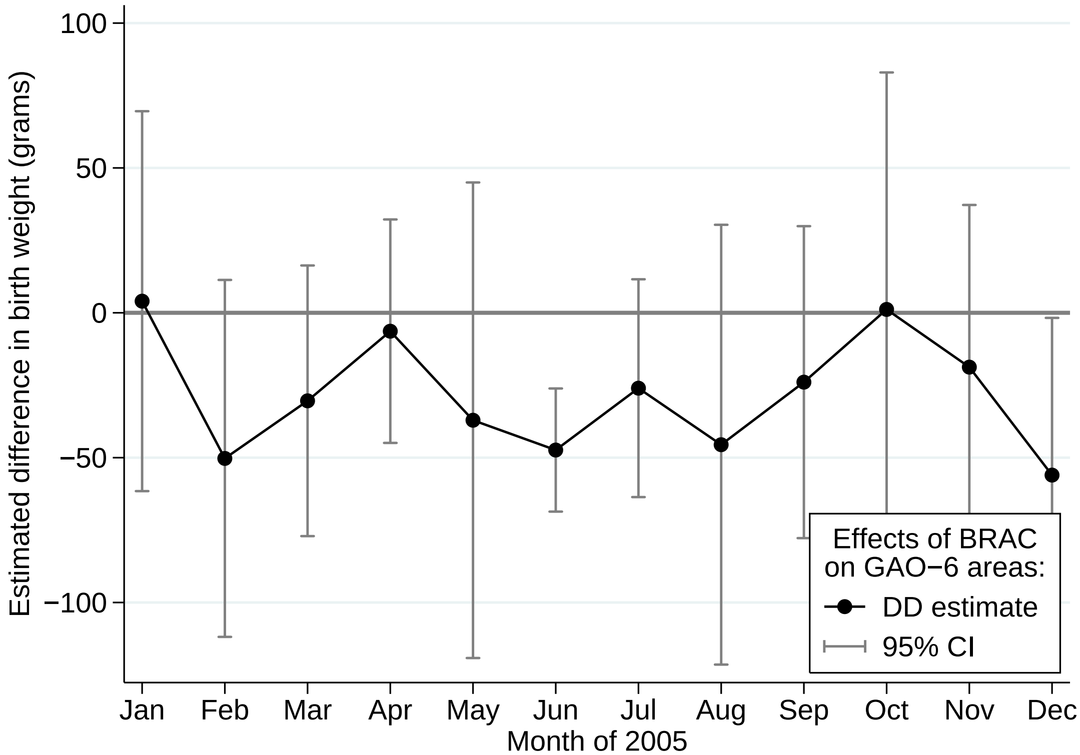

FIGURE 3.10: Estimated birth weight effects by month

TABLE 3.11: Sensitivity of estimates to the specification of demographic controls

	Gestational age (days)					Birth weight (grams)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Jan.–Apr. '04	0.349	0.252	0.237	0.232	0.265	11.115	9.052	8.001	6.903	6.154
	(0.418)	(0.410)	(0.427)	(0.426)	(0.421)	(11.924)	(13.609)	(12.572)	(13.101)	(13.590)
May–Aug. '04	-0.070	-0.186	-0.293	-0.285	-0.253	4.761	4.395	7.201	7.095	6.303
	(0.489)	(0.441)	(0.419)	(0.421)	(0.427)	(9.258)	(9.730)	(13.485)	(13.216)	(14.455)
Sept.–Dec. '04	-0.340	-0.452*	-0.380^{+}	-0.372^{+}	-0.325	-21.373**	-24.676**	-24.216**	-24.276**	-24.359**
	(0.215)	(0.189)	(0.219)	(0.216)	(0.213)	(8.271)	(6.982)	(9.183)	(9.148)	(9.189)
Jan.–Apr. '05	-0.352	-0.421	-0.369	-0.389	-0.358	-17.081	-17.829	-20.246	-21.639	-22.293
	(0.269)	(0.271)	(0.265)	(0.276)	(0.240)	(14.383)	(16.505)	(15.994)	(15.874)	(15.871)
May–Aug. '05	-0.517*	-0.603**	-0.595*	-0.594*	-0.567*	-30.488*	-35.164*	-35.894^{+}	-36.756^{+}	-37.634^{+}
	(0.250)	(0.204)	(0.273)	(0.267)	(0.226)	(12.270)	(15.163)	(19.721)	(19.900)	(20.272)
Sept.–Dec. '05	0.405	0.306	0.355	0.354	0.386^{+}	-19.075	-19.596	-21.943	-22.633	-23.080
	(0.247)	(0.235)	(0.243)	(0.255)	(0.216)	(25.416)	(22.386)	(28.659)	(29.189)	(30.166)
Control group	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Demographics	No	Yes	Interacted[1]	Interacted[2]	Interacted[3]	No	Yes	Interacted[1]	Interacted[2]	Interacted[3]
Equality test: p	0.000	0.000	0.000	0.000	0.000	0.009	0.000	0.072	0.085	0.074
Deviation test: p	0.000	0.000	0.000	0.000	0.000	0.177	0.044	0.076	0.084	0.070
Cells	115,708	115,708	115,708	115,708	115,708	115,708	115,708	115,708	115,708	115,708
Clusters	1,646	1,646	1,646	1,646	1,646	1,646	1,646	1,646	1,646	1,646
Adj. R^2	0.337	0.341	0.341	0.343	0.345	0.499	0.514	0.514	0.514	0.515

Notes. Difference-in-differences estimates displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. [1] Interacted with treatment group. [2] Interacted with treatment group and calendar month. [3] Interacted with treatment group, calendar month, and year. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.12: Sensitivity of estimates to the specification of demographic controls (military control)

	Gestational age (days)					Birth weight (grams)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Jan.–Apr. '04	0.101	0.178	0.161	0.249	0.261	8.302	5.751	4.231	2.536	-14.971
	(0.469)	(0.461)	(0.473)	(0.563)	(0.597)	(12.551)	(14.532)	(13.338)	(16.058)	(17.569)
May–Aug. '04	-0.438	-0.340	-0.496	-0.616	-0.530	2.934	1.414	3.205	4.454	-8.875
	(0.544)	(0.506)	(0.476)	(0.454)	(0.466)	(10.501)	(12.506)	(14.710)	(17.835)	(18.487)
Sept.–Dec. '04	-0.417	-0.254	-0.267	-0.399^{+}	-0.478	-20.055^{+}	-22.512^{*}	-23.298^{*}	-16.945	-30.665
	(0.258)	(0.257)	(0.256)	(0.205)	(0.369)	(10.020)	(10.425)	(11.147)	(15.480)	(19.501)
Jan.–Apr. '05	-0.535^{+}	-0.404	-0.391	-0.423	-0.826^{*}	-22.014	-23.781	-25.758	-28.016	-46.272^{+}
	(0.301)	(0.341)	(0.307)	(0.364)	(0.390)	(15.526)	(17.746)	(17.279)	(23.503)	(24.529)
May–Aug. '05	-0.833^{*}	-0.739^{**}	-0.737^{*}	-0.823^{*}	-1.242^{*}	-38.139^{*}	-44.729^{*}	-46.010^{*}	-41.835	-50.821
	(0.327)	(0.272)	(0.361)	(0.404)	(0.468)	(14.360)	(17.719)	(21.692)	(28.427)	(31.038)
Sept.–Dec. '05	0.182	0.353	0.319	0.400	-0.071	-31.234	-33.438	-36.896	-26.654	-36.944
	(0.288)	(0.276)	(0.291)	(0.268)	(0.364)	(27.588)	(27.880)	(30.861)	(38.625)	(42.352)
Control group	Military	Military	Military	Military	Military	Military	Military	Military	Military	Military
Demographics	No	Yes	Interacted[1]	Interacted[2]	Interacted[3]	No	Yes	Interacted[1]	Interacted[2]	Interacted[3]
Equality test: p	0.000	0.000	0.000	0.000	0.000	0.054	0.013	0.086	0.222	0.674
Deviation test: p	0.000	0.001	0.001	0.013	0.016	0.237	0.080	0.099	0.127	0.382
Cells	3,236	3,236	3,236	3,236	3,236	3,236	3,236	3,236	3,236	3,236
Clusters	45	45	45	45	45	45	45	45	45	45
Adj. R^2	0.465	0.478	0.478	0.478	0.498	0.770	0.777	0.777	0.779	0.780

Notes. Difference-in-differences estimates displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. [1] Interacted with treatment group. [2] Interacted with treatment group and calendar month. [3] Interacted with treatment group, calendar month, and year. Statistical significance symbols: $^{+}\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$.

