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Assessing the Empirical Upside of Personalized Criminal Procedure

Matthew B. Kugler[†] and Lior Jacob Strahilevitz^{††}

Though personalization of law is often viewed as a new idea, pockets of criminal procedure already tolerate it. Many courts have held that Miranda warnings must be tailored when read to juveniles or people with limited English proficiency; a suspect's age is necessarily part of the judicial calculus when determining whether the police's questioning of her is a custodial interrogation; and some state courts consider a person's demographic characteristics when deciding whether they have consented to a search. The question before us now is whether society should go further. Should the law of criminal procedure pay more attention to individual differences in privacy expectations, personality, and cognitive abilities? In this Essay, we adopt an empirical approach, assessing the extent to which the state could meaningfully personalize criminal procedure. Saved for a later day is the normative question of whether it should.

We conducted a survey on a nationally representative sample of adult Americans to determine the extent to which factors relevant to criminal procedure law can be predicted by demographic and personality differences. The data revealed that a number of factors predict people's relevant expectations, behaviors, and knowledge. Women consistently perceive less freedom to refuse consent to a law enforcement search, whereas those who have been arrested or have a close friend who has been arrested perceive more freedom to say no. African Americans are more likely to suspect that an officer would draw a weapon or plant evidence in a vehicle during an encounter with the police. Younger and more educated Americans have stronger expectations of privacy against surveillance than older and less educated people. Older and more educated Americans, as well as those who have greater exposure to the criminal justice system, have a better understanding of their Miranda rights than their younger, less educated, and less experienced counterparts. Various ideological and personality factors also correlate with divergent responses. That said, in no instances are demographic and personality considerations hugely predictive. Models incorporating a wide range of predictors typically explained less than 10 percent of the observed variation in individual behavior, expectations, and attitudes. Although

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we have not tested all approaches to criminal procedure personalization, our empirical investigation of traditional techniques suggests data-driven efforts to personalize criminal procedure may not be worth the trouble.

This data-driven approach does shed light on another issue in criminal procedure, however. In a long line of cases, courts have had to decide whether deviations from the standard script for Miranda warnings warranted the exclusion of confessions. We tested several versions of the Miranda warning, including one deemed inadequate in a recent case. We found no differences in comprehension either overall or among respondents at increased risk of misunderstanding their rights (younger respondents and the less well educated). We believe that this experimental approach provides a valuable method of evaluating the appropriateness of nonstandard Miranda warnings.

INTRODUCTION

Imagine a person is being questioned by the police. If this is a mere friendly chat, then the police need not advise that person of her rights. If, however, this is a “custodial interrogation,” then the person—the suspect—must generally be given a *Miranda* warning for any incriminating statements she makes to be admissible in court. Certain factors seem obviously relevant to the determination of whether the interrogation was custodial: Did the conversation occur in a locked room at the station? Was the officer asking the questions armed? And so on. But should the courts and police also consider the characteristics of the suspect? The Supreme Court in *J.D.B. v North Carolina*¹ held that the age of the person being questioned is necessarily relevant to a determination of whether the interrogation was custodial too, at least in the case of juveniles.² It left unresolved the question of whether other personal characteristics also must be considered.

The Court in *J.D.B.* took a step toward what we call the personalization of criminal procedure. By personalization we mean tailoring the law’s content on the basis of a person’s individual characteristics. The sort of personalization we have in mind is, for the most part, not based on an individual’s own previous behavior—though such personalization has arisen in the doctrine occasionally thanks to either a police officer’s memory of previous interactions or technological developments that facilitate

¹ 564 US 261 (2011).

² *Id.* at 277.

information sharing among officers.³ Instead, we consider personalization on the basis of group characteristics, looking to the attitudes, behaviors, and expectations of similar others.

