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Biosecurity, Terrorism, and Food Consumption Behavior: Using Experimental Psychology to Analyze Choices Involving Fear

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How would a possible food safety scare influence food consumption? Using techniques from experimental psychology, a study of 103 lunchtime participants suggests that a food scare—avian influenza—would decrease consumption of the affected food by 17% if the subjects believed it was naturally occurring, and by 26% if they believed it was the result of terrorism. While individual consumption decreased, very few eliminated all consumption of the affected food. We argue that experimental psychology is essential when attempting to study behavior in food safety where hypothetical scenarios and surveys would not capture the emotional nature of the response.

Key words: avian influenza, experimental psychology, food safety, terrorism

Introduction

As food safety has become a headline issue, economists have struggled to find practical ways to assess consumer response to potential future threats to our food system. Food safety incidents, such as the outbreak of salmonella due to contaminated jalapeno peppers or peanut butter, are singular events. Consequently, while studying one outbreak using historic data may reveal some important behavior, it is difficult to generalize any results from such an approach to determine which characteristics of the outbreak drove consumer responses.

Two primary alternative methods are generally used to study food safety threats. First, survey methods allow investigators to ask consumers about hypothetical threats. Investigators can then manipulate the characteristics of the potential threat (e.g., the location of discovery) to more directly assess their influence across different segments of consumers (Turvey, Onyango, and Hallman, 2007). For example, when respondents to a national survey were asked about a hypothetical outbreak of avian influenza (AI) in the United States, their reported perceived risk was acute enough to imply that poultry markets would be devastated for at least several months (Condry et al., 2007). Yet, past research has also shown that consumers can be substantially biased when responding to hypothetical questions (Bradburn, Sudman, and Wansink, 2004). Thus,

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even with such dramatic survey responses, we are left to wonder "to what extent are these responses real?"

A second approach for studying food safety crises has been economic experiments that have sought to determine a consumer's willingness to pay for food safety information (e.g., Hayes et al., 1995). While this method eliminates hypothetical bias, it cannot be used to assess consumer behavior *after* a food safety threat has already occurred. Because of the direct risk to participants of consuming contaminated food, an economic experimenter cannot ask participants if they will eat food that has an increased risk of contamination in an incentive-compatible way.

We introduce an alternate method of assessing behavior in response to a food safety risk. Our method, derived from experimental psychology, can be used to directly assess how individuals will alter consumption choices when responding to a food safety incident. A key feature of this method is the use of deception, which is typically forbidden in economic experiments. This decision was not taken lightly. As we argue in the following sections, in situations where personal safety is at risk, deception enables us to study a new class of economic problems and may present substantial benefits to society. We propose some key characteristics of the problem that indicate when deceptive methods may be justified as an alternative to economic experiments. We describe a food choice experiment conducted in the fall of 2007 at Cornell University (Ithaca, New York). The experiment was designed to measure the actual response of participants to a perceived biosecurity threat related to the discovery of bird flu at a local processor. Information about the bird flu outbreak was delivered through the use of a confederate posing as a participant. The weight of food consumed by each participant was recorded and provides our primary measure of risk response.

This methodology and its results are notable for several reasons. First, while there is a significant literature on survey responses to hypothetical food risks, we present a novel experimental protocol that moves the investigation to the level of actual behavior. Second, because terrorism has historically played only a small role in food contamination (Turvey, Onyango, and Hallman, 2008), it has been difficult to gauge potential consumer response to a terrorism incident. Our approach enables us to contrast the psychological response to naturally versus intentionally occurring risks. Finally, as we consider the conventional approaches by agricultural and food economists to the investigation of food security risks, this study provides a fresh approach to economic investigation by using tools of psychology in a controlled laboratory situation.

The paper begins with a background discussion on biosecurity in the context of natural versus intentional (terrorist) food risks. A brief discussion is then provided of the psychology and economic principles governing choices involving fear. Next, we review the economic literature that has investigated deception and experimental protocols. Our experimental protocol is then presented and the results summarized and discussed.

Background

Food security is an integral part of U.S. food policy, including under its umbrella a range of activities from food safety, certification of food handlers, bioterrorism, and biosecurity to economics. Derrickson and Brown (2002) report the definition of food security according to the Life Science and Research Office of the American Societies of

Experimental Biology as "the assured ability to acquire safe, nutritious, socially, and culturally-acceptable foods," with the key phrase being the use of the term "safe." Likewise, they define food insecurity as "whenever the availability of nutritionally adequate and safe foods or the ability to acquire foods in socially acceptable ways is limited or uncertain." Keenan et al. (2001) provide a similar definition. Threats to food security can be divided into two primary sources: (*a*) threats that are naturally occurring due to incidental or neglectful contamination in the normal processing and production of food, and (*b*) those occurring due to intentional human interference such as terrorist action. Examples of naturally occurring threats are plentiful, providing many opportunities for historical data analysis. Alternatively, intentional food security threats are extremely rare, making comparisons or analysis of government response strategies difficult.