TABLE 3.13: Estimated effects of BRAC list major closure announcement

	Gestational age (days)					Birth weight (grams)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Jan.–Apr. '04	0.049	-0.049	-0.156	0.039	0.240^{+}	-3.771	-3.811	-3.542	-3.077	-4.851
	(0.108)	(0.121)	(0.131)	(0.284)	(0.136)	(2.959)	(3.612)	(3.748)	(5.300)	(3.427)
May–Aug. '04	-0.031	-0.149	-0.297^{*}	-0.058	0.003	-5.458^{+}	-7.083^{*}	-5.028	-8.360	-7.596^{+}
	(0.115)	(0.125)	(0.120)	(0.302)	(0.155)	(3.019)	(3.449)	(3.772)	(5.908)	(4.322)
Sept.–Dec. '04	0.014	-0.038	-0.155	0.192	0.005	-4.279	-6.675^{*}	0.500	4.717	-3.054
	(0.083)	(0.095)	(0.097)	(0.188)	(0.125)	(2.757)	(3.327)	(3.246)	(4.569)	(4.262)
Jan.–Apr. '05	0.221^{*}	0.078	0.057	0.094	0.298^{*}	-3.546	-4.787	-3.402	-10.560^{*}	-0.897
	(0.106)	(0.119)	(0.133)	(0.216)	(0.147)	(3.195)	(3.555)	(3.678)	(5.210)	(4.384)
May–Aug. '05	0.116	-0.029	-0.265^{*}	-0.160	0.074	-0.377	-1.276	-4.829	-3.230	-1.937
	(0.097)	(0.111)	(0.128)	(0.323)	(0.129)	(3.044)	(3.859)	(3.612)	(5.956)	(3.874)
Sept.–Dec. '05	0.214^{*}	0.117	0.154	0.106	0.278^{+}	2.781	-1.193	4.612	0.400	5.534
	(0.099)	(0.131)	(0.130)	(0.205)	(0.154)	(3.182)	(3.903)	(3.627)	(5.176)	(4.512)
Control group	Baseline	Minor	Major	Military	States	Baseline	Minor	Major	Military	States
PTT, 2000–2003: p	0.145	0.167	0.002	0.105	0.682	0.076	0.079	0.601	0.320	0.152
SIT: p	0.121	0.235	0.039	0.065	0.074	0.147	0.661	0.055	0.015	0.282
Equality test: p	0.518	0.471	0.008	0.464	0.217	0.261	0.537	0.082	0.263	0.321
Mean deviation test: p	0.252	0.229	0.003	0.258	0.092	0.998	0.567	0.102	0.682	0.273
Cells	118,733	9,789	10,496	5,757	19,333	118,733	9,789	10,496	5,757	19,333
Clusters	1,688	136	146	80	279	1,688	136	146	80	279
Adj. R^2	0.381	0.672	0.651	0.679	0.498	0.548	0.825	0.777	0.825	0.649

Notes. Difference-in-differences estimates displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.14: Effects on gestational age (days), additional control variables printed

Jan.–Apr. ’04	0.255	0.090	0.167	0.157	0.480
	(0.408)	(0.463)	(0.394)	(0.453)	(0.399)
May–Aug. ’04	-0.176	-0.288	-0.275	-0.367	-0.161
	(0.446)	(0.470)	(0.451)	(0.499)	(0.447)
Sept.–Dec. ’04	-0.444*	-0.436*	-0.488*	-0.281	-0.455*
	(0.191)	(0.216)	(0.202)	(0.244)	(0.209)
Jan.–Apr. ’05	-0.424	-0.571^{+}	-0.590*	-0.410	-0.305
	(0.274)	(0.322)	(0.288)	(0.324)	(0.274)
May–Aug. ’05	-0.595**	-0.789**	-0.801**	-0.757**	-0.583*
	(0.202)	(0.188)	(0.190)	(0.261)	(0.237)
Sept.–Dec. ’05	0.297	0.239	0.234	0.354	0.378
	(0.244)	(0.294)	(0.207)	(0.250)	(0.265)
Age $\in [20, 24]$	-0.096	-0.216	-1.189	0.265	-0.467
	(0.208)	(1.025)	(0.857)	(1.356)	(0.486)
Age $\in [25, 34]$	-0.852**	-1.577	-1.712^{+}	-1.090	-1.341*
	(0.236)	(1.165)	(0.988)	(1.610)	(0.556)
Age > 34	-2.448**	-2.222	-3.692**	-2.502	-2.930**
	(0.290)	(1.406)	(1.279)	(2.023)	(0.682)
Non-Hispanic White	-0.458	0.332	-0.077	1.206	-0.830
	(0.311)	(1.354)	(0.994)	(1.024)	(0.570)
Black	-4.128**	-3.450**	-4.152**	-4.141**	-3.922**
	(0.376)	(1.133)	(1.029)	(1.445)	(1.397)
Other rage/origin	-0.864	-3.868	-2.860*	-6.146^{+}	-0.112
	(0.611)	(3.116)	(1.285)	(3.503)	(0.697)
Race/origin missing	-1.233*	-3.008*	-0.223	-1.485	-0.476
	(0.577)	(1.169)	(1.206)	(1.454)	(0.906)
High school	-0.278^{+}	-1.192	1.047	1.655	-0.528
	(0.167)	(0.881)	(0.721)	(1.437)	(0.437)
Some college	-0.388^{+}	-0.370	1.430^{+}	1.498	0.346
	(0.208)	(1.042)	(0.810)	(1.696)	(0.520)
College	0.164	0.256	2.126*	2.636	-0.061
	(0.232)	(1.175)	(0.946)	(1.999)	(0.600)
Edu. missing	-0.572	0.640	1.804*	3.415^{+}	-2.132
	(0.735)	(2.498)	(0.912)	(1.962)	(1.463)
Married	0.704**	2.641**	0.676	-0.148	0.333
	(0.141)	(0.720)	(0.467)	(0.822)	(0.343)
Second birth	-0.814**	-0.181	-0.390	-0.034	-0.866*
	(0.138)	(0.639)	(0.577)	(0.979)	(0.335)
Third birth	-1.510**	-1.490^{+}	-1.327*	-2.060^{+}	-1.544**
	(0.160)	(0.790)	(0.660)	(1.194)	(0.376)
Fourth birth	-1.599**	-0.310	-2.257**	-0.941	-1.907**
	(0.204)	(1.077)	(0.812)	(1.408)	(0.479)
Fifth birth or higher	-2.084**	-3.677**	-2.114*	-3.310*	-2.087**
	(0.235)	(1.235)	(0.869)	(1.398)	(0.505)
Birth order missing	-2.191*	-3.677^{+}	-1.040	-0.544	-2.503
	(0.880)	(1.909)	(1.070)	(1.091)	(1.994)
Unknown smoking status	-0.674	-1.780*	2.871**	4.637**	-2.173
	(0.556)	(0.837)	(1.008)	(0.748)	(1.892)
Smokes 1–5 cigs per day	-1.449**	-3.358*	-0.971	-1.017	-1.778
	(0.271)	(1.295)	(1.226)	(1.761)	(1.105)
Smokes 6–10 cigs per day	-0.845**	-0.614	-0.479	-2.461	-1.868^{+}
	(0.214)	(0.972)	(0.858)	(1.820)	(1.060)
Smokes 11–20 cigs per day	-0.749**	-0.186	-2.369^{+}	-0.967	-1.411
	(0.283)	(1.536)	(1.293)	(3.084)	(1.684)
Smokes 21+ cigs per day	-0.574	-1.338	-0.230	-4.435	-1.682
	(0.737)	(4.332)	(3.740)	(7.745)	(3.610)
Smokes unknown amount	-0.696^{+}	-1.816	2.451	2.764	-0.568
	(0.390)	(1.939)	(1.698)	(2.223)	(0.449)
Control group	Baseline	Minor	Major	Military	States
Deviation, p	115,708	6,764	9,991	3,236	16,524
Cells	1,646	94	139	45	240
Clusters	0.342	0.602	0.626	0.480	0.273

Notes. Standard errors, in parentheses, are clustered by county. Omitted categories are “age < 20”, “race missing”, “education missing”, and “non-smoker.” Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.15: Effects on birth weight (grams), additional control variables printed

Jan.–Apr. ’04	10.892	8.302	12.170	6.519	11.905
	(13.063)	(13.615)	(14.213)	(13.812)	(13.374)
May–Aug. ’04	6.672	4.474	9.396	1.931	6.656
	(10.234)	(10.403)	(11.952)	(12.509)	(10.666)
Sept.–Dec. ’04	-22.256**	-28.334**	-21.263*	-22.385*	-18.047*
	(7.797)	(7.775)	(8.742)	(10.232)	(8.908)
Jan.–Apr. ’05	-16.726	-22.711	-21.188	-23.612	-8.784
	(16.896)	(16.271)	(17.161)	(17.756)	(17.076)
May–Aug. ’05	-32.263*	-37.982**	-44.928**	-43.625*	-27.658+
	(14.736)	(13.090)	(13.123)	(16.495)	(16.066)
Sept.–Dec. ’05	-20.359	-29.911	-31.153	-34.121	-10.563
	(24.397)	(22.641)	(24.745)	(27.802)	(25.000)
Age $\in [20, 24]$	5.914	26.508	9.435	39.277	-21.474
	(6.384)	(28.717)	(23.531)	(32.259)	(17.212)
Age $\in [25, 34]$	19.997**	36.622	22.694	44.567	-6.268
	(7.095)	(31.386)	(24.904)	(37.617)	(19.435)
Age > 34	-2.123	2.654	52.335+	63.681	-31.083
	(8.554)	(37.645)	(28.302)	(51.297)	(22.279)
Non-Hispanic White	9.423	-21.650	62.909**	35.430	17.983
	(8.170)	(24.578)	(19.481)	(25.700)	(19.153)
Black	-233.944**	-267.033**	-212.612**	-216.154**	-212.789**
	(11.789)	(32.386)	(22.060)	(40.860)	(41.766)
Other rage/origin	-1.536	-113.182**	-120.877**	-34.356	58.311*
	(12.793)	(33.662)	(37.153)	(50.207)	(25.694)
Race/origin missing	-17.706	-43.434	44.928	-29.558	15.651
	(15.856)	(31.090)	(41.143)	(38.989)	(29.833)
High school	26.563**	-24.361	12.474	72.394*	23.033
	(5.448)	(26.386)	(18.365)	(29.286)	(14.379)
Some college	55.133**	7.423	64.045**	84.243*	66.382**
	(6.293)	(32.850)	(19.914)	(31.895)	(16.355)
College	86.273**	29.177	88.400**	80.645*	75.414**
	(6.958)	(32.927)	(20.306)	(36.185)	(17.879)
Edu. missing	-21.401	-135.547*	18.188	3.170	-61.176
	(19.562)	(56.823)	(35.993)	(37.884)	(47.589)
Married	41.181**	84.388**	14.265	14.860	40.124**
	(4.187)	(19.408)	(16.178)	(24.454)	(11.095)
Second birth	74.575**	76.754**	90.432**	93.847**	87.004**
	(4.400)	(18.007)	(14.632)	(26.292)	(11.696)
Third birth	85.641**	84.925**	80.878**	71.601*	85.325**
	(5.213)	(18.459)	(19.182)	(29.585)	(12.408)
Fourth birth	82.650**	131.506**	91.884**	74.049+	82.172**
	(6.416)	(27.047)	(25.606)	(42.418)	(15.496)
Fifth birth or higher	86.626**	75.285**	70.622**	47.977	89.514**
	(7.115)	(27.519)	(25.566)	(38.152)	(16.337)
Birth order missing	40.127+	103.683**	-11.445	48.962+	12.469
	(23.128)	(34.714)	(35.216)	(25.718)	(32.831)
Unknown smoking status	-35.385**	-22.725+	63.045*	36.728*	-29.377
	(7.201)	(11.686)	(28.765)	(18.030)	(36.860)
Smokes 1–5 cigs per day	-171.140**	-186.345**	-196.978**	-169.708**	-158.584**
	(7.915)	(31.582)	(34.066)	(43.746)	(28.092)
Smokes 6–10 cigs per day	-220.913**	-180.154**	-200.615**	-195.778**	-247.434**
	(6.757)	(34.455)	(24.517)	(46.281)	(28.591)
Smokes 11–20 cigs per day	-260.817**	-313.414**	-267.910**	-260.923**	-266.078**
	(8.719)	(47.149)	(36.934)	(73.408)	(44.702)
Smokes 21+ cigs per day	-277.677**	-495.309**	-203.458+	-298.916	-301.618*
	(23.473)	(134.987)	(116.722)	(213.243)	(122.666)
Smokes unknown amount	-200.448**	-115.091*	-49.268	-110.725*	-212.003**
	(11.265)	(54.589)	(51.007)	(49.774)	(12.907)
Control group	Baseline	Minor	Major	Military	States
Deviation, p	115,708	6,764	9,991	3,236	16,524
Cells	1,646	94	139	45	240
Clusters	0.524	0.796	0.762	0.780	0.479

Notes. Standard errors, in parentheses, are clustered by county. Omitted categories are “age < 20”, “race missing”, “education missing”, and “non-smoker.” Statistical significance symbols: $^{+}\ p < 0.10$, $^{*}\ p < 0.05$, $p < 0.01$.