In recent years, the personalization of law has become a hot topic, one that has now sparked its first law review symposium. But to date, and even in this Symposium, personalization proposals have largely focused on private law.⁴ Here, we examine whether the kind of personalization described in *J.D.B.* is warranted in different domains of criminal procedure. The question of whether public law in general, and constitutionalized bodies of doctrine like criminal procedure in particular, ought to be personalized on the basis of immutable traits has scarcely been addressed. Such personalization raises obvious hard normative and constitutional questions.⁵ Before we consider those questions, however, we should consider whether personalization would lead to meaningfully better accuracy. If demographics do not substantially predict attitudes, expectations, and beliefs in criminal procedure, then the benefits of tailoring this area of law are unlikely to be large enough to make the game worth the candle. For this

³ For an illustration of this sort of personalization, compare *United States v Taylor*, 511 F3d 87, 92 (1st Cir 2007) (holding that the suspect's evident nervousness during an interaction with a police officer created reasonable suspicion because in previous interactions with the same officer the suspect had been calm), with *United States v McKoy*, 428 F3d 38, 41 (1st Cir 2005) (holding that nervousness alone during an interaction with a police officer is not enough to generate reasonable suspicion).

⁴ See generally, for example, Anthony J. Casey and Anthony Niblett, *The Death of Rules and Standards*, 92 Ind L J 1401 (2017); Omri Ben-Shahar and Ariel Porat, *Personalizing Negligence Law*, 91 NYU L Rev 627 (2016); Ariel Porat and Lior Jacob Strahilevitz, *Personalizing Default Rules and Disclosure with Big Data*, 112 Mich L Rev 1417 (2014); Cass R. Sunstein, *Deciding by Default*, 162 U Pa L Rev 1 (2013).

Other recent scholarship examines algorithmic decision-making's role in determining whether reasonable or individualized suspicion exists. Such decision-making is a form of personalized policing policy. See generally, for example, Andrew Guthrie Ferguson, *Big Data and Predictive Reasonable Suspicion*, 163 U Pa L Rev 327 (2015); Elizabeth E. Joh, *Policing by Numbers: Big Data and the Fourth Amendment*, 89 Wash L Rev 35 (2014); Michael L. Rich, *Machine Learning, Automated Suspicion Algorithms, and the Fourth Amendment*, 164 U Pa L Rev 871 (2016). Our interest here is the related but distinct topic of personalized legal doctrine.

⁵ We recognize that such personalization on the basis of race may well be subject to strict scrutiny under existing law. See *Johnson v California*, 543 US 499, 509 (2005) (applying strict scrutiny to a prison policy of initially placing new inmates with cellmates of the same race). As we hope will become apparent, we are interested in examining whether data-driven personalization on the basis of race might be used to promote equal treatment in a criminal justice system that systematically disadvantages discrete and insular minorities. Note that some courts have been open to the possibility of using immutable characteristics to inform algorithmic decision-making. See *State v Loomis*, 881 NW2d 749, 765–67 (Wis 2016) (finding no due process violation in bail algorithm that considered gender to be a relevant factor in assessing recidivism risk).

reason, we largely sidestep the normative questions in this Essay and focus on the empirical.

After gathering data on several potential candidates for personalization, we have not found evidence to indicate that the benefits from personalizing criminal procedure law are particularly large. To be sure, that could change if the American public becomes substantially more polarized than it already is. But a perhaps surprising takeaway from our research is that, for many questions involving criminal procedure attitudes and expectations, people are people. Though Americans do disagree on these issues, that disagreement is not substantially predicted by their social categories.