The incidence of disease from foodborne pathogens is a chronic problem and a constant challenge to food security (Penings, Wansink, and Meulenberg, 2002; Hennessy, 2008). Reports from the Centers for Disease Control (Mead et al., 1999) estimate that the total annual number of reported and unreported cases in the United States could be as high as 76 million, with 323,000 hospitalizations and over 5,000 deaths annually. Of 38.6 million reported (or verifiable) cases, there were 181,000 hospitalizations and 2,718 deaths annually.

Understanding how consumers respond to a food safety risk is also important to the study of agricultural and food economics because the economic consequence of a food safety risk can be staggering. For example, in 1983 and 1984, a highly contagious avian influenza (AI) epidemic led the government to require the complete depopulation of chickens and decontamination of premises in Pennsylvania. Eradicating the pathogenic strain of AI in Pennsylvania cost about $465 million in direct costs and $150 million in lost trade. So significant was its effect on supply that it contributed to a $349 million rise in turkey, chicken, and egg expenditures in the first six months of the outbreak. It is also estimated that, absent repopulation, the economic costs would have risen to $5.4 billion (1984) dollars (Brown, 1999). Similar actions resulted from a 2002 outbreak in Virginia. Failing to prevent food contamination can also damage perceived reputation and quality and credence (Caswell and Mojduszka, 1996), increase the costs of product liability litigation (Buzby, Frenzen, and Rasco, 2001; Lenain, Bonturi, and Koen, 2002), and result in the loss of market value of company stock (Salin and Hooker, 2001; Wang et al., 2002) and the loss of export markets (Nitsch and Schumacher, 2004; Hennessy, 2008).

The American agriculture and food system presents a tempting target for terrorist attacks (Hennessy, 2008; Sandler and Enders, 2002; Owens, 2002; Foxell, 2001; Cameron and Pate, 2001; Chalk, 2001). Several studies have suggested that the threat of agroterrorism could negatively impact food consumption behavior. Based on evidence from an agroterrorism survey (Turvey, 2006), risk perceptions increase with proximity to the threat, and intended consumption might be slow to rebound after such a threat.

Industry and government have made large legislative, regulatory, and research investments designed to identify, reduce, and eliminate threats posed at key points in the chain (Sheffi, 2001; Ollinger and Ballenger, 2003). These include (*a*) improved quality control measures throughout the food supply chain (Antle, 2000), (*b*) traceability of food through mandated country-of-origin labeling (COOL), (*c*) FDA regulations implementing the 2002 Bioterrorism Act, and (*d*) the USDA's National Animal Identification

System. Many of these efforts overlap with general efforts made to stem food safety risks, such as the Hazard Analysis Critical Control Points (HACCP) system (USDA/Food Safety and Inspection Service, 1996; Nganje and Mazzacco, 2000). Furthermore, some have argued for increased shipment visibility through radio frequency identification device (RFID) tracking (Collins, 2004), and greater collaboration along the food supply chain, risk pooling, and increased reliance on public/private partnerships in information sharing and security (Sheffi, 2001).

All of these investments serve to improve government response and increase consumer trust in the food system. In the end, however, it is the consumer's response to the threat that will determine the effectiveness of government policies. For example, complete trust in the food system may lead to sluggish consumer response when warnings are issued, which would further exacerbate the health consequences of the threat. In order to make government and industry reaction effective, we must be able to accurately anticipate how consumer response will interact with a food safety threat.

Risk Perceptions and the Psychology of Food Consumption

To a large extent, our experimental protocol follows, or was inspired by, the varied experiments reported in Wansink (2004, 2006) relating to food consumption and decision making. How consumers perceive food safety risks from either purchasing food that is potentially tainted through natural occurrences or through bioterrorist attacks depends upon their perceptions of risk—real or imagined—and their attitude toward risk (Wansink, 2002). Here, risk perceptions are a powerful economic variable (Pennings and Wansink, 2004). If consumers overestimate the risks from particular foods, food producers miss out on potential profits while consumers miss out on beneficial consumption (Pennings, Wansink, and Meulenberg, 2002). On the other hand, if consumers underestimate the risks associated with a particular food, they expose themselves to unanticipated health consequences and food producers may face additional legal or other costs from coping with associated potential liability.

There is good reason to question the accuracy of a consumer's risk perceptions. In a 1996 Food Market Institute report, 98% of consumers knew that harmful bacteria were found in meat and poultry, but only 74% made the link to dairy and egg products. Furthermore, only 43% recognized that fruits and vegetables may contain harmful bacteria. By understanding consumer risk perceptions, food establishments can be better advised of the potential strategies for dealing with food safety incidents. The consequences of this are sizable. For example, if consumer caution is expressed by a reduction in quantity consumed rather than a cessation of eating potentially exposed foods, individuals may maintain their pre-incident level of exposure while severely reducing producer profits. This is a primary motivation for the current research.

Using Surveys to Estimate Food Safety Risks

Risk perception relates to the cognitive ability of humans to perceive and judge risks. Its emergence as a field of study has often been described as a challenge to the rationality of the expected utility hypothesis (Arrow, 1982) in that it requires the consistent use of conditional probabilities to revise beliefs as new information evolves over time—a tedious task for casual consumers. Alternatively, the representativeness heuristic of Tversky and Kahneman (1974, 1981) contends individuals take any data to be representative of the process that generated it—no matter what the sample size. Thus individuals may overstate the probability of states of nature that are particularly similar to current evidence. This leads individuals to make very big decisions based on very little evidence, and in turn may lead to short-term overreaction to food safety scares.

How and *what* people think about the threat of exposure to a microorganism, the experience of the physical disease state, and the consequences of their illness are differentiated within their cognitive understanding (Leventhal, Meyer, and Nerenz, 1980; Leventhal, Nerenz, and Steele, 1984). Economic researchers have typically treated food hazards as a single phenomenon (e.g., a hypothetical outbreak of salmonella) without specifying the type of information available at the time of the food decision. As discussed previously, the primary method of measurement has been the use of surveys in which individuals are asked a variety of "risk" questions pertaining to specific events, items, or activities (e.g., a risky event or action) which can measure different aspects or test specific hypotheses regarding risk attitude (i.e., dread, severity, knowledge, etc.) and risk perception.

An alternative survey approach has been to investigate how consumers responded to a food security threat after the fact. Following the first U.S. mad cow case in December 2003, a nationwide survey indicated that consumer response was modest at worst, with a predictive return to normal consumption by the summer of 2004—a prediction that was borne out in reality (Schilling, Hallman, and Turvey, 2004). Likewise, a recent survey by Rutgers University in response to the 2006 spinach recall showed that of the 154 respondents who had fresh spinach on hand, 19.2% ate it with knowledge of the recall while 74% did not consume it[1] (see also Cuite et al., 2007). These provide examples of individuals who consume foods that are potentially contaminated after the food scare is widely publicized, but the differentiated responses cannot easily be generalized in economic terms. It may be that individuals have a hard time resisting the consumption of foods they have already purchased, which implies a blurry, but important, distinction between the economy of choice and the psychology of choice.

Using Auctions and Experiments to Estimate Food Safety Risks

Despite the widespread tendency to use surveys to estimate food safety risks, there has been a recent move toward applying principles of experimental economics and auctions to address the problem. For instance, Hayes et al. (1995) used food auctions to determine consumer willingness to pay for food safety information. These experiments endowed participants with food that had been purchased from a local eatery. Then, subjects participated in an auction mechanism that would allow participants to upgrade to similar food for which the probability of contamination had been controlled. Hayes et al. found subjects were willing to pay about $0.70 on average per meal to improve their food safety information.[2] Such information is very important in determining the

[1] These data were provided to the authors from the Food Policy Institute, Rutgers University.

[2] Lusk and Shogren (2007, pp. 121–129) provide a review of this and several other studies examining the value of food safety.

cost-effectiveness of food safety measures. Further, by placing individuals in a non-hypothetical setting—one they perceive as real—they achieve a high degree of congruence with the real-world decision context and thus better predictive power. When congruence with real-world decisions is possible, the laboratory provides a clear advantage in assessing behavior. Hayes et al. also report that consumers tend to underestimate the risk of foodborne illnesses. This is despite the common observation that individuals overestimate the probability of rare events (see, e.g., Kahneman and Tversky, 1979).

Natural human behavior can present a severe challenge in containing a threat whether it originates in nature or at the hands of man. Our experiment seeks to elucidate whether a food threat *actually* impacts food intake. Specifically, our intent is not to determine whether food biosecurity surveys of a hypothetical nature such as those reported in Condry et al. (2007), or in Turvey, Onyango, and Hallman (2007), would actually come to pass, but whether, under controlled conditions, even the *suggestion* of a possible problem can affect actual consumption behavior. Our basis is founded on much of the food psychology literature which suggests individuals have a very difficult time resisting foods either when they are in view (Boon et al., 1998; Cornell, Rodin, and Weingarten, 1989), or that they have already purchased and currently are present within the household (Chandon and Wansink, 2002). Other experiments documented in Wansink (2006) reveal that overt suggestion is sufficient to influence food consumption even if respondents explicitly deny that the suggestion had any influence at all. It is important, therefore, to determine if the threat of contamination can overcome the natural tendency of individuals to consume rather than discard or abandon a food.

Deception in Experimental Economics and Psychology

Surveys and ex post analyses of foodborne illness outbreaks have been the primary mode of investigating consumer response to food contamination. Yet much of the food psychology literature documents how individuals are either unaware of their behavior (Wansink, 2006) or systematically underestimate and underreport their consumption (Wansink and Kim, 2005). Thus, surveys based on consumer perceptions may not reveal much about behavior occurring after a food contamination scare. Ex post analysis of a particular food scare faces another set of challenges in that they cannot be controlled or randomized. For instance, an *E. coli* scare linked to spinach in the Northeast may not tell us much about what would happen if different information were available regarding the source, the extent of contamination, or the seriousness of the disease associated with contamination.

Experimental methods have been shown to be important tools for addressing such issues and creating a base of research upon which to build food policy. Experiments allow for control and the opportunity to observe actual behavior of consumers facing a food contamination threat. For this reason, we designed a food psychology experiment which allows us to control the information given to participants. Unfortunately, conventional experimental economics in its current orthodoxy cannot be used in this context. Experimental economics discourages the deception of participants, which is necessary to convince individuals a threat has occurred unless experimenters actually plan on carrying out a threat themselves. For this reason we turn to methods that are commonplace in studies of psychology and food behavior.

The arguments against deception in economic experiments are well established. As Davis and Holt (1992, pp. 23–24) argue, by deceiving subjects, economists may gain a reputation for lying to participants. If this is the case, then participants may not believe in the experimental incentives used to test economic behavior. This sentiment is echoed by Ledyard (1995) and also by Hey (1991), who draws the distinction between experimental economics and experimental psychology, suggesting deception is expected by subjects in experimental psychology. Hey (1998) further contends that as soon as any subject suspects deception is employed, the experimenter has lost the clean control for which the experiment was employed. If subjects do not believe in the incentive structures they face, they will not behave according to them. Finally, if economic experimenters obtain a reputation for deceiving subjects, the subject pool may be invalid for all future experiments.

Bonetti (1998) argues deception may be useful and beneficial to experimental economists. In particular, he suggests the use of deception to (*a*) hide the research hypothesis, and (*b*) fool subjects into believing that other subjects have behaved in a certain way. The latter is necessary to examine individual reaction to rare behavior on the part of other players (for example, in a financial markets experiment). Further, Bonetti points out that little has been done to examine the impact of deception in economic experiments due to the common prohibition.

In contrast to economists, psychologists have long used deception within their experiments. They also have a substantial literature examining the validity and effects of deception on subject response. For example, in two similar experiments, Finney (1987) and Chipman (1966) compare subjects who had been informed of the possibility of deception to those who had not, finding no significant difference in behavior. Bonetti (1998) reviews more than a dozen psychology experiments examining the use of deception, with nearly all concluding that suspicion of deception has little impact on performance. Moreover, we suggest that arguments for reputation effects are tenuous at best. In order for subjects to believe deception is impossible in an economic experiment, subjects must first have the ability to differentiate between economic and psychology experiments.

Subjects are unlikely to participate in enough research experiments to acquire this sort of experiential learning. Even if they did, serial participants often perform differently than first-time participants, raising questions about validity of their repeated behavior. Secondly, maintaining this public good requires not only that economists not deceive their subjects, but that no other experimentalists take the name of economics in vain. For example, psychologists may deceive their subjects into believing they are participating in an economic experiment in order to test some hypothesis involving deception. Such a rule is very difficult to enforce on other disciplines.

We contend the use of deception should be evaluated based on its costs and benefits. The previous discussion documents many of the costs involved—i.e., questions of validity if a deception is suspected, and contamination of the subject pool for future experiments. Yet in certain circumstances, the benefits may outweigh the costs. In the case of response to health-threatening food safety, deception may be the best if not the only way to confidently proceed. Food safety scares have tremendous consequences that are measured in lives as well as in dollars. An understanding of individual response to a scare could prove to be invaluable. Further, the rare nature of food safety scares (and particularly those due to terrorism) prevents satisfactory study outside of a laboratory experiment or a hypothetical survey.

Like hypothetical surveys, which also allow the researcher control over the food scare scenario, a food experiment involving deception faces a severe validity challenge. Subjects facing a hypothetical food scare may have a difficult time predicting their own behavior, and bias their results accordingly. As long as subjects believe in a deception, such a potential bias can be overcome. Alternatively, if participants in an experiment suspect the food scare is a perpetrated hoax, results may understate the true magnitude of their consumer response. Careful manipulation checks and debriefings would be necessary to determine if the intervention was convincing enough to generate a genuine response.

Experimental Methodology

We focus on how the source of risk may influence risk response. Under controlled conditions in which consumers are offered identical risks but with some cohorts informed of a possible terrorist link, the psychological profiles are considerably different. We hypothesize generally that while individuals will reduce their consumption of a potentially contaminated food, they will not cease to consume altogether. The sinister side of terrorism may lead some to believe that a more damaging contaminant is present than would most often occur naturally. For this reason, we hypothesize that consumers will respond more dramatically to a terrorist threat than to a naturally occurring threat. Such behavior could present a tremendous challenge to policy makers in securing the food supply. In particular, if a contaminant is highly concentrated, consuming even small amounts could have substantial health consequences.

In our lunchtime experiments, participants were placed in a situation where they were required to choose whether or not to eat chicken that might be tainted with AI. This disease was chosen for two reasons. First, the disease has been widely reported on in the news, with much of the news anticipating outbreaks in the United States and eventual transfer to humans. Yet the reporting has not been in such detail that a casual observer would know much about it. This prior level of information lends credibility to the notion that there may have been an outbreak. Secondly, the disease cannot be spread to humans through consumption of cooked chicken. Hence, we could provide a plausible explanation regarding why the contamination did not concern us. The experiment consisted of three conditions, which we refer to here as CONTROL, NATURAL, and TERROR.

Participants were recruited for a "food marketing study," and promised $5 and a meal for their participation. Each session took place at 12:30 on a Tuesday, Wednesday, or Thursday. Subjects were directly informed that the experiment would be conducted by a food psychologist who is a member of the applied economics faculty and that the experiments were *not* associated with the experimental economics laboratory.[3] Participants entered a room featuring a buffet line to their left and a set of three long tables (seating up to 20 persons each) arranged in a "U" shape on their right. Participants were asked to enter the buffet line and select as much as they liked of each of the foods: boneless fried chicken tenders, French fries, pudding, apple sauce, celery, macaroni salad, soda, and bottled water. Subjects were instructed to take at least a little of each item, and each item was to be placed on a separate small plate on their tray. At the end

[3] In addition, the experiments were conducted in a different physical location than the experimental economics laboratory.

of the buffet line, all plates were weighed individually, and participants were then told to be seated at one of the three long tables on the outer edge of the "U" and to wait for instruction from the experimenter before beginning to eat. By sitting on the outside of the "U", each individual could easily see and hear all of the other participants.

After all participants were seated, one of the experimenters checked each tray to make sure everyone had complied with instructions. Any participants who had failed to do so were sent back to the buffet to complete the instructions. Unbeknownst to the participants, each session contained a confederate who posed as a participant but who was actually a paid professional actor. This 23-year-old male had been advised not to comply with the instructions and to create a carefully scripted mild disturbance. In each of the three treatments, he failed to take any chicken. One of the experimenters would ask the confederate to return to the buffet and take some chicken.[4] The exact script can be found in the appendix. Experimenter roles were carefully maintained so that the scripted dialogue was performed in as similar a manner as possible under each treatment.

In the CONTROL condition, the confederate replied he was a vegetarian, and did not feel comfortable taking any meat. The experimenter then told the confederate that taking at least a little meat was required for the study, but that he could choose not to participate and still receive the $5 participation fee. At this point the confederate apologized and excused himself. One of the other experimenters gave him $5 and filled out the necessary receipt forms in view of the other participants (though not in a central location).

Under the NATURAL condition, the confederate replied that a bird flu outbreak at an Auburn (New York) chicken farm (Auburn is approximately 45 miles from where the study was being conducted) was reported on the news that morning, and he was afraid to even touch the chicken. The experimenter then acknowledged he had seen the report, but asserted that AI could not be spread by consumption of chicken. The confederate stated that he did not feel comfortable taking any chicken. The experimenter then told the confederate that taking at least a little meat was required for the study, but he could choose not to participate and still receive the $5 participation fee. At this point the confederate apologized and excused himself. One of the other experimenters gave him $5 and filled out the necessary receipt forms in view of the other participants (though not in a central location).

The conversation between the confederate and the experimenter in the TERROR condition was nearly identical to that under the NATURAL condition, except that in telling about the news story, the confederate mentioned it was suspected terrorists were involved.

In all three conditions the confederate was seated near the center of the other participants, and all participants were aware of the approximately 90-second interaction between the confederate and the experimenter. After the confederate left, the experimenter informed the remaining subjects to begin eating. After completing their meals, the participants' plates were again weighed to determine how much of each item had been consumed. Each subject was then asked to respond to a survey. Following completion of the survey, participants were debriefed in small groups and were asked to discuss their experience with the experiment and provide their impressions.

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[4] Each of the following conversations was scripted. To conserve space, we present summaries only. The conversations in the NATURAL and TERROR conditions differed only by the mention of terrorism.

Table 1. Summary Statistics for All Treatments

Variable	Treatment				p-Value
	All	CONTROL	NATURAL	TERROR	
Gender (1 = female)	0.521 (0.498)	0.400 (0.495)	0.653 (0.485)	0.531 (0.507)	0.200
Body Mass Index (BMI)	21.638 (4.598)	21.579 (4.347)	21.431 (4.156)	22.314 (3.295)	0.930
Weight (lbs.)	147.841 (35.917)	148.420 (33.892)	144.625 (21.856)	142.667 (29.324)	0.699
No. of Observations	85	29	26	30	

Note: Values in parentheses are standard deviations.

Experiment Results

A total of 85 participants completed the experiment. Summary statistics for all treatments can be found in table 1. As would be expected because of random assignment to the conditions, the samples were similar in body mass index (BMI), weight, and gender, suggesting the experiment presents a valid control for external factors. Debriefing interviews and discussions with participants after the experiment indicated that all subjects were aware of the interaction between the confederate and the experimenter. Furthermore, an overwhelming majority of participants believed the confederate was sincere, though some described him pejoratively as a "hippie." Indeed, based on manipulation checks, those in the NATURAL and TERROR treatments believed there was a larger chance of the chicken being contaminated (an average difference of 1.22 on a 9-point Likert scale, $p = 0.000$).[5] While most agreed with the statement, "I didn't believe the chicken had bird flu" (average of 7.864 on a 9-point Likert scale), those in the NATURAL and TERROR treatments were slightly less likely to agree ($p = 0.001$). This finding seems to support the general statements by participants that they did not think the confederate had influenced their consumption of the food they selected.

Other questions showed a similar pattern of results, with those in the NATURAL and TERROR conditions being slightly more likely to agree with the statements, "I believed this was partially infected with bird flu" (average for CONTROL = 1.34, and tainted treatments = 1.67, $p = 0.075$), and "I believed there was at least a small chance of contamination" (average for CONTROL = 1.59, and tainted treatments = 3.00, $p = 0.002$). Alternatively, participants in the tainted treatment groups were no less likely to agree with the statement, "I believed there was a low risk of me contracting bird flu" (average for CONTROL = 6.12, and tainted treatments = 5.94, $p = 0.974$).

Other survey questions examined individual opinions toward general food safety and bird flu. Those in the tainted treatment groups were slightly less likely to agree with the statement, "I believed eating chicken with bird flu would be harmful to me," though the

[5] All p-values reported result from nonparametric difference in means tests. These results are more accurate but virtually identical to those derived from more conventional tests. All Likert scale questions record a 9 for "strongly agree" and a 1 for "strongly disagree," with participants able to choose any number 1 through 9. In each case there is no significant difference between the NATURAL and TERROR treatments. Hence, both are combined and reported as the "tainted treatments."

difference was insignificant (average for CONTROL = 6.44, and tainted treatments = 5.78, $p = 0.126$). All three groups demonstrated a degree of uncertainty regarding the link between eating chicken infected with bird flu and contracting the disease, as evidenced by the mid-range responses. Interestingly, those in the NATURAL and TERROR treatments were significantly more likely to agree with the statement, "When eating chicken, I am exposed to much risk" (average for CONTROL = 2.12, and tainted treatments = 3.61, $p = 0.003$). This finding suggests the personal experience of deciding whether to eat potentially infected chicken may have raised participants' concern or perception of risk, but it did not prevent them from consuming a reasonable percentage of the chicken.

To determine the motivating factors for eating chicken, participants responded to another series of Likert-scale questions with statements of the form, "I ate the chicken today because: ..."

- "It tasted okay" (average for CONTROL = 7.34, and infected treatments = 5.84, $p = 0.005$).
- "I didn't believe it had bird flu" (average for CONTROL = 8.38, and infected treatments = 6.97, $p = 0.003$).
- "I didn't think it would hurt me" (average for CONTROL = 8.52, and infected treatments = 7.25, $p = 0.011$).
- "The chicken didn't taste funny" (average for CONTROL = 7.28, and infected treatments = 6.34, $p = 0.091$).
- "It was safe" (average for CONTROL = 8.02, and infected treatments = 6.87, $p = 0.040$).

In each case those in the infected treatments displayed significantly weaker agreement with the motivations mentioned, suggesting their beliefs regarding the safety and purity of the chicken had been altered by our experiment.

Table 2 compares consumption of chicken and other foods for the various treatments. When analyzing consumption, we use the percentage of food taken that was actually consumed rather than analyzing the absolute amount of food consumed (a difference measure). The reason for analyzing the percentage is because it naturally enables us to control for the individuals' expectations about how much they would eat prior to the treatment effects. This procedure will naturally control for differences in consumption volume due to physiological differences, making the resulting consumption data more uniform (Bradburn, Sudman, and Wansink, 2004). Within the CONTROL treatment, chicken consumption was in excess of 90%, which is similar to the 92% generally reported in previous food choice experiments (Wansink and Cheney, 2005). Consumption of sides in all treatments appears to be lower than in previous experiments. This may be due to the requirement that participants were to take a small amount of each food.

Table 2 shows the expected relationship between chicken consumption and treatment. Participants in the NATURAL condition consumed a significantly smaller percentage than those in the CONTROL, while those in the TERROR treatment consumed a significantly smaller percentage than those in either the NATURAL or CONTROL treatments. We observe no significant differences between consumption of other foods in the

Table 2. Percentage of Food Consumed, by Treatment

		NATURAL		TERROR		
Food Consumed	CONTROL	Mean	p-Value vs. CONTROL	Mean	p-Value vs. CONTROL	p-Value vs. NATURAL
Chicken	0.919 (0.139)	0.805 (0.266)	0.022**	0.686 (0.271)	0.000***	0.079*
French Fries	0.562 (0.370)	0.693 (0.344)	0.426	0.441 (0.299)	0.184	0.003***
Macaroni Salad	0.493 (0.392)	0.572 (0.369)	0.459	0.418 (0.358)	0.661	0.354
Celery	0.465 (0.419)	0.535 (0.379)	0.684	0.471 (0.406)	0.798	0.372
Apple Sauce	0.499 (0.386)	0.569 (0.354)	0.478	0.649 (0.319)	0.114	0.337
Pudding	0.556 (0.404)	0.463 (0.353)	0.359	0.492 (0.392)	0.330	0.806

Notes: Single, double, and triple asterisks (*) denote statistical significance at the 10%, 5%, and 1% levels, respectively. Values in parentheses are standard deviations.

CONTROL and NATURAL conditions. Likewise, those in the TERROR treatment ate similar percentages of foods unrelated to the threat, though they did consume a significantly smaller percentage of their French fries than those in the NATURAL treatment. Table 3 displays OLS regression results for percentage of chicken consumed, controlling for age, gender, height, weight, and treatment. *Height*, *TERROR*, and *NATURAL* are the only significant explanatory variables, with *TERROR* reducing consumption by 26%, while *NATURAL* reduces consumption by only 17%.

Although we observe significant drops in the percentage of chicken eaten, this drop tended to occur at an individual level. Importantly, few participants reduced their consumption to zero when they heard about the threat. Table 4 summarizes the percentage of individuals within each treatment who consumed all, none, or some of their chicken. The table reveals there is no statistical difference by treatment between the percentage of individuals eating none of their chicken. However, there are surprisingly large declines in the percentage of participants who ate all of their chicken. Further, we see a tremendous jump in those who ate only some of their chicken. This result may not exactly mimic how individuals would behave in their home with food sitting in their refrigerator. For example, our treatments offered none of the other meat alternatives one might find in a typical home. Nevertheless, the results are highly suggestive that individuals think more directly about reducing their consumption than actually eliminating potentially contaminated foods.

Discussion

How would a concern about a possible food safety scare influence food intake and consumption? This study suggests the impact of such a food scare depends on whether people believe the threat was naturally occurring or whether it was a result of bioterrorism. Food safety threats attributed to terrorism result in a more dramatic decrease in consumption. Interestingly, however, any threat widely reduces intake, but it does not appear to eliminate consumption for most people in our experiment.

Table 3. OLS Regression Results: Treatment Effects on Percentage of Chicken Consumption

Variable	Coefficient	Standard Error	p-Value
TERROR	−0.261***	0.053	0.000
NATURAL	−0.169*	0.087	0.055
Age	0.006	0.010	0.553
Sex	−0.097	0.066	0.146
Weight (lbs.)	−0.001	0.001	0.423
Height (inches)	0.014*	0.008	0.080
Constant	0.082	0.636	0.897
$R^2 = 0.405$; adjusted $R^2 = 0.362$			

Note: Single, double, and triple asterisks (*) denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Percentage of Participants Consuming All, Some, or None of Their Chicken, by Treatment

		NATURAL		TERROR		
Chicken Consumed	CONTROL	Mean	p-Value vs. CONTROL	Mean	p-Value vs. CONTROL	p-Value vs. NATURAL
All	0.555 (0.506)	0.154 (0.368)	0.010**	0.129 (0.341)	0.005***	0.851
Some	0.444 (0.506)	0.769 (0.430)	0.013**	0.806 (0.402)	0.015**	0.778
None	0 (—)	0.077 (0.272)	0.640	0.065 (0.250)	0.169	0.925

Notes: Single, double, and triple asterisks (*) denote statistical significance at the 10%, 5%, and 1% levels, respectively. Values in parentheses are standard deviations.

New challenges and new questions in agricultural economics may encourage the use of fresh methods to provide insights or direction. This research introduces one such tool—food psychology consumption experiments—which may prove useful in understanding the individual behavior that will go on to manifest itself in market-level demand. Important controls and manipulation checks are necessary when conducting food psychology consumption experiments. Even then, it is important that the results be replicated with other such studies and be triangulated with multiple methods. Thus, other studies are necessary to validate the results we have obtained, and to further expand our understanding of consumer reactions to food safety scares.

This study is one of the first experimental studies to investigate how food safety concerns influence the consumption of immediately available food to hungry individuals. While food safety concerns might dampen the purchase of a food at the supermarket, it has a different impact at the point of consumption. Specifically, *when naturally hungry persons face food which may be contaminated, they may reduce their consumption of the food but they do not tend to avoid eating it altogether, if there are no close substitutes available.*

Of those participating in our experiment, only 6% to 8% in the infected treatments responded by refusing to eat the chicken they had taken. This small effect is likely to be only a temporary response, leading us to question if most surveys attempting to address this issue might unknowingly exaggerate the decrease in consumption they estimate might occur. If so, policy measures based on survey responses may fail to identify the most prominent risks to health and safety (e.g., continued exposure to contaminated foods already purchased).

Because there was no other meat immediately available, participants may have eaten some chicken when they otherwise would have avoided it. This is an important limitation to our findings. Participants could have purchased alternatives in one of the local eateries (the closest of which was four floors away) immediately following the experiment. However, this would come at some additional cost not imposed on the chicken we had offered. It is difficult to guess the magnitude of the effect of allowing seemingly safer close substitutes on chicken consumption. Future work should address this potential interaction with substitutes.