TABLE 3.16: Monthly estimates for 2006, gestational age and birth weight

	Gestational age (days)					Birth weight (grams)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
January	1.027	-0.154	0.070	0.004	0.199	3.951	-0.295	1.400	-5.914	8.420
	(0.452)	(0.496)	(0.431)	(0.470)	(0.438)	(25.507)	(23.979)	(20.969)	(23.501)	(26.888)
February	1.935	0.385	0.539	0.278	0.889	40.788*	35.381^{+}	40.168*	25.003	45.364*
	(0.983)	(0.522)	(0.491)	(0.511)	(0.552)	(18.174)	(17.947)	(18.167)	(19.270)	(19.222)
March	0.592	-0.871	-0.778	-0.539	-0.119	0.970	-2.501	-4.627	-14.511	4.421
	(0.474)	(0.811)	(0.823)	(0.858)	(0.810)	(12.510)	(12.303)	(14.661)	(14.738)	(13.669)
April	2.227	0.630	0.848	0.809	1.229^{+}	20.049	18.224	15.819	12.819	31.702^{+}
	(1.363)	(0.569)	(0.598)	(0.668)	(0.633)	(16.420)	(18.122)	(18.146)	(18.307)	(17.002)
May	1.700	0.192	0.327	0.280	0.849	41.264*	38.331*	36.988*	41.530*	48.818**
	(1.280)	(0.791)	(0.803)	(0.870)	(0.777)	(16.071)	(16.190)	(14.363)	(16.544)	(17.276)
June	0.566	-0.603	-0.421	-0.603	-0.408	30.383*	30.627*	29.116*	22.603	35.428*
	(0.491)	(0.840)	(0.817)	(0.855)	(0.913)	(13.218)	(14.195)	(14.177)	(15.115)	(14.232)
July	0.314	-1.154^{+}	-1.052	-1.395*	-0.971	2.620	7.005	-0.979	-7.784	1.228
	(0.223)	(0.686)	(0.674)	(0.680)	(0.749)	(5.320)	(7.311)	(6.128)	(9.838)	(6.679)
August	1.334	0.229	0.296	0.157	0.494	11.155	11.419	10.439	8.697	16.100
	(0.679)	(0.542)	(0.507)	(0.554)	(0.519)	(16.716)	(17.974)	(17.916)	(18.906)	(16.859)
September	0.308*	-1.224**	-1.225**	-1.232*	-1.101*	-17.658	-18.379	-11.869	-24.222^{+}	-8.746
	(0.141)	(0.460)	(0.456)	(0.469)	(0.475)	(12.033)	(12.056)	(12.809)	(12.825)	(12.794)
October	1.265	0.175	0.106	-0.077	0.371	18.737**	16.200**	12.053*	9.363	22.061**
	(0.366)	(0.289)	(0.287)	(0.294)	(0.334)	(4.697)	(5.981)	(5.452)	(7.798)	(6.624)
November	0.501	-0.778	-0.717	-0.609	-0.605	-35.771	-39.153	-31.912	-32.678	-35.587
	(0.216)	(0.471)	(0.462)	(0.567)	(0.469)	(26.495)	(28.852)	(30.689)	(29.566)	(26.045)
December	1.399	0.208	0.251	0.000	0.352	27.180^{+}	26.835*	28.792*	25.206^{+}	31.762*
	(0.320)	(0.278)	(0.269)	(0.322)	(0.285)	(13.970)	(12.954)	(13.032)	(14.160)	(14.911)
Control group	Baseline	Minor	Major	Military	States	Baseline	Minor	Major	Military	States
Cells	135,039	7,892	11,658	3,776	19,312	135,039	7,892	11,658	3,776	19,312
Clusters	1,646	94	139	45	240	1,646	94	139	45	240
Adj. R^2	0.351	0.611	0.643	0.498	0.290	0.518	0.795	0.765	0.781	0.477

Notes. Monthly difference-in-differences estimates. Four-month period effects estimates for 2004 and 2005 included but not printed. Standard errors, in parentheses, are clustered by county. Baseline control group. Models include county fixed effects, year indicators, mothers' characteristics, and calendar month indicators interacted with treatment group. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, $p < 0.01$.

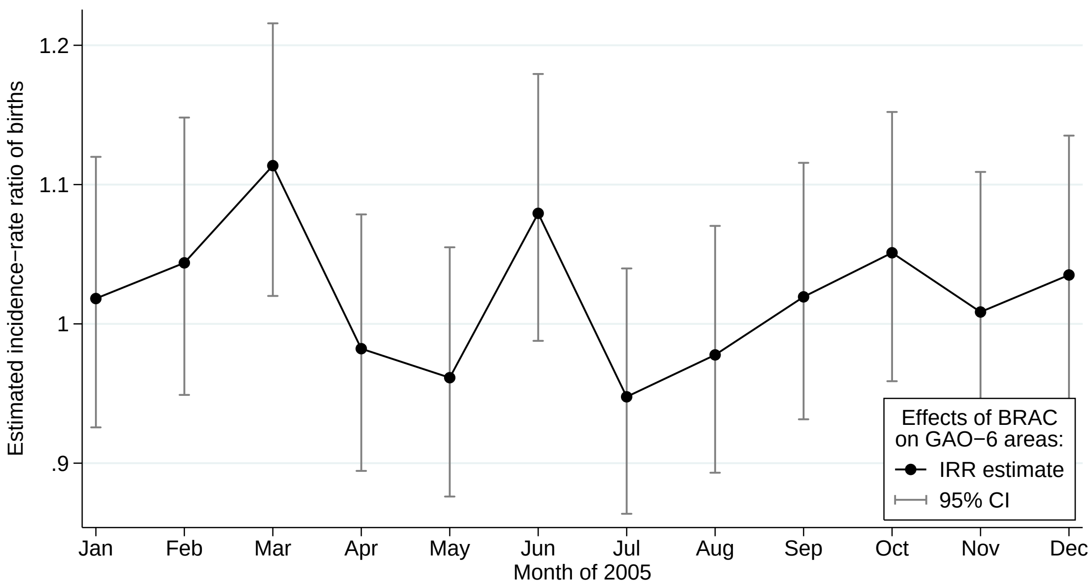

FIGURE 3.11: Estimated birth rate effects by month

Explanation of individual-level estimates — The individual-level estimates use the same samples as the main analysis except that the data are not aggregated into county-month cells and the year 2006 is included.[23] The states and military samples are used for computational tractability. Each birth is assigned a birth date corresponding to the midpoint of the month of birth. A nominal start date is assigned by subtracting the gestational age from the birth date (without adding two weeks). Exposure to the BRAC announcement for each birth was calculated by comparing the nominal start date with May 13, 2005. Exposure in the first trimester was defined as having a nominal start date within the period starting 13 weeks before May 13, 2005 and ending on May 13, 2005. Exposure in the second trimester was defined as having a nominal start date within the period starting 26 weeks before May 13, 2005 and ending 13 weeks before May 13, 2005. Exposure in the third trimester was defined as having a nominal start date more than 26 weeks before May 13, 2005 and being born after May 13, 2005. In addition, exposure calculations also required that the pregnancy reached the trimester in question. The full-term instrument is defined for each birth by calculating the exposure for a pregnancy that started on the same date and ended with a gestational age of 39 weeks.

For each birth indexed by (i, c, y, m), which means birth i in county c in year y in calendar month m, the outcome x is modeled by

$$x_{i,c,y,m} = \sum_{t=1}^{3}(\beta_t D^t_{i,c,y,m} + \psi_t B^t_{i,c,y,m}) + \boldsymbol{\phi}' \boldsymbol{Z}_{i,c,y,m} + \alpha_c + \gamma_y + \delta_m + \Delta_m \times \{c \in \text{GAO-6}\} + \epsilon_{i,c,y,m} \tag{3.1}$$

where

- B^t indicates if the pregnancy was in trimester t on May 13, 2005,
- $D^t_{i,c,y,m}$ indicates if the pregnancy was in trimester t on May 13, 2005 and $c \in$ GAO-6,
- $\alpha_c, \gamma_y, \delta_m, \Delta_m$ are county, year, month, and interacted month fixed effects, and
- $\boldsymbol{Z}$ is a vector of mother characteristics (as described in the main text).

[23] South Dakota contains a GAO-6 site and switched to the revised birth certificate in 2006. So some non-comparable items related to complications and abnormalities are not considered in the individual-level results.

Thus, the estimate of β_t is an estimate of effect of exposure to the BRAC announcement in trimester t. These estimates are reported in table 3.17. In the instrumented models, the variables B^t and D^t are instrumented for by the full-term instruments $\tilde{B}^t$ and $\tilde{D}^t$, respectively. That is, $\tilde{B}^t_{i,c,y,m}$ takes the value that $B^t_{i,c,y,m}$ would have if birth (i, c, y, m) occurred at exactly 39 weeks gestational age. The two variables are identical for all births that actually had a gestational age of 39 weeks. Models that put any characteristic of the mother on the left-hand side do not put any demographic characteristics on the right-hand side.

TABLE 3.17: Individual-level estimates of effects of exposure to the BRAC announcement

	Gestational age (days)				Birth weight (grams)			
	Simple exposure	Instrumented	Simple exposure	Instrumented	Simple exposure	Instrumented	Simple exposure	Instrumented
Trimester 1	0.533*	0.493+	0.447*	0.445*	6.922	6.390	12.612	12.695
	(0.262)	(0.259)	(0.224)	(0.221)	(9.154)	(9.289)	(8.748)	(8.933)
Trimester 2	0.060	0.075	0.286	0.299	21.122	21.102	22.073+	22.019+
	(0.390)	(0.384)	(0.351)	(0.349)	(13.044)	(13.005)	(12.451)	(12.536)
Trimester 3	-1.609**	-1.104*	-1.243**	-0.917*	-22.416	-24.948	-16.162	-21.595
	(0.395)	(0.462)	(0.329)	(0.452)	(16.547)	(20.915)	(16.419)	(21.002)
Control group	Military	Military	States	States	Military	Military	States	States
Births	872,037	872,037	1,345,383	1,345,383	872,037	872,037	1,345,383	1,345,383
Clusters	45	45	240	240	45	45	240	240
Adj. R^2	0.017	0.014	0.016	0.013	0.044	0.044	0.039	0.039

Notes. Difference-in-differences estimates displayed (raw coefficients). Standard errors, in parentheses, are clustered by county. Symbols of significance at level p: $^{+}\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$.

TABLE 3.18: Individual-level estimates of effects of exposure to the BRAC announcement (preterm birth and low birth weight)

	Preterm (proportion)				Low birth weight (proportion)			
	Simple exposure	Instrumented	Simple exposure	Instrumented	Simple exposure	Instrumented	Simple exposure	Instrumented
Trimester 1	-0.017**	-0.017**	-0.015**	-0.015**	-0.011	-0.011	-0.013	-0.013
	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.008)	(0.008)	(0.008)
Trimester 2	-0.008+	-0.008+	-0.009**	-0.009**	-0.005	-0.005	-0.004	-0.004
	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)
Trimester 3	0.018*	0.013	0.018**	0.014	0.017*	0.018*	0.019**	0.020**
	(0.007)	(0.010)	(0.007)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Control group	Military	Military	States	States	Military	Military	States	States
Births	872,037	872,037	1,345,383	1,345,383	872,037	872,037	1,345,383	1,345,383
Clusters	45	45	240	240	45	45	240	240
Adj. R^2	0.009	0.008	0.011	0.010	0.011	0.011	0.011	0.011

Notes. Difference-in-differences estimates displayed (raw coefficients). Standard errors, in parentheses, are clustered by county. Symbols of significance at level p: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.19: Individual-level estimates of effects of exposure to the BRAC announcement (pregnancy characteristics)

	C-section	Induced labor	Meconium staining	Mother's age	Tobacco use (Y/N)	Married
Trimester 1	-0.015^{+}	0.021^{+}	0.004	0.012	0.008	0.016
	(0.009)	(0.011)	(0.006)	(0.278)	(0.006)	(0.010)
Trimester 2	0.011	0.034	0.019**	-0.199^{+}	-0.012	0.013
	(0.028)	(0.022)	(0.003)	(0.110)	(0.011)	(0.013)
Trimester 3	0.006	0.054	0.009	-0.082	0.007	0.006
	(0.012)	(0.040)	(0.009)	(0.067)	(0.009)	(0.013)
Control group	States	States	States	States	States	States
Births	1,337,074	1,333,422	1,333,580	1,345,383	1,345,383	1,345,383
Clusters	240	240	240	240	240	240
Adj. R^2	0.027	0.041	0.015	0.057	0.045	0.045

Notes. Difference-in-differences estimates displayed (raw coefficients) from instrumented models. Standard errors, in parentheses, are clustered by county. Symbols of significance at level p: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.20: Individual-level estimates of effects of exposure to the BRAC announcement (race and education)

	Race/ethnicity			Education			
	Hispanic	White	Black	< HS	High school	Some colleg	College
Trimester 1	-0.009	-0.005	0.003	0.004	0.030	-0.014	-0.020^{+}
	(0.011)	(0.015)	(0.011)	(0.006)	(0.019)	(0.012)	(0.011)
Trimester 2	-0.014*	0.025	-0.017*	0.000	0.011	0.001	-0.011
	(0.007)	(0.018)	(0.007)	(0.009)	(0.014)	(0.011)	(0.011)
Trimester 3	-0.019**	0.035	-0.006^{+}	-0.011	0.007	0.007	-0.005
	(0.005)	(0.024)	(0.003)	(0.007)	(0.017)	(0.011)	(0.009)
Control group	States	States	States	States	States	States	States
Births	1,345,383	1,345,383	1,345,383	1,345,383	1,345,383	1,345,383	1,345,383
Clusters	240	240	240	240	240	240	240
Adj. R^2	0.206	0.220	0.094	0.046	0.022	0.009	0.077

Notes. Difference-in-differences estimates displayed (raw coefficients) from instrumented models. Standard errors, in parentheses, are clustered by county. Symbols of significance at level p: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.21: Estimated effects of BRAC list announcement on complications and abnormalities in the GAO-6

	Abnormal conditions of the newborn				Complications of labor and/or delivery			
	Any	Meconium aspiration syndrome	Ventilator 30+ min.	Ventilator any	Any	Breech	Dysfunc. labor	Meconium staining
Jan.–Apr. ’04	0.031	0.000	0.000	0.033	0.027^{+}	0.001	-0.002	0.015^{+}
	(0.026)	(0.002)	(0.004)	(0.025)	(0.014)	(0.007)	(0.008)	(0.009)
May–Aug. ’04	0.004	-0.004*	-0.003	0.008	0.032*	0.003	0.000	0.008
	(0.006)	(0.002)	(0.008)	(0.007)	(0.015)	(0.002)	(0.004)	(0.005)
Sept.–Dec. ’04	-0.006	-0.002	-0.005	-0.002	0.025	0.004	0.005	0.022**
	(0.008)	(0.002)	(0.005)	(0.008)	(0.024)	(0.004)	(0.005)	(0.008)
Jan.–Apr. ’05	0.009	-0.002	-0.001	0.009^{+}	0.031^{+}	0.006	-0.003	0.020*
	(0.006)	(0.002)	(0.007)	(0.005)	(0.019)	(0.004)	(0.009)	(0.010)
May–Aug. ’05	-0.006	-0.004^{+}	-0.001	-0.000	0.037	0.001	-0.002	0.007
	(0.013)	(0.002)	(0.009)	(0.014)	(0.026)	(0.005)	(0.009)	(0.009)
Sept.–Dec. ’05	-0.009	-0.002	-0.001	-0.007	0.051*	0.005	0.011*	0.022**
	(0.012)	(0.003)	(0.003)	(0.013)	(0.021)	(0.004)	(0.005)	(0.004)
Control group	States	States	States	States	States	States	States	States
PTT, 2000–2003: p	0.190	0.428	0.282	0.208	0.000	0.199	0.008	0.672
SIT: p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Equality test: p	0.315	0.148	0.979	0.325	0.042	0.647	0.005	0.008
Mean deviation test: p	0.494	0.051	0.941	0.892	0.881	0.545	0.628	0.191
Cells	16,514	16,514	16,514	16,524	16,518	16,518	16,518	16,518
Clusters	240	240	240	240	240	240	240	240
Adj. R^2	0.601	0.221	0.224	0.616	0.768	0.139	0.408	0.518

Notes. Coefficient estimates displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.22: Estimated effects of BRAC list announcement on unemployment in the GAO-6

	Unemployment rate					Log of unemployment rate				
	1	2	3	4	5	6	7	8	9	10
Jan.–Apr. 2004	-0.012	-0.063	-0.115	-0.169	-0.092	-0.028	-0.038	-0.047	-0.049	-0.031
	(0.175)	(0.180)	(0.178)	(0.184)	(0.197)	(0.035)	(0.036)	(0.036)	(0.037)	(0.039)
May–Aug. 2004	-0.150	-0.134	-0.247	-0.315	-0.196	-0.066	-0.063	-0.086	-0.088	-0.054
	(0.339)	(0.346)	(0.347)	(0.350)	(0.359)	(0.079)	(0.080)	(0.080)	(0.081)	(0.083)
Sept.–Dec. 2004	-0.421	-0.345	-0.495	-0.594	-0.522	-0.122	-0.108	-0.139	-0.145	-0.125
	(0.499)	(0.506)	(0.508)	(0.517)	(0.529)	(0.121)	(0.122)	(0.122)	(0.124)	(0.126)
Jan.–Apr. 2005	-0.204	-0.176	-0.438	-0.578	-0.607	-0.096	-0.086	-0.141	-0.145	-0.145
	(0.493)	(0.506)	(0.502)	(0.511)	(0.535)	(0.107)	(0.109)	(0.108)	(0.110)	(0.114)
May–Aug. 2005	-0.181	-0.130	-0.401	-0.630	-0.577	-0.103	-0.090	-0.148	-0.165	-0.152
	(0.546)	(0.564)	(0.553)	(0.564)	(0.593)	(0.123)	(0.125)	(0.124)	(0.126)	(0.130)
Sept.–Dec. 2005	-0.292	-0.268	-0.539	-0.778	-0.657	-0.123	-0.112	-0.172	-0.190	-0.169
	(0.616)	(0.629)	(0.626)	(0.636)	(0.671)	(0.144)	(0.145)	(0.145)	(0.147)	(0.153)
Control group	Baseline	Minor	Major	Military	States	Baseline	Minor	Major	Military	States
Equality, p	0.674	0.572	0.479	0.450	0.783	0.775	0.750	0.702	0.528	0.846
Deviation, p	0.381	0.299	0.227	0.529	0.485	0.677	0.563	0.553	0.856	0.732
Cells	79,008	7,200	8,496	2,976	11,520	79,008	7,200	8,496	2,976	11,520
Adj. R^2	0.836	0.864	0.859	0.884	0.794	0.854	0.879	0.884	0.912	0.807

Notes. Difference-in-differences estimates displayed. All models include year fixed effects, calendar month indicator variables interacted with treatment group, and group-specific linear trends. The equality test has the null hypothesis that all three coefficients in 2005 are equal. Deviation test explained in main text. Standard errors, in parentheses, are clustered by county. Statistical significance symbols: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.23: Effects on mothers' characteristics: Age

	Age < 20		Age 20–24		Age 25–34		Age > 34	
Jan.–Apr. 2004	0.95	1.15	0.94*	0.29	-0.72+	-0.79	-1.17**	-0.66+
	(0.78)	(0.81)	(0.40)	(0.45)	(0.39)	(0.48)	(0.32)	(0.36)
May–Aug. 2004	0.30	0.41	3.04**	2.06+	-1.66+	-1.43	-1.68**	-1.05+
	(0.42)	(0.47)	(1.10)	(1.13)	(0.93)	(0.97)	(0.57)	(0.60)
Sept.–Dec. 2004	-0.41	-0.44	1.61+	1.00	-0.21	-0.20	-1.00*	-0.36
	(0.85)	(0.88)	(0.84)	(0.88)	(0.88)	(0.93)	(0.47)	(0.51)
Jan.–Apr. 2005	0.37	0.68	1.20*	0.66	-0.88	-1.07	-0.68	-0.27
	(0.88)	(0.91)	(0.61)	(0.65)	(0.81)	(0.88)	(0.47)	(0.52)
May–Aug. 2005	0.53	0.57	0.46	0.14	-0.64	-0.56	-0.35	-0.15
	(0.80)	(0.83)	(1.14)	(1.17)	(1.14)	(1.18)	(0.55)	(0.59)
Sept.–Dec. 2005	0.79	0.97	1.72	1.46	-1.94	-2.61	-0.57	0.18
	(0.56)	(0.58)	(1.08)	(1.13)	(2.23)	(2.30)	(1.45)	(1.47)
Control group	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military
PTT, 2000–2003: p	0.000	0.037	0.050	0.204	0.063	0.356	0.001	0.211
SIT: p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Equality test: p	0.742	0.735	0.769	0.740	0.723	0.487	0.871	0.962
Mean deviation test: p	0.929	0.669	0.529	0.575	0.526	0.324	0.742	0.907
Cells	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236
Clusters	1,646	45	1,646	45	1,646	45	1,646	45
Adj. R^2	0.385	0.558	0.629	0.786	0.460	0.582	0.673	0.786

Notes. Coefficient estimates*100 displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.24: Effects on mothers' characteristics: Educational attainment

	< High school		High school		Some college		College		Missing	
Jan.–Apr. 2004	0.28	1.47	-0.36	0.35	1.22^{+}	1.80^{+}	-0.65	-0.21	-0.49*	-3.40^{+}
	(0.85)	(1.24)	(1.74)	(1.80)	(0.65)	(1.03)	(0.78)	(0.84)	(0.24)	(1.83)
May–Aug. 2004	-0.82	0.57	0.34	0.91	1.17	1.82	-0.02	0.68	-0.66^{+}	-3.99^{+}
	(0.52)	(1.07)	(1.33)	(1.44)	(1.27)	(1.45)	(0.72)	(0.75)	(0.38)	(2.06)
Sept.–Dec. 2004	-0.36	1.06	1.63**	2.44**	-0.14	0.31	-0.76	-0.46	-0.37	-3.35
	(0.78)	(1.18)	(0.24)	(0.50)	(0.67)	(0.96)	(0.73)	(0.77)	(0.29)	(2.03)
Jan.–Apr. 2005	0.74	2.55^{+}	1.87	2.15	-0.05	0.18	-1.87**	-0.94	-0.69^{+}	-3.94^{+}
	(0.90)	(1.49)	(1.21)	(1.31)	(0.89)	(1.05)	(0.50)	(0.61)	(0.41)	(2.23)
May–Aug. 2005	0.43	2.02^{+}	-0.50	0.69	1.29	1.75	-1.03*	-0.22	-0.19	-4.25
	(0.54)	(1.06)	(1.28)	(1.50)	(0.79)	(1.10)	(0.47)	(0.66)	(0.14)	(2.73)
Sept.–Dec. 2005	1.56	2.90^{+}	1.76	3.49*	-1.80**	-1.92^{+}	-1.13**	0.02	-0.38	-4.50
	(1.21)	(1.63)	(1.33)	(1.44)	(0.60)	(1.05)	(0.33)	(0.59)	(0.24)	(2.74)
Control group	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military
PTT, 2000–2003: p	0.000	0.003	0.207	0.147	0.928	0.960	0.146	0.364	0.000	0.006
SIT: p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Equality test: p	0.410	0.572	0.002	0.082	0.004	0.003	0.391	0.472	0.324	0.380
Mean deviation test: p	0.193	0.294	0.006	0.032	0.006	0.004	0.241	0.606	0.163	0.935
Cells	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236
Clusters	1,646	45	1,646	45	1,646	45	1,646	45	1,646	45
Adj. R^2	0.806	0.850	0.625	0.757	0.451	0.593	0.852	0.852	0.456	0.537

Notes. Coefficient estimates*100 displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.25: Effects on mothers' characteristics: Race/ethnicity

	Hispanic		White		Black		Other		Missing	
Jan.–Apr. 2004	0.35	1.12	1.57	1.75	-0.15	0.35	0.45	1.68*	-2.23	-4.89*
	(1.06)	(1.18)	(1.92)	(2.22)	(0.60)	(0.64)	(0.38)	(0.67)	(2.02)	(2.39)
May–Aug. 2004	-1.11	-0.14	1.14	1.04	0.68	0.86	1.39+	2.56*	-2.10	-4.31+
	(1.06)	(1.19)	(2.08)	(2.38)	(0.61)	(0.65)	(0.82)	(1.06)	(1.82)	(2.23)
Sept.–Dec. 2004	1.57	2.57+	-1.14	-2.81	-0.57**	-0.26	1.21	2.29*	-1.07	-1.79
	(1.38)	(1.42)	(1.32)	(1.68)	(0.14)	(0.20)	(0.88)	(1.07)	(0.82)	(1.44)
Jan.–Apr. 2005	1.07	1.80+	1.09	-0.02	-0.06	0.58	0.50	1.36	-2.60	-3.72
	(0.86)	(0.94)	(2.64)	(2.94)	(0.75)	(0.77)	(0.87)	(1.11)	(2.25)	(2.63)
May–Aug. 2005	-1.77**	-0.94	4.36	3.38	-0.84**	-0.49+	0.80	1.78+	-2.55	-3.73
	(0.46)	(0.56)	(2.83)	(3.17)	(0.22)	(0.29)	(0.67)	(0.91)	(2.16)	(2.65)
Sept.–Dec. 2005	-1.11	-0.05	0.76	-0.22	-0.62	-0.37	1.47*	2.36*	-0.50**	-1.73
	(0.77)	(0.85)	(0.69)	(1.46)	(0.44)	(0.49)	(0.60)	(0.92)	(0.18)	(1.44)
Control group	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military
PTT, 2000–2003: p	0.083	0.230	0.001	0.048	0.072	0.064	0.267	0.398	0.000	0.026
SIT: p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.350	0.000	0.000
Equality test: p	0.000	0.005	0.131	0.146	0.538	0.437	0.000	0.002	0.479	0.573
Mean deviation test: p	0.105	0.117	0.048	0.057	0.275	0.232	0.882	0.949	0.299	0.319
Cells	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236
Clusters	1,646	45	1,646	45	1,646	45	1,646	45	1,646	45
Adj. R^2	0.981	0.986	0.972	0.954	0.958	0.976	0.951	0.988	0.462	0.436

Notes. Coefficient estimates*100 displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.26: Effects on mothers' characteristics: Total birth order

	First		Second		Third		Fourth		Fifth+		Missing	
Jan.–Apr. 2004	2.08	1.62	-0.86	-0.95	-1.41*	-0.76	1.17	1.67	-0.83	-0.45	-0.15	-1.13
	(1.67)	(1.78)	(1.33)	(1.41)	(0.57)	(0.64)	(1.00)	(1.03)	(0.88)	(0.91)	(0.17)	(1.05)
May–Aug. 2004	3.80	3.67	0.29	0.20	-1.26	-0.55	-2.05**	-1.84*	-0.75	-0.48	-0.04	-0.99
	(2.71)	(2.79)	(0.98)	(1.07)	(1.18)	(1.23)	(0.73)	(0.76)	(0.62)	(0.71)	(0.15)	(1.06)
Sept.–Dec. 2004	2.16**	1.80**	0.65	0.23	-1.08	-0.93	-0.99	-0.76	-0.68	-0.41	-0.07	0.07
	(0.49)	(0.60)	(1.85)	(1.91)	(0.93)	(1.03)	(1.12)	(1.14)	(0.44)	(0.51)	(0.10)	(0.20)
Jan.–Apr. 2005	1.32	0.94	0.35	0.02	-0.28	0.26	-0.28	-0.38	-0.95	-0.84	-0.15	-0.00
	(1.51)	(1.55)	(0.97)	(1.07)	(0.94)	(0.99)	(0.65)	(0.67)	(0.59)	(0.64)	(0.14)	(0.23)
May–Aug. 2005	1.20	0.83	0.33	-0.11	1.03	1.27	-1.66	-1.46	-0.85+	-0.67	-0.05	0.14
	(1.77)	(1.85)	(0.72)	(0.77)	(0.87)	(0.93)	(1.11)	(1.13)	(0.46)	(0.51)	(0.09)	(0.29)
Sept.–Dec. 2005	2.43*	1.82+	0.71	0.31	-0.74*	-0.79+	-1.22**	-0.99**	-1.13+	-0.44	-0.05	0.09
	(0.94)	(1.02)	(0.44)	(0.49)	(0.33)	(0.41)	(0.20)	(0.24)	(0.66)	(0.70)	(0.10)	(0.29)
Control group	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military
PTT, 2000–2003: p	0.001	0.004	0.001	0.140	0.466	0.222	0.000	0.000	0.600	0.914	0.076	0.145
SIT: p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Equality test: p	0.220	0.456	0.807	0.855	0.056	0.013	0.398	0.705	0.852	0.912	0.356	0.314
Mean deviation test: p	0.554	0.648	0.840	0.789	0.224	0.256	0.480	0.558	0.652	0.952	0.151	0.140
Cells	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236
Clusters	1,646	45	1,646	45	1,646	45	1,646	45	1,646	45	1,646	45
Adj. R^2	0.265	0.479	0.102	0.155	0.060	0.116	0.111	0.223	0.360	0.539	0.213	0.360

Notes. Coefficient estimates*100 displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.27: Effects on mothers' characteristics: Tobacco use