I. BACKGROUND

The empirical study of Fourth Amendment attitudes has entered its adolescence. Several teams of researchers, each with their own particular methodology and primary topics of interest, compete to map out the privacy expectations of ordinary Americans. Sometimes, as in the recently decided cell-site geolocation case of *Carpenter v United States*,⁶ the research sends a single coherent message: more people expect privacy in such and such domain than do not. But even when an overwhelming majority of people expect privacy in a given context, there is usually a meaningful minority who do not. And this minority is somewhat identifiable; the existing literature has revealed that sometimes personality and demographic factors are correlated with privacy attitudes.⁷

It is not just in the privacy expectations context that we might encounter meaningful demographic variation. Some research studies indicate that African American citizens are more likely to be unnerved by the proximate presence of police officers

⁶ 138 S Ct 2206 (2018). We helped write an amicus brief in this case citing data from a number of researchers in this field. See Brief of Amici Curiae Empirical Fourth Amendment Scholars in Support of Petitioner, *Carpenter v United States*, No 16-402 (US filed Aug 14, 2017) (available on Westlaw at 2017 WL 3530963). The papers containing these data included Bernard Chao, et al, *Why Courts Fail to Protect Privacy: Race, Age, Bias, and Technology*, 106 Cal L Rev 263, 297–98 (2018); Alisa Smith, Sean Madden, and Robert P. Barton, *An Empirical Examination of Societal Expectations of Privacy in the Digital Age of GPS, Cell Phone Towers, and Drones*, 26 Albany L J Sci & Tech 111, 133 (2016); Matthew Tokson, *Knowledge and Fourth Amendment Privacy*, 111 Nw U L Rev 139, 177 (2016); Matthew B. Kugler and Lior Jacob Strahilevitz, *Actual Expectations of Privacy, Fourth Amendment Doctrine, and the Mosaic Theory*, 2015 S Ct Rev 205, 259; and Christine S. Scott-Hayward, Henry F. Fradella, and Ryan G. Fischer, *Does Privacy Require Secrecy? Societal Expectations of Privacy in the Digital Age*, 43 Am J Crim L 19, 52–53 (2015).

⁷ See Kugler and Strahilevitz, 2015 S Ct Rev at 262–63 (cited in note 6).

than their Caucasian counterparts, and tragic, well-publicized instances of police misconduct that victimized African American citizens underscore a potential source of those demographic differences.⁸ It also has long been suggested that African Americans understandably go to great lengths in traffic stops to project calm and avoid provoking confrontations with police.⁹ Should we ignore this backdrop of racial disparities when assessing whether a given interaction with the police was free of improper coercion?

A host of other Fourth Amendment questions could also implicate demographic differences: Which interactions with law enforcement are suspicious, and which are normal? Do different kinds of people feel equally free to refuse when police ask permission to search their property or stop them for questioning? Do Americans from different backgrounds understand their *Miranda* rights in the same way after hearing them read?

Perhaps surprisingly, there is little scholarship that considers this kind of group- and demographic-based personalization of criminal procedure law in any depth.¹⁰ There is one well-done article from the early 1990s that discussed racial disparities in consent and reasonable suspicion cases. It noted that many African American males regard their interactions with law enforcement quite warily and may regard themselves as having been seized, rather than free to leave, when stopped by the police.¹¹ The author insisted that he was not advocating and would not advocate for a “separate” Fourth Amendment for black men. Considering the possibility briefly, he wrote: “Nothing could be further from the

⁸ See David A. Sklansky, *Traffic Stops, Minority Motorists, and the Future of the Fourth Amendment*, 1997 S Ct Rev 271, 312–16 (describing the police harassment experienced by African American motorists).

⁹ See, for example, Devon W. Carbado, *(E)racing the Fourth Amendment*, 100 Mich L Rev 946, 953–54 (2002).

¹⁰ In a way, all criminal procedure is personalized. For instance, the reasonable suspicion standard calls for “particularized suspicion” based on a suspect’s actions and demeanor, among other factors. *Illinois v Wardlow*, 528 US 119, 124 (2000). Here we are discussing personalization based on less immediate, and more statistically driven, factors.

¹¹ Tracey Maclin, “*Black and Blue Encounters*”—*Some Preliminary Thoughts about Fourth Amendment Seizures: Should Race Matter?*, 26 Valp U L Rev 243, 250–56 (1991). See also Sklansky, 1997 S Ct Rