The New Behavioral Law and Economics

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Contents (*excerpted in part or in full here)

Introduction

Fifty years ago, a group of college seniors considered the risk of tetanus, or "lockjaw" (Leventhal, Singer, and Jones 1965) – a disease that may cause death or serious harm in unvaccinated individuals. Many of the seniors, like many of the rest of us, would tend to understate the risk of actually contracting tetanus upon falling behind on inoculation. How, if at all, should government respond to individuals' frequent assumption that "it won't happen to them" when "it" is something bad (such as contracting tetanus)?

Few would suggest a legal mandate of an up-to-date tetanus vaccine – tetanus not being a communicable disease – but the study of college seniors focused on an approach with similar effects: creating a *default* in favor of vaccination. In a strikingly successful manipulation, the study's authors asked half of the college seniors to review their schedules for the coming fortnight and locate a specific time at which they would pass by the health service and could stop in for an inoculation. This seemingly minor request increased the proportion of seniors in the study who obtained tetanus shots by more than sevenfold!

While these sorts of "default" or status quo effects have received enormous recent attention in behavioral economics, this book passionately champions a competing prominent strand of behavioral economics analysis of law. Rather than changing the "default" people face with an eye to channeling them into a desired form of behavior, law often does, and should,

debias people's misperceptions – bringing them closer to a state of *ken*.[1] What is so compelling about *ken* approaches is that, in these approaches, law acknowledges the human limits identified by behavioral economics but responds *only* through efforts to improve the accuracy of people's perceptions about facts. If the best available evidence suggests that "Will" has an X% chance of developing tetanus in the twelve months following falling behind on his vaccinations, but Will's own estimate is 0.1X% because he thinks "it won't happen to him," a *ken* approach will seek to move his 0.1X% estimate closer to X% – but it will not otherwise seek to influence his decision about vaccination.

Why is this important? There are several possible answers, one of which is at the heart of this book. Even for a relatively uncomplicated matter such as tetanus inoculation, people have complex objectives and constraints. In the study of college seniors, for instance, the students defaulted into inoculation may well have been busy with senior honors theses, completion of their graduation requirements, and researching and interviewing for post-graduation jobs. Tetanus inoculation, while typically a relatively minor procedure, can produce meaningful negative effects.[2] Furthermore, college seniors are not even due for inoculation against tetanus

[1] For purposes of this book, "ken" will refer to knowledge or capacity, as in "[C]limbing ken may be crucial for the fish, which live in strong, high-flow streams" (*National Geographic*, Jan. 22, 2009).

[2] As one recipient reported, "I got a tetanus shot 22 days ago. The muscle in my arm where the shot was given is still so sore the arm is useless. I've used it despite the pain in an effort to strengthen the muscle, but it causes the pain to become severe." http://www.wisegeek.com/why-does-the-tetanus-vaccine-hurt-so-much.htm. If that experience is extreme, "[t]he truth is that the tetanus vaccine is … a particularly painful shot to receive and the residual pain can last for days or even weeks. Some people report feelings of numbness … radiating pain throughout their arms, neck, and back, … general fatigue and muscle weakness." Id.; see also http://www.medicinenet.com/tetanus_toxoid-injection/article.htm (product information) ("Mild fever, joint pain, muscle aches, nausea, tiredness, or pain/itching/swelling/redness at the injection site may occur. Acetaminophen may be used to reduce these effects. If any of these effects persist or worsen, tell the doctor or pharmacist promptly. … Tell the doctor immediately if any

under the standard immunization schedule in the United States. Government's defaulting them into getting a shot without ever directly engaging with the potential factual errors in their risk perceptions shares some of the same strands of "paternalism" that legally mandating a tetanus immunization schedule would exhibit; in both cases, government chooses a particular vaccination schedule *it* believes is best and then induces that schedule in its citizens (either by mandating or by defaulting). In a free society, people should, as much as possible, be left alone to choose the actions they think are best – not those the government thinks are best – when, as in the case of tetanus, primarily their own well-being is at stake.

Ken approaches preserve this fundamental attribute of freedom. Unlike defaults, these approaches do not seek to channel people's decisions; instead they simply seek to improve the factual accuracy of people's risk perceptions. Recall Will, who was optimistic about his risk of contracting tetanus in the next twelve months. If Will comes to understand that his true risk is X%, it could nonetheless be the right decision for Will not to get vaccinated when due for inoculation (say, because Will's father is widowed and undergoing grueling medical treatments as he battles a terminal illness, and Will – who knows from past experience that he is in the group of individuals likely to experience extreme soreness and fever from the tetanus vaccine – is his father's sole source of companionship and emotional strength). As long as Will's estimate of his tetanus risk is consistent with the best available evidence, a government committed to freedom should *not* default him into a particular inoculation schedule (or legally mandate that he follow such a schedule). And the same is true of decisions about matters other than tetanus.

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of these rare but very serious side effects occur: tingling of the hands/feet, hearing problems, trouble swallowing, muscle weakness, seizures.").

Because *ken* approaches do not involve government in channeling citizens toward particular outcomes that government views as what citizens "would" (or should) want in an ideal world, these approaches allow legal academics and policy makers to move beyond the increasingly rancorous debates in recent years over "paternalism" in behavioral economics.[3] *Ken* approaches are simply focused on bringing people's estimates into closer alignment with the estimates suggested by the best available evidence. While many difficult issues of implementation remain (and are discussed in this book), there is also a crucial matter of principle at stake: In a free society, government should generally not seek to channel its citizens' decisions toward government's preferred choices.[4]

American law often reflects an implicit recognition of this book's plea that government look first to *ken* approaches. In the past decade, a striking example comes from the 2006 Pension Protection Act, in which Congress clarified that firms would not violate federal pension law if *the firms* defaulted employees into a particular level of retirement saving in a 401(k) plan. Note what the 2006 Act did *not* do: It did not have *the government* specify a default of a particular level of retirement saving in such plans – even though the evidence is overwhelming that specifying a default of contributing a positive fraction of earnings to a 401(k) plan significantly increases the likelihood that an individual will save,[5] and even though a great many American firms today still do not specify such a positive contribution default. Not only would a government-specified contribution default have increased saving significantly, but just such a government-specified default was adopted in the same year as the Pension Protection Act by New Zealand, and a year later the United Kingdom followed suit (Beshears, Choi, Laibson,

[3] See Chapter 2.
[4] Of course, third-party effects provide a qualification. See Chapter 6 for further discussion.
[5] <Sources.>

Madrian, and Weller 2010). *Yet such a default was never even seriously considered in the American context.*[6] Instead, the American legislators chose an arrangement under which *a private actor*, not the government, selects among potential default contributions – a positive 401(k) contribution versus a zero 401(k) contribution. And the 2006 Pension Protection Act's placing the choice of "default" in the hands of private actors has been followed by extensive research on optimal disclosure formats in the retirement saving context – exactly in the spirit of the *ken* approaches celebrated in the following chapters.[7]

[6] At least, my research on the 2006 Act and subsequent developments did not turn up any mention of consideration of such a default.

[7] <Sources on disclosure formats in retirement saving context.> The pension example, involving private firms, reveals that the term "paternalism" is sometimes used to describe the behavior of a non-governmental actor such as an employer. See Beshears, Choi, Laibson, Madrian, and Weller (2010, p. at __) (describing how "[t]he staunchest opponents" of *employer* adoption of automatic enrollment felt it was "too paternalistic"). In this book, however, "paternalism" is used as the term is typically used in behavioral law and economics – to refer to actions by government.

Chapter 1: What We Don't Know

1.1 "It can't happen to me"

This book is addressed to contexts in which people tend to underestimate the risk of negative events befalling them. Such optimistic bias about negative events can occur in a variety of ways. People may have reasonably accurate information about the average risk of a negative event but assume that their own risk is below average (an assumption that can of course be true for some people – but not for all). Alternatively, people may have little information about the average risk of a given occurrence but nonetheless assume that their own risk is "very low" – much lower than it actually is – or they may develop an excessively low estimate of the average risk. <Remainder of this chapter omitted.>

Chapter 2: The Virtues of *Ken*

2.1 Ways to *Ken*

A central aspect of behavioral law and economics is the attempt to refine and deepen the conventional economic argument for government provision of information when individuals would otherwise operate under misperceptions about factual matters. Economics powerfully defends the availability of truthful, relevant information as an alternative to curtailing individual choice; behavioral economics both qualifies and strengthens the force of this prescription by distinguishing methods of providing truthful, accurate information that typically do little to correct factual misperceptions from methods that are efficacious in achieving this objective.

In seeking to decrease misperceptions among those who believe "it can't happen to me," three general approaches in behavioral economics have enjoyed particular success. <Remainder of this section omitted.>

2.2 Default-Setting, *Ken*, and Paternalism

Recall the college seniors who considered the risk of tetanus and, in some cases, were asked to locate a specific time in the coming weeks when they would pass by the health service and could stop in for an inoculation. Creating a form of default or status quo under which they

would get an inoculation profoundly altered behavior; that much is clear. But was there a change for the better?

Tetanus not being communicable, it is natural to approach this question by looking to the individual seniors' well-being, but that approach immediately places us in an immensely difficult spot. For, as William Congdon, Jeffrey Kling, and Sendhil Mullainathan (2011) emphasize in their recent comprehensive book on behavioral economics and public policy, the fundamental upshot of the behavioral economics literature on default or status quo effects is that "choices no longer reveal preference … because people may reveal multiple preferences" (p. 8). As "[w]elfare analysis is built on the assumption that choice reveals preference, with social welfare reflecting an aggregation of the utility functions thus revealed," the instability of preference "deprives … economics of a clean analytical foundation for assessing the welfare impact of policies" (pp. 8-9).

The instability of preference was all the opening a group of fierce "antipaternalists" needed to launch a broad-scale attack in recent years on the use of behavioral economics in policy making. As economist Glen Whitman (2010) expressed the "paternalism" concern, using more measured language than, unfortunately, has been sometimes been employed in these debates:

> [I]n recent years, a novel form of paternalism has emerged on the policy stage. … '[N]ew paternalism' seeks to make people better off *by their own standards*. New paternalism has gone by many names, including … 'libertarian paternalism,' [and] arose from the burgeoning field of behavioral economics ….
>
> [W]hen confronted with an inconsistency of choice, the new paternalists have often – with no grounding in theory or evidence – chosen among competing preferences. … Thus, when choosing between more and less patient rates of time discounting, they have always favored greater patience. When choosing between 'hot' and 'cold' preferences related to purchases, they have always favored the 'cold' ones. What gives them license to do this? … One the one hand, the new

> paternalists want to say they're making people better off according to their own true preferences. On the other, they say inconsistencies of choice reveal that people don't actually *have* true preferences. They can't have it both ways! In many of their policy proposals, the new paternalists are in fact picking and choosing whichever preferences they think best.

Law professor and economist Jonathan Klick has similarly, if more colorfully, objected to policy proposals that "impose some kind of external judgment" to sort "good" preferences from "bad".[1]

Now, this is "not my usual crowd" – to borrow a wonderful line from Dean Martha Minow (1995). However, while Professors Whitman and Klick and their fellow travelers do not approach many things in the same way that the Congdon, Kling and Mullainathan volume does – or in the same way that this book does – these "antipaternalists" have a point in objecting to attempts to channel individuals toward government-preferred choices.

Channeling proponents have responded that "paternalism" is inevitable, so that attacking defaults for being "paternalistic" is an incoherent, or at least a misguided, critique: "[G]overnments … have to provide starting points of one or another kind; this is not avoidable. … [Governments] do so every day through the rules of contract and tort, in a way that inevitably

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[1] > I wish I had as much money as [behavioral economist Richard] Thaler. Presumably, among the reasons I don't are because I neither save enough nor work hard enough. I take my boys to Phillies games, costing money and time, among other diversions that thwart my stated goal of being rich. Would I be better off if someone nudged me to save more money and to spend more time working?
>
> Thaler and the other libertarian paternalists repeatedly emphasize that they want to make people better off as judged by themselves. If I say I want to be rich, but then fail to do things that are likely to lead to that outcome, does that mean that I need to be nudged or does it mean that my stated goal was a highly conditional statement (shorthand for "I want to be rich as long as it means I don't have to work very hard or give up any consumption in the short term, otherwise I would rather continue doing what I'm doing perhaps while wearing a monocle and a top hat")? Whether a nudge makes someone better off as judged by himself depends crucially on whether we think actions or words better reflect the person's subjective judgment.

affects some preferences and choices. In this respect, the antipaternalist position is unhelpful – a literal nonstarter." (Sunstein and Thaler 2003, p. 1165.) If this were right, it would certainly be a compelling riposte to the "antipaternalists." Is it?

If "paternalism" referred to any influence, of any sort, of government on people's attitudes and behavior, then certainly "paternalism" would be inevitable; indeed, this theme was a major emphasis of the Critical Legal Studies movement in legal thought in the 1980s.[2] However, the fact that government cannot pristinely and austerely avoid ever influencing people's choice behavior in any way (other than through the correction of factual errors) obviously does not establish all forms of influence as undifferentiatedly "paternalistic".[3]

Consider some extreme cases of concern with government influence. It has been widely documented that people's response to written material may vary greatly with a simple change in the *font* in which the material is presented (e.g., Doyle and Bottomly 2004). Because it is implausible to suggest, in response, that any government communication with its citizens, even in the form of (for example) a statement about the level of one's Social Security benefits or Medicare prescriptions, is objectionably "paternalistic" by virtue of its effects on people's attitudes and behaviors, "paternalism" cannot sensibly refer to *any* such effect of government

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[2] See, for example, Kelman (1989, p. __):

> [W]hat seems to be demonstrated . . . is the impossibility of defining desires for end states without recognizing that the end states can scarcely be understood without reference to the collective and legal background.
>
> There is no single-valued 'desire' as such . . ., [no] abstract, presocial, authentic representation of the 'real' desire; each is simply the contextually influenced understanding of both a want and an end state that cannot be abstracted from the legal setting in which one understands precisely what it is that one is seeking.

Legal scholars of varying persuasions have widely accepted the ubiquity of government influence on preferences. See, e.g., Kaplow and Shavell (2002, pp. 413-18).

[3] Indeed, default-setting advocates themselves implicitly acknowledge this truth in assiduously distinguishing defaults from mandates.

behavior. To take another case, a recent study found that subjects faced with a decision whether to purchase a $14.99 DVD were significantly more likely to purchase when the decision not to purchase was worded "not buy" than when the decision not to purchase was worded "keep the $14.99 for other purchases" (Frederick, Novemsky, Wang, Dhar, and Nowlis 2009). Even such a familiar and simple decision was significantly shaped by the details of the verbal formulation. When literally any detail about a government communication may significantly influence people's behavior, it is unhelpful to use the term "paternalism" to refer to any effect of government on such behavior. Sensibly defined, then, "paternalism" is not inevitable, and we are back to the hard question of what sorts of endeavors fall within the proper province of government in a free society.

Just as some forms of government influence on people's attitudes and behaviors are inevitable, some government default-setting is inevitable. At the most foundational level, a default establishing a set of communicative signals (a language) for individuals to use – if nothing else to specify a change to a different set of signals they wish to utilize instead – is unavoidable, and thus any system of law involves government in establishing at least some defaults (see, e.g., Thaler and Sunstein 2008). But defaults such as the one adopted in the college tetanus study are *not* foreordained. In the tetanus study, both the default of no pre-scheduling of an inoculation time and the default of pre-scheduling of an inoculation time could be avoided by having the college seniors choose *whether* they wanted to pre-schedule an inoculation time.[4] To be sure, that choice regime itself is a sort of default, as the seniors could instead be asked to select whether they would be given the choice about pre-scheduling an inoculation time, and so on and so on in an infinite recursion. As with paternalism, some version

[4] See Carroll, Choi, Laibson, Madrian and Metrick (2009) for an extensive analysis of an analogous approach to retirement saving in 401(k) accounts.

of default-setting is inevitable, but this thin observation does not tell us much of interest. In the tetanus study, there is *not* a need to specify either a default of no pre-scheduling or a default of pre-scheduling, and it seems natural to think that a free society would generally prefer a default of student choice over whether to pre-schedule to either of those other defaults (though it is not the task of this book to offer a rigorous defense of this view).[5] <Remainder of this section and the following section omitted.>

[5] Among other things, the answer will vary with context; the material circumstances surrounding some contexts may restrict the set of plausible default rules. Consider the much-discussed example of a cafeteria (Thaler and Sunstein 2008); if government were setting up multiple cafeterias in close proximity to each other, it might be natural to think that a free society would generally prefer that individuals have the choice between a government cafeteria placing salad and fruit first and a government cafeteria placing desserts first – but if government is setting up a single cafeteria in a given vicinity, a single default *is* inevitable for the cafeteria – either salad and fruit first or desserts first. In these sorts of circumstances, government choices that seek to shape people's decisions through the choice of the default may be inevitable. In many circumstances, however, such as in the tetanus study or with respect to retirement saving in 401(k) accounts under the 2006 Pension Protection Act, it seems possible – and conventional – for government to choose a default without in any way seeking to shape people's behavior.

Chapter 5: Application III – Tobacco Regulation[1]

Smoking is a domain in which empirical study of individuals' factual perceptions about risks – and of the effects of tobacco regulation on such perceptions – has been particularly extensive. In part, the large body of available data is a result of an important and controversial article by W. Kip Viscusi (1990). Viscusi's study provides the jumping-off point for the analysis in this chapter.

5.1 Optimism Bias and Smoking

The core finding in Viscusi's 1990 study was that smokers asked to estimate a typical smoker's risk of developing smoking-related lung cancer tended to *overstate* this risk. The study, however, focused exclusively on smoking-related lung cancer. As Viscusi himself was careful to note, lung cancer is responsible for only about a third of all smoking-related deaths (Viscusi 1990). Thus, assessing the accuracy of individuals' perceptions of smoking risks requires data on other major forms of smoking-related illness as well. The empirical evidence discussed in this section supports the conclusion that, indeed, the average consumer significantly underestimates the risk of smoking-related health conditions.

[1] Tables 5.1 and 5.3 and the analysis of these tables are from Jolls (2013).

About a decade ago, the Annenberg Tobacco Risk Study asked respondents in the United States to indicate about what number of smokers out of 100 could be expected to "die from a smoking-related illness."[2] Epidemiological evidence places this figure at about 50 out of 100 smokers. Thus, the survey responses to this risk-of-mortality question may be analyzed by categorizing them as "underestimates" (estimates of fewer than 34 smoking-related deaths out of 100 smokers); responses in the "accurate range"; and "overestimates" (estimates of 68 or more deaths out of 100 smokers). As shown in Romer and Jamieson's (2001) Table 3, which is reproduced in the Appendix, respondents who smoke (defined as those who have smoked one or more cigarettes in the preceding 30 days) tend to underestimate the number of smoking-related deaths out of 100 smokers; the ratio of risk underestimation to risk overestimation among smokers in Table A1 (94:82) is more than half again as large as the ratio of risk underestimation to risk overestimation among nonsmokers (69:93).

To probe the robustness of this suggestive evidence of risk underestimation by smokers, Table 5.1 below presents results from multinomial logit regressions of respondents' underestimation, accurate estimation, or overestimation of smoking risks on respondents' smoking status and the demographic covariates of sex, race, ethnicity, age, and geographic region, covariates that are not reflected in Romer and Jamieson's summary statistics table. Columns (A), (C), (E), and (G) of Table 5.1 present estimated marginal effects for the outcome of risk underestimation, while columns (B), (D), (F), and (H) of the table present estimated marginal effects for the outcome of response in the accurate range. (Estimated coefficients

[2] http://www.icpsr.umich.edu/icpsrweb/HMCA/studies/3049 provides access to the full set of available data from the Annenberg Tobacco Risk Study. Aspects of the data other than those analyzed here are discussed in Romer and Jamieson (2001) and Slovic (2001).

appear in Appendix Table 1.) Estimates are all relative to effects for the omitted outcome of risk overestimation.

The results in Table 5.1 support the conclusion that smokers often have factual misperceptions about the risk of death from a smoking-related illness. The estimated marginal effect of being a smoker on risk underestimation is positive and differs from zero by a statistically significant margin in each of columns (A), (C), (E), and (G); as the results with and without controls for the demographic covariates noted above show, controlling for these covariates has little effect on the results. Columns (B), (D), (F), and (H) of Table 5.1 reinforce the conclusion, as they show that smoking is modestly negatively correlated with responses in the accurate range. Overall, these results suggest that smokers tend to underestimate the risks of smoking.

Table 5.1: Perceived Frequency of Death from a Smoking-Related Illness: Estimated Marginal Effects of Respondents' Smoking Status And Demographic Characteristics

	(A) Under-estimation	(B) Accurate range	(C) Under-estimation	(D) Accurate range	(E) Under-estimation	(F) Accurate range	(G) Under-estimation	(H) Accurate range
Smoker	0.119***	-0.090**	0.103***	-0.082**	0.128***	-0.118**	0.096**	-0.092**
	(0.039)	(0.043)	(0.038)	(0.041)	(0.041)	(0.047)	(0.041)	(0.044)
Male					0.008	0.031	0.009	0.007
					(0.040)	(0.047)	(0.039)	(0.042)
White					-0.066	0.142	-0.053	0.120
					(0.088)	(0.094)	(0.083)	(0.086)
Black					0.016	0.073	-0.031	0.076
					(0.098)	(0.120)	(0.088)	(0.109)
Asian					0.095	-0.060	-0.005	0.076
					(0.177)	(0.187)	(0.148)	(0.173)
Hispanic					0.039	-0.070	0.026	-0.042
					(0.068)	(0.075)	(0.065)	(0.067)
Age 17-19					0.041	-0.045	0.036	-0.049
					(0.052)	(0.058)	(0.049)	(0.051)
Age 20-22					-0.024	0.060	0.018	0.008
					(0.051)	(0.063)	(0.053)	(0.056)
West					-0.085	0.022	-0.106*	0.032
					(0.060)	(0.081)	(0.057)	(0.072)
Mid-west					0.043	0.052	0.010	0.049
					(0.065)	(0.076)	(0.060)	(0.068)
South					-0.017	0.107	0.002	0.072
					(0.056)	(0.070)	(0.056)	(0.064)
N	557	557	557	557	557	557	557	557
Survey weights	yes	yes	no	no	yes	yes	no	no

Notes: Each of column pairs (A)-(B), (C)-(D), (E)-(F), and (G)-(H) presents estimated marginal effects from multinomial logit regressions of perceived frequency of death from a smoking-related illness on respondents' smoking status and demographic characteristics. "Underestimation" = 0 to 33 deaths; "accurate range" = 34 to 67 deaths; "overestimation" (68 to 100 deaths) is the omitted dependent variable. The dummy variable for smoking is equal to 1 if the respondent smoked cigarettes in the preceding 30 days and 0 otherwise. Effects of race are relative to individuals who identified as "other" rather than white, black, or Asian. Effects of Hispanic ethnicity are relative to individuals identified as non-Hispanic. Effects of age are relative to individuals age 14-16. Effects of geographic region are relative to individuals from the northeast. One or more of the demographic covariates were missing for ten of the observations reflected in Table A1; accordingly, those observations are not reflected in the regressions reported in this table. Standard errors are in parentheses.

* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.

5.2 *Ken* Through Mandated Tobacco Warnings

How might smokers attain a more accurate understanding of the factual risks of smoking? Governments in the United States and around the world have adopted requirements that tobacco advertisements and packaging carry mandated health warnings. The mandated health warnings adopted in 2009 in the United States – warnings that are in the process of being revised in response to adverse judicial decisions – involve two major components. First, the warnings contain a specified text statement (for example, "WARNING: Smoking can kill you"), and second, they incorporate an accompanying graphic image. Table 5.2 below presents a series of the most recent text-image warnings developed for use in the United States.

Tables 5.3 through 5.5 present empirical results on the effects of these warnings in a large-scale experimental study in the United States (*Experimental Study of Graphic Cigarette Warning Labels*). The national opt-in e-mail list sample used in the study consists of smokers who viewed either a text-image warning of the sort shown in Table 5.2 or a paired warning that contained the same text statement as the text-image warning but no accompanying graphic image.[3]

[3] http://www.regulations.gov/#!documentDetail;D=FDA-2010-N-0568-0008 provides access to the full set of available study data. The specific data used in the empirical analysis in this section appears in Appendix C-2 (pp. 1-29, 141-169, and 212-239).

Table 5.2: United States Text-Image Tobacco Warnings

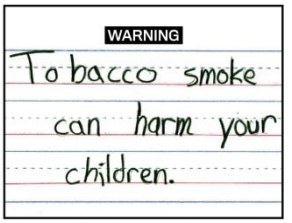
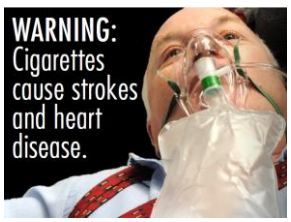
1	 
2	   
3	
4	 
5	 
6	 
7	   

Table 5.2: United States Text-Image Tobacco Warnings
(continued from previous page)

8	   
9	

Source: http://www.regulations.gov/#!documentDetail;D=FDA-2010-N-0568-0002 (under "Proposed Required Warning Images").

Tables 5.3, 5.4, and 5.5 present estimates of the effects of text-image versus text-only warnings on respondents' answers to the eight questions from the experimental study that involved subjects' factual perceptions of smoking risks (as distinguished from questions about smoking's emotional valence for respondents and other matters distinct from their factual perceptions of smoking risks).[4] The estimated effects are in the form of logistic regression odds ratios with values less than one if respondents exposed to an text-image warning were *less* likely to have answered a given question affirmatively than respondents exposed to a text-only warning and values greater than one if they were *more* likely to have answered affirmatively. Tables 5.3, 5.4, and 5.5 reflect results for all warning pairs that, for at least one of the eight risk perception questions in the experimental study, had an odds ratio that differed from one by more than a factor of two – a reasonably demanding test of magnitude – *and* that differed from one by a statistically significant margin. Accordingly, warning pairs with odds ratios either less than one-half or more than two – and different from one by a statistically significant margin at the five-percent level – for one or more of the eight risk perception questions noted above are reflected in Tables 5.3, 5.4, and 5.5.

Table 5.3 presents results from respondents over the age of 24, while Table 5.4 presents results from respondents in their early twenties, and Table 5.5 presents results for "youth" (teen) respondents. The twelve warning pairs contained in Table 5.3 represent one-third of the total

[4] The full text of the survey is provided in Appendix A of *Experimental Study of Graphic Cigarette Warning Labels*. Order effects of the survey questions are not a concern for the analysis here because the analysis examines results for respondents who viewed a text-image warning *relative to* results for those who viewed a paired text-only warning, and there is no reason to think that the ordering of questions would produce *differential* effects across these two groups.

group of 36 warning pairs examined in the data analyzed for respondents over the age of 24;[5] by contrast, far smaller numbers of warning pairs met the criteria for inclusion in the younger samples reflected in Tables 5.4 and 5.5.

Columns (A) and (B) of Tables 5.3, 5.4, and 5.5 present estimates of the effects of text-image versus text-only warnings on respondents' level of agreement with two assertions that individuals with factually accurate risk perceptions would consider to be false. The first assertion is that someone who has smoked a pack of cigarettes a day for more than twenty years will achieve "little health benefit" from quitting (corresponding results appear in column (A) in Tables 5.3, 5.4, and 5.5); and the second assertion is that because "many people who smoke live to a ripe old age," smoking is "not all that bad" for one's health (corresponding results appear in column (B) in Tables 5.3, 5.4, and 5.5).[6] Respondents who chose either "strongly agree" or "agree," rather than "neither agree nor disagree," "disagree," or "strongly disagree," in response to these assertions can reasonably be viewed as exhibiting factual misperceptions at least to some degree;[7] accordingly, an association between a given text-image warning and a lower level of agreement with the assertions provides some evidence that factual misperceptions are lower with the text-image warning.

As shown by the results in columns (A) and (B) of Table 5.3, the text-image warnings in rows II (image of woman in rain), IV (image of woman in oxygen mask), V (image of smoke approaching baby), VI (image of cancerous lesion on lip), and X (image of gravestone) produced

[5] The study grouped each of the nine text statements with between two and six graphic images, a procedure that produced 36 text-image warnings in all for study within each of the three samples of respondents.
[6] Question text is from *Experimental Study of Graphic Cigarette Warning Labels* (Appendix A, pp. A-11 to A-12).
[7] Question coding information is from *Experimental Study of Graphic Cigarette Warning Labels* (Appendix B, pp. 4-18 to 4-19).

substantially lower levels of agreement with the assertions than text-only counterparts. In these five rows, at least one of the two estimated odds ratios in columns (A) and (B) satisfies both the magnitude test noted above – here, that the estimated odds ratio is below one-half, indicating a substantially lower level of agreement with the assertions among respondents who viewed a text-image warning – and a test of statistical significance. In row IV, estimated odds ratios are below one-half both in column (A) *and* in column (B). While text-image warnings were associated with lower levels of agreement with one or both of the assertions in the five rows just noted, in rows IX, XI, and XII these warnings were associated with *higher* levels of agreement with one of the assertions; with respect to row IX, however, estimated odds ratios for a number of the questions other than question (A) support the conclusion that factual misperceptions are *lower* with a text-image warning (columns (D), (E), (F), and (H) of row IX, with estimated odds ratios of 1.749, 2.105, 1.985, and 2.138, estimates that will be discussed more fully below). Overall, the estimates discussed thus far provide support for the conclusion that factual misperceptions tend to be (though they are not inevitably) lower with a text-image warning than with a text-only alternative.

<Discussion of results in columns (A) and (B) of Tables 5.4 and 5.5 to be added.>

Columns (C) and (D) of Tables 5.3, 5.4, and 5.5 present estimates of the effects of text-image versus text-only warnings on respondents' answers to the question "Do you think your smoking has affected your health?" and a follow-on question asking "how concerned" respondents were that smoking had affected their health.[8] Respondents with accurate perceptions of smoking risks probably could not answer "no" when asked whether smoking had

[8] Question text is from *Experimental Study of Graphic Cigarette Warning Labels* (Appendix A, pp. A-11 to A-12); coding information is also provided (Appendix B, pp. 4-18 to 4-19).

affected their health; thus, an increase in the frequency of "yes" answers to the "health effects of smoking" with a text-image warning – as indicated by an estimated odds ratio greater than one in column (C) of Table 5.3 – provides support for a decline in factual misperceptions.

Analysis of responses to the inquiry about "how concerned" respondents were that smoking had affected their health is complicated by the fact that two different interpretations of the question are possible. Respondents might have thought that they were being asked about the level of likelihood they attached to experiencing smoking-related health effects (just as someone "very concerned" about the weather might think the chance of rain was very high). Alternatively, respondents might have believed that they were being asked about the degree of negative emotion or feeling ("concern") they had about smoking-related health effects. On the first interpretation of the question – which matches the conception of risk utilized in this chapter (compare Slovic 2001) – respondents are being asked about the likelihood of a negative effect, and thus answers to the question should be probative, at least to some degree, of perceptions of smoking risks; on the second interpretation, however, respondents' answers might be unrelated to the perceived likelihood of harm. On the first interpretation, a greater level of expressed concern about health effects among respondents will tend to support the conclusion that factual misperceptions have declined because, as discussed below, a diverse set of analyses and data suggest that underestimation of the overall risks of smoking is common. Of course, there is no guarantee that any increase in perceived risk, no matter how large, would reduce individuals' factual misperceptions; it is conceivable that factual misperceptions could be larger if respondents' perceived level of risk swung from too low to an even larger error in the opposite direction.

As shown by the results in column (C) of Table 5.3, a text-image warning produced a substantial enough increase, relative to the paired text-only warning, in the frequency of "yes" answers to the question about whether respondents believed smoking had affected their health to yield estimated odds ratios greater than two, and different from one by a statistically significant margin, both in row III and in row VII. Thus, the results in these rows suggest a lower level of factual misperceptions under the text-image warning. In addition, in row IX of Table 5.3, the estimated odds ratio of 1.749 in column (C) exceeds one by a statistically significant margin at the ten-percent level – providing some slight additional support for the conclusion that factual misperceptions are lower under a text-image warning than under a text-only alternative. Only in row I of Table 5.3 does the evidence provide support for increased rather than reduced factual misperceptions under a text-image warning (estimated odds ratio of 0.387 in column (C)). Finally, in rows VIII and X of Table 5.3, a text-image warning was, relative to its text-only counterpart, associated with greater "concern" about smoking-related health effects (column (D)),

<Discussion of results in columns (C) and (D) of Tables 5.4 and 5.5 to be added.>

Table 5.3: Logistic Regression Odds Ratios – Text-Image vs. Text-Only Warnings, "Adult" Sample

	Warning statement	# of obs.	Warning image (for text-image warning)	(A) "Little benefit from quitting"	(B) "Smoking not bad"	(C) Affect own health	(D) Concern about affecting own health	(E) Likelihood of cancer	(F) Likelihood of fatal lung disease	(G) Likelihood of heart disease	(H) Likelihood of stroke
I	1 ("cigarettes are addictive")	517	Hole in throat	0.943 (0.900)	0.540 (0.156)	0.387*** (0.005)	0.766 (0.362)	0.999 (0.998)	0.861 (0.602)	0.932 (0.806)	1.179 (0.580)
II	1 ("cigarettes are addictive")	517	Woman in rain	1.443 (0.404)	0.325** (0.026)	0.789 (0.507)	0.886 (0.681)	0.723 (0.265)	0.886 (0.674)	0.775 (0.376)	0.856 (0.614)
III	2 ("harm to children")	727	Smoke at blond toddler	0.744 (0.493)	0.449 (0.060)	2.469** (0.015)	1.459 (0.203)	1.782** (0.046)	1.478 (0.175)	1.802** (0.041)	1.813** (0.045)
IV	2 ("harm to children")	727	Girl/ oxygen mask	0.431* (0.085)	0.243*** (0.005)	1.090 (0.793)	1.202 (0.530)	1.182 (0.564)	0.966 (0.904)	1.358 (0.288)	1.092 (0.772)
V	2 ("harm to children")	727	Smoke approaching baby	0.744 (0.492)	0.304** (0.012)	1.226 (0.538)	1.149 (0.636)	1.504 (0.158)	1.274 (0.399)	1.133 (0.665)	1.307 (0.370)
VI	4 ("cause cancer")	518	Cancerous lesion on lip	0.391** (0.041)	0.756 (0.493)	0.685 (0.274)	1.074 (0.804)	2.137** (0.010)	1.917** (0.026)	2.397*** (0.003)	2.287*** (0.005)
VII	5 ("cause strokes…")	517	Man/ oxygen mask	0.838 (0.647)	1.034 (0.941)	2.384** (0.014)	0.709 (0.238)	0.629 (0.118)	0.474** (0.012)	0.839 (0.545)	0.569* (0.060)
VIII	7 ("kill you")	516	Man in casket	1.497 (0.385)	0.687 (0.352)	1.325 (0.410)	3.033*** (0.000)	1.879** (0.034)	2.263*** (0.007)	1.479 (0.184)	2.075** (0.021)
IX	7 ("kill you")	516	Woman in coffin	2.440** (0.041)	0.767 (0.502)	0.769 (0.417)	1.749 (0.068)	2.105** (0.013)	1.985** (0.023)	1.397 (0.258)	2.138** (0.017)
X	7 ("kill you")	516	Gravestone	1.891 (0.161)	0.360** (0.032)	1.537 (0.232)	2.785*** (0.001)	2.240*** (0.007)	2.452*** (0.003)	1.537 (0.145)	1.975** (0.031)
XI	8 ("disease in nonsmokers")	619	Smoke at man	3.712** (0.031)	0.974 (0.951)	0.873 (0.685)	0.816 (0.489)	0.881 (0.671)	0.955 (0.879)	0.960 (0.891)	1.217 (0.524)
XII	8 ("disease in nonsmokers")	619	Woman crying	5.105*** (0.006)	1.158 (0.722)	1.452 (0.285)	0.939 (0.831)	0.960 (0.892)	1.116 (0.717)	1.157 (0.627)	0.966 (0.911)

Source: Estimated odds ratios are from *Experimental Study of Graphic Cigarette Warning Labels* (Appendix C-2, pp. 1–29).

Notes: Odds ratios are based on nine multinomial logit regressions (corresponding to the nine textual statements in the warnings in Table 5.2) of survey responses on dummy variables for each text-image warning associated with a given warning statement and controls for age, gender, race/ethnicity, education, income, and whether the respondent had plans to stop smoking within 30 days. p-values are reported in parentheses. (Correspondence with the lead author of the study confirmed that the figures in parentheses, which appear to be mistakenly described in Appendix C-2 of the study, are p-values.) Starred estimates are statistically significant at the 10%, 5%, or 1% level: * p<0.10; ** p<0.05; *** p<0.01.

Table 5.4: Logistic Regression Odds Ratios – Text-Image vs. Text-Only Warnings, "Young Adult" Sample

	Warning statement	# of obs.	Warning image (for text-image warning)	(A) "Little benefit from quitting"	(B) "Smoking not bad"	(C) Affect own health	(D) Concern about affecting own health	(E) Likelihood of cancer	(F) Likelihood of fatal lung disease	(G) Likelihood of heart disease	(H) Likelihood of stroke
I	2 ("harm to children")	< >	Girl/oxygen mask	1.074 (0.844)	0.686 (0.347)	0.846 (0.576)	0.576* (0.068)	1.142 (0.650)	2.078** (0.012)	1.070 (0.813)	0.984 (0.956)
II	2 ("harm to children")	< >	Smoke approaching baby	0.739 (0.444)	0.283** (0.012)	0.953 (0.880)	0.630 (0.128)	1.236 (0.477)	1.287 (0.381)	1.184 (0.560)	1.158 (0.613)
III	8 ("disease in nonsmokers")	< >	Graveyard	1.098 (0.804)	0.536 (0.170)	0.985 (0.960)	2.011** (0.021)	1.080 (0.787)	1.774** (0.048)	2.065** (0.013)	1.869* (0.041)

Source: Estimated odds ratios are from *Experimental Study of Graphic Cigarette Warning Labels* (Appendix C-2, pp. 141–169).
Notes: Odds ratios are based on nine multinomial logit regressions (corresponding to the nine textual statements in the warnings in Table 5.2) of survey responses on dummy variables for each text-image warning associated with a given warning statement and controls for age, gender, race/ethnicity, education, income, and whether the respondent had plans to stop smoking within 30 days. p-values are reported in parentheses. (Correspondence with the lead author of the study confirmed that the figures in parentheses, which appear to be mistakenly described in Appendix C-2 of the study, are p-values.) Starred estimates are statistically significant at the 10%, 5%, or 1% level: * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table 5.5: Logistic Regression Odds Ratios – Text-Image vs. Text-Only Warnings, "Youth" Sample

	Warning statement	# of obs.	Warning image (for text-image warning)	(A) "Little benefit from quitting"	(B) "Smoking not bad"	(C) Affect own health	(D) Concern about affecting own health	(E) Likelihood of cancer	(F) Likelihood of fatal lung disease	(G) Likelihood of heart disease	(H) Likelihood of stroke
I	2 ("harm to children")	< >	Note in child's hand-writing	2.096** (0.046)	0.816 (0.683)	1.991 (0.225)	1.503 (0.572)	0.830 (0.537)	0.950 (0.869)	0.954 (0.869)	1.029 (0.918)
II	3 ("fatal lung disease")	< >	Healthy/ diseased lungs	0.629 (0.171)	0.894 (0.818)	3.791** (0.045)	0.443 (0.263)	0.695 (0.236)	0.504** (0.028)	0.778 (0.377)	0.867 (0.623)
III	6 ("harm your baby")	< >	Baby in incubator	0.432** (0.938)	1.040 (0.938)	0.233** (0.037)	0.852 (0.805)	0.790 (0.458)	0.701 (0.243)	0.456*** (0.007)	0.566* (0.058)
IV	8 ("disease in nonsmokers")	< >	Graveyard	0.792 (0.507)	0.691 (0.438)	0.233** (0.029)	0.457 (0.251)	1.103 (0.751)	1.011 (0.971)	1.098 (0.745)	0.782 (0.391)
V	8 ("disease in nonsmokers")	< >	Man with hands up	0.473* (0.053)	0.485 (0.170)	0.187** (0.010)	0.340 (0.117)	1.236 (0.490)	1.059 (0.848)	1.003 (0.991)	0.708 (0.226)

Source: Estimated odds ratios are from *Experimental Study of Graphic Cigarette Warning Labels* (Appendix C-2, pp. 212–239).
Notes: Odds ratios are based on nine multinomial logit regressions (corresponding to the nine textual statements in the warnings in Table 5.2) of survey responses on dummy variables for each text-image warning associated with a given warning statement and controls for age, gender, race/ethnicity, education, income, and whether the respondent had plans to stop smoking within 30 days. p-values are reported in parentheses. (Correspondence with the lead author of the study confirmed that the figures in parentheses, which appear to be mistakenly described in Appendix C-2 of the study, are p-values.) Starred estimates are statistically significant at the 10%, 5%, or 1% level: * p<0.10; ** p<0.05; *** p<0.01.

The remaining four risk perception questions in the experimental study are health-condition-specific questions. These questions ask how likely respondents believe a smoker is to experience cancer, "fatal lung disease," heart disease, and stroke.[9] A higher frequency of "extremely likely" and "very likely" answers, relative to "moderately likely," "somewhat likely," and "not at all likely" answers, to these questions with text-image warning may or may not suggest reduced factual misperceptions with such a warning; it depends on what range of probability respondents attach to "very likely," "moderately likely," and the other answer categories to the health-condition-specific questions, as well as the actual probability of the various individual health conditions given the best available evidence.[10]

What is most important in the analysis here, however, is not whether individuals' estimates of the risk of a particular smoking-related health condition, such as cancer, have become more accurate but whether their *overall* estimates of smoking risks have become more accurate; even in a case, for instance, in which text-image type warning produced an overestimate of the risk of smoking-related cancer in particular – such that factual misperceptions with respect to that health condition had actually increased – it is possible, perhaps likely, that overall estimates of smoking risks would, by virtue of the text-image warning, be both higher and closer to factually accurate. (Many smoking-related health conditions appear to be entirely off respondents' radar screens.) Nonetheless, the value of responses to the health-condition-specific questions in Table 2 is significantly limited by the considerations noted here.

[9] Question text is from *Experimental Study of Graphic Cigarette Warning Labels* (Appendix A, pp. A-11 to A-12).
[10] Question coding information was obtained through correspondence with the experimental study's lead author.

In each of rows VI, VIII, IX, and X of Table 5.3, half or more of the health-condition questions elicited significantly higher expressed likelihoods – odds ratios above two and different from one by a statistically significant margin – from respondents who viewed a text-image warning than from those who viewed a text-only warning. Moreover, in row III, three of the four health condition questions produced estimated odds ratios that, although not over two, are close to two (1.782, 1.802, and 1.813) and different from one by a statistically significant margin. Only row VII of Table 5.3 exhibits the opposite pattern of lower expressed likelihoods with a text-image-type warning than with its text-only counterpart (columns (F) and (H)). In row IX, which the discussion above noted had one estimated odds ratio (column (A)) that supported an increase in factual misperceptions with a text-image warning in comparison to a text-only warning, the results for the four health condition questions point in the opposite direction (estimated odds ratios of 2.105, 1.985, 1.397, and 2.138).

<Discussion of results in columns (E) through (H) of Tables 5.4 and 5.5 to be added.>

The results reported in this section generally support the conclusion that text-image warnings tend to reduce individuals' factual misperceptions relative to these warnings' text-only counterparts. To be sure, it is important not to infer that in every case a text-image warning would reduce such misperceptions; the available data is not so definitive. However, the fact that among the 36 warning pairs in the data analyzed in Table 5.3, for instance, nine text-image warnings produced lower factual misperceptions employing the fairly restrictive criteria used in constructing Table 5.3 – while in only three cases was there evidence (often quite limited) of higher factual misperceptions – supports the conclusion that text-image warnings on balance tend to reduce factual misperceptions. Importantly, in the three cases in which there was evidence of higher factual misperceptions under text-only warning in Table 5.3 (rows I, XI, and XII), only a

single estimated odds ratio in each case supported the conclusion of increased factual misperceptions. This pattern provides some suggestion of the absence of a systematic increase in factual misperceptions associated with any text-image warning. By contrast, seven of the nine warning pairs that produced evidence of lower factual misperceptions under an text-image warning had two or more estimated odds ratios that differed from one by a statistically significant margin in the direction of reduced factual misperceptions with a text-image warning (rows III, IV, VI, VII, VIII, IX, and X), and six of these seven warning pairs had three or more estimated odds ratios that differed from one by a statistically significant margin in that direction.

Chapter 6: Beyond Paternalism

Recently, over a thousand employees at a large American company were subjected to the same "default of inoculation" as the college seniors described at the start of this book – though this time the inoculation was against influenza rather than tetanus (Milkman, Beshears, Choi, Laibson and Madrian 2011). These employees received mailers that provided them with information about the availability of free on-site vaccination – information that was provided to all company employees for whom influenza vaccination was indicated by Centers for Disease Control and Prevention guidelines – and also prompted them to write down a particular date and time on which they planned to get vaccinated. (Specifically, the mailer included the following additional line of text: "Many people find it helpful to make a plan for getting their shot. You can write yours here." The additional text was followed by blanks for the date and time.) The predictable effect of this manipulation was to increase the likelihood of vaccination relative to employees who received the same information about the availability of free on-site vaccination but were not prompted to make a plan for getting vaccinated (though, interestingly, by less of a margin than in the tetanus study).

The influenza study, however, differs profoundly from the tetanus study in that the effort to increase inoculation rates could well be motivated *not* a belief that inoculation would make individuals who obtained it better off but, instead, by a belief that increased inoculation rates

would yield significant *social* benefits. Because influenza is contagious, every inoculation of a given individual produces a benefit to anyone who comes into contact with that individual.

It is possible to quibble over whether social benefits fundamentally drive influenza inoculation policy, which most heavily targets individuals 50 years or age or older and those with chronic health conditions, suggesting at least some focus on protecting vulnerable individuals rather than society as a whole. (Of course, if the heavily targeted individuals are not only more likely to suffer severe consequences from influenza but also more likely to catch influenza, then influenza inoculation policy fits nicely with a focus on social benefits.) The crucial point, however, is more general. *Policies that default individuals into particular outcomes may well be justified on the basis of significant third-party effects of such individuals' behavior*. Policies addressed to third-party interests are, of course, familiar and ubiquitous in today's world; recent behavioral economics work on the power of default effects generates the novel and important suggestion that third-party effects may be effectively addressed through defaults rather than mandatory rules. But nothing in this argument provides any normative basis for using defaults to channel individuals' behavior "for their own good."

William Congdon, Jeffrey Kling, and Sendhil Mullainathan (2011, pp. 8-9) express this fundamental point well:

> [In practice, most government] policies aim to [address] ... cases [in which] there are first-order consequences of individuals' behavior to *society*, independent of the consequences for their own welfare. For example, if decisionmaking biases lead individuals to systematically disfavor fuel-efficient vehicles, one can debate what their true utility function is. But the carbon externality that they impose in making their choice is clear.

Congdon, Kling and Mullainathan's book therefore "focus[es] on how behavioral economics changes policies in areas in which government already plays a traditional role" (p. 9; cf.

Korobkin 2009). The approach taken above is complementary in addressing how both existing law and potential reform may seek to eliminate or reduce individuals' misperceptions about risks in important decision making domains. Together, it is hoped that these approaches can move behavioral economics analysis of policy and law beyond the paternalism debates of recent years and into a bright future ahead.

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Appendix Table 1: Perceived Frequency of Death from a Smoking-Related Illness – Estimated Coefficients from Multinominal Logit Regressions

	(A) Under-estimation	(B) Accurate range	(C) Under-estimation	(D) Accurate range	(E) Under-estimation	(F) Accurate range	(G) Under-estimation	() Accurate range
Smoker	0.516**	-0.126	0.430	-0.135	0.489	-0.261	0.350	-0.214
	(0.227)	(0.210)	(0.221)	(0.325)	(0.242)	(0.234)	(0.234)	(0.216)
Male					0.156	0.199	0.082	0.067
					(0.246)	(0.227)	(0.223)	(0.206)
White					-0.015	0.596	0.031	0.533
					(0.456)	(0.452)	(0.430)	(0.429)
Black					0.380	0.479	0.042	0.330
					(0.535)	(0.528)	(0.506)	(0.497)
Asian					0.438	-0.028	0.246	0.435
					(0.870)	(0.904)	(0.838)	(0.803)
Hispanic					0.049	-0.268	0.034	-0.161
					(0.380)	0.364)	(0.346)	(0.325)
Age 17-19					0.145	-0.119	0.081	-0.167
					(0.305)	(0.275)	(0.276)	(0.250)
Age 20-22					0.024	0.256	0.150	0.105
					(0.328)	(0.302)	(0.305)	(0.275)
West					-0.554	-0.139	-0.641	-0.147
					(0.407)	(0.364)	(0.366)	(0.329)
Midwest					0.492	0.448	0.238	0.320
					(0.378)	(0.361)	(0.341)	(0.326)
South					0.238	0.546	0.257	0.422
					(0.354)	(0.341)	(0.324)	(0.309)
Constant	-0.349**	0.344**	-0.291*	0.325	-0.631	-0.549	-0.427	-0.275
	(0.165)	(0.142)	(0.160)	(0.138)	(0.593)	(0.571)	(0.553)	(0.535)
N	557	557	557	557	557	557	557	557
Survey Weights	yes	yes	no	no	yes	yes	no	no

Notes: Each of column pairs (A)-(B), (C)-(D), (E)-(F), and (G)-(H) presents estimated coefficients from multinomial logit regressions of perceived frequency of death from a smoking-related illness on respondents' smoking status and demographic characteristics. "Underestimation" = 0 to 33 deaths; "accurate range" = 34 to 67 deaths; "overestimation" (68 to 100 deaths) is the omitted dependent variable. The dummy variable for smoking is equal to 1 if the respondent smoked cigarettes in the preceding 30 days and 0 otherwise. Effects of race are relative to individuals who identified as "other" rather than white, black, or Asian. Effects of Hispanic ethnicity are relative to individuals identified as non-Hispanic. Effects of age are relative to individuals age 14-16. Effects of geographic region are relative to individuals from the northeast. One or more of the demographic covariates were missing for ten of the observations reflected in Table A1; accordingly, those observations are not reflected in the regressions reported in this table. Standard errors are in parentheses.

* Significant at 10% level.
** Significant at 5% level.
*** Significant at 1% level.