	Smoker		Unknown status		1–5 cigs		6–10 cigs		11–20 cigs		21–40 cigs		40+ cigs	
Jan.–Apr. 2004	-0.28	-0.73	0.20	0.15	-0.01	-0.13	-0.15	-0.21	0.01	-0.17	0.03	0.00	-0.01	-0.01
	(0.93)	(0.97)	(0.16)	(0.23)	(0.33)	(0.35)	(0.38)	(0.40)	(0.48)	(0.50)	(0.06)	(0.07)	(0.01)	(0.01)
May–Aug. 2004	-0.23	-0.87	0.13	0.10	0.71*	0.65*	-0.65	-0.97	-0.22	-0.33	-0.01	-0.08+	0.01**	-0.00
	(0.68)	(0.72)	(0.15)	(0.24)	(0.28)	(0.32)	(0.73)	(0.76)	(0.22)	(0.24)	(0.04)	(0.04)	(0.00)	(0.00)
Sept.–Dec. 2004	-0.08	-0.70	-0.05	-0.04	0.50*	0.38	-0.62	-0.72	-0.40	-0.66*	-0.00	-0.05	0.00	0.00
	(0.46)	(0.51)	(0.11)	(0.21)	(0.25)	(0.29)	(0.54)	(0.56)	(0.28)	(0.29)	(0.05)	(0.05)	(0.00)	(0.00)
Jan.–Apr. 2005	-0.60	-1.17+	0.30*	0.17	-0.13	-0.23	0.09	-0.03	-0.24	-0.47	0.10	0.06	-0.01	-0.02
	(0.51)	(0.59)	(0.14)	(0.25)	(0.16)	(0.20)	(0.67)	(0.71)	(0.37)	(0.40)	(0.08)	(0.09)	(0.01)	(0.01)
May–Aug. 2005	0.45	-0.33	0.22+	0.15	0.48	0.44	0.08	-0.27	-0.07	-0.33	0.00	-0.06	0.00*	0.00
	(0.97)	(1.01)	(0.12)	(0.24)	(0.42)	(0.45)	(0.75)	(0.77)	(0.37)	(0.39)	(0.04)	(0.05)	(0.00)	(0.00)
Sept.–Dec. 2005	-1.09	-1.31	0.04	0.03	0.35	0.47	-0.51	-0.56	-0.11	-0.30	0.01	-0.04	0.00*	0.00
	(1.21)	(1.26)	(0.12)	(0.20)	(0.35)	(0.39)	(0.80)	(0.82)	(0.41)	(0.43)	(0.05)	(0.05)	(0.00)	(0.00)
Control group	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military
PTT, 2000–2003: p	0.000	0.002	0.679	0.112	0.374	0.166	0.009	0.073	0.000	0.074	0.259	0.677	0.304	0.438
SIT: p	0.000	0.000	0.036	0.081	0.000	0.035	0.000	0.024	0.000	0.108	0.000	0.097	0.459	0.684
Equality test: p	0.281	0.612	0.072	0.235	0.121	0.073	0.039	0.504	0.911	0.934	0.377	0.300	0.595	0.447
Deviation test: p	0.162	0.344	0.124	0.130	0.504	0.573	0.013	0.887	0.701	0.830	0.365	0.254	0.338	0.252
Cells	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236	115,708	3,236
Clusters	1,646	45	1,646	45	1,646	45	1,646	45	1,646	45	1,646	45	1,646	45
Adj. R^2	0.784	0.871	0.317	0.262	0.481	0.705	0.613	0.722	0.513	0.554	0.147	0.109	0.009	0.001

Notes. Coefficient estimates*100 displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$.

TABLE 3.28: Effects on mothers' characteristics: Prenatal visits, weight gain, obstetrics

	# prenatal visits		Weight gain < 16		Weight gain > 60		C-section		Induced labor	
Jan.–Apr. 2004	-35.01*	-22.21	-0.21	-0.46	-0.18	-0.34	-2.27	-2.02	-0.15	-3.39
	(13.65)	(19.11)	(0.72)	(0.92)	(0.34)	(0.36)	(1.76)	(1.81)	(1.39)	(2.13)
May–Aug. 2004	-39.59**	-41.97*	4.08+	4.48+	-0.13	-0.08	-0.70	-0.84	0.59	-2.22
	(14.22)	(18.09)	(2.33)	(2.41)	(0.47)	(0.50)	(1.55)	(1.60)	(1.06)	(1.65)
Sept.–Dec. 2004	-35.32**	-36.69*	1.42	1.60	-1.23**	-1.27*	0.12	0.16	3.77	2.24
	(12.90)	(15.41)	(1.57)	(1.63)	(0.45)	(0.49)	(1.82)	(1.88)	(3.58)	(3.73)
Jan.–Apr. 2005	-42.82+	-46.75+	2.12*	2.15+	-0.17	-0.37	-3.06*	-3.62*	5.10	3.09
	(24.02)	(27.70)	(1.04)	(1.16)	(0.75)	(0.78)	(1.53)	(1.57)	(4.47)	(4.68)
May–Aug. 2005	-26.42**	-32.91*	1.06	1.72	-0.40	-0.42	0.06	0.02	4.88	3.05
	(7.93)	(15.50)	(1.03)	(1.17)	(0.43)	(0.45)	(1.40)	(1.45)	(3.37)	(3.58)
Sept.–Dec. 2005	-20.52	-38.23	0.71	1.22	-0.13	-0.04	0.76	0.81	3.07	2.26
	(15.84)	(24.07)	(0.48)	(0.81)	(0.21)	(0.24)	(3.16)	(3.23)	(2.42)	(2.57)
Control group	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military	Baseline	Military
PTT, 2000–2003: p	0.037	0.536	0.124	0.487	0.024	0.132	0.409	0.705	0.065	0.016
SIT: p	0.000	0.003	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Equality test: p	0.363	0.742	0.058	0.515	0.764	0.678	0.000	0.000	0.478	0.876
Mean deviation test: p	0.686	0.485	0.657	0.967	0.506	0.593	0.525	0.468	0.282	0.637
Cells	115,629	3,236	112,159	3,019	112,159	3,019	115,683	3,236	115,698	3,236
Clusters	1,646	45	1,598	42	1,598	42	1,646	45	1,646	45
Adj. R^2	0.772	0.701	0.371	0.615	0.134	0.254	0.452	0.599	0.677	0.766

Notes. Coefficient estimates*100 displayed. Standard errors, in parentheses, are clustered by county. All models include year and calendar month indicators. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

TABLE 3.29: Birth rates around the BRAC 2005 announcement

	1	2	3	4	5
Jan.–Apr. 2004	0.042^{+}	0.055^{*}	0.045^{+}	0.036	0.027
	(0.024)	(0.024)	(0.024)	(0.025)	(0.024)
May–Aug. 2004	0.023	0.033	0.027	0.012	0.017
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Sept.–Dec. 2004	0.030	0.035	0.034	0.008	0.027
	(0.024)	(0.024)	(0.024)	(0.025)	(0.024)
Jan.–Apr. 2005	0.037	0.047^{+}	0.043^{+}	0.030	0.032
	(0.024)	(0.024)	(0.024)	(0.025)	(0.024)
May–Aug. 2005	-0.010	0.002	-0.004	-0.008	-0.010
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Sept.–Dec. 2005	0.028	0.041^{+}	0.026	0.014	0.020
	(0.024)	(0.024)	(0.024)	(0.025)	(0.024)
Control group	Baseline	Minor	Major	Military	States
Equality test: p	0.329	0.363	0.369	0.553	0.444
Mean deviation test: p	0.142	0.158	0.185	0.323	0.217
Cells	118,512	6,768	10,008	3,240	17,280
Clusters	1,646	94	139	45	240

Notes. Coefficient estimates from Poisson conditional fixed-effects models. Models include terms for year and group-interacted calendar month. The equality test has the null hypothesis that all three coefficients in 2005 are equal. Deviation test explained in main text. Standard errors, in parentheses, are clustered by county. Statistical significance symbols: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

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