Within our experiment, information on the potential food safety threat was introduced by an actor portraying a university student. Individual responses are likely affected by the information source. For example, a government announcement may have a very different impact on immediate consumption. Further, the food served was delivered by a local restaurant just prior to consumption. Thus, the level of consumption may reflect an inherent trust that a food retailer would not provide potentially tainted food due to liability concerns. Nevertheless, we found significant decreases in consumption when the possibility of a food safety threat was introduced.

The fact that a naturally occurring food safety scare does not impact consumption as severely as a bioterrorism scare has policy implications. To date, most of the preparations for food safety threats assume a naturally occurring source of contamination. Given that bioterrorism may have a more dramatic influence, it challenges government entities to consider alternative channels of risk communication and perhaps different response patterns. For example, the most effective response would be one that identifies both the sources of contaminated food and the customers who have purchased such foods. Personalized warnings to affected customers may be much more effective than general news announcements. Additionally, modern communication and data technology have reduced the cost of such targeted announcements.

Finally, our paper has introduced a new technique for addressing food safety in economics—the use of psychology experiments. Psychology experiments allow us the freedom to deceive participants as is necessary to determine true responses to imminent threats. Such experiments should not be taken lightly, but benefits must be weighed against the cost to future subject pools and the potential for internal validity.

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Appendix:
Food Consumption Experiment Scripts

▪ *CONTROL Script*

EXPERIMENTER 1: Excuse me, we need you to take some chicken.

CONFEDERATE: I would rather not. I'm a vegetarian.

[Experimenter 1 calls Experimenter 2 from buffet line area.]

EXPERIMENTER 1 to Experimenter 2: He doesn't want to take any chicken.

EXPERIMENTER 2: Sorry sir, but for this study we need you to take some of each of the items. You could take a small amount if you like and we won't require you to eat it.

CONFEDERATE: I'm a vegetarian. I really wouldn't feel right taking any. I don't even want to go near it.

EXPERIMENTER 2: Well, if you would rather not, we won't be able to let you participate, but you may still collect the $5 participation fee we promised.

CONFEDERATE: Okay. Sorry, I just don't feel right about it.

[Experimenter 2 escorts the confederate to a third experimenter near the buffet line—away from the other subjects, but within their line of sight. He asks the third experimenter to give him his participation fee. The confederate is paid and instructed to fill out a receipt form and then leaves.]

▪ *NATURAL Script*

EXPERIMENTER 1: Excuse me, we need you to take some chicken.

CONFEDERATE: I would rather not. There was a story about a bird flu outbreak on the news this morning.

[Experimenter 1 calls Experimenter 2 from buffet line area.]

EXPERIMENTER 1 to Experimenter 2: He doesn't want to take any chicken.

EXPERIMENTER 2: Sorry sir, but for this study we need you to take some of each of the items. You could take a small amount if you like and we won't require you to eat it.

CONFEDERATE: There was a story on the news this morning about some bird flu outbreak in Auburn. I don't even want to go near it.

EXPERIMENTER 2: Yes, I saw the story too, but they said that it couldn't affect humans.

CONFEDERATE: No, I really don't want to.

EXPERIMENTER 2: Well, if you would rather not, we won't be able to let you participate, but you may still collect the $5 participation fee we promised.

CONFEDERATE: Okay. Sorry, I just don't feel right about it.

[Experimenter 2 escorts the confederate to a third experimenter near the buffet line—away from the other subjects, but within their line of sight. He asks the third experimenter to give him his participation fee. The confederate is paid and instructed to fill out a receipt form and then leaves.]

▪ *TERROR Script*

EXPERIMENTER 1: Excuse me, we need you to take some chicken.

CONFEDERATE: I would rather not. There was a story about a bird flu outbreak on the news this morning.

[Experimenter 1 calls Experimenter 2 from buffet line area.]

EXPERIMENTER 1 to Experimenter 2: He doesn't want to take any chicken.

EXPERIMENTER 2: Sorry sir, but for this study we need you to take some of each of the items. You could take a small amount if you like and we won't require you to eat it.

CONFEDERATE: There was a story on the news this morning about some bird flu outbreak in Auburn. They suspect terrorism. I don't even want to go near it.

EXPERIMENTER 2: Yes, I saw the story too, but they said that it couldn't affect humans.

CONFEDERATE: No, I really don't want to.

EXPERIMENTER 2: Well, if you would rather not, we won't be able to let you participate, but you may still collect the $5 participation fee we promised.

CONFEDERATE: Okay. Sorry, I just don't feel right about it.

[Experimenter 2 escorts the confederate to a third experimenter near the buffet line—away from the other subjects, but within their line of sight. He asks the third experimenter to give him his participation fee. The confederate is paid and instructed to fill out a receipt form and then leaves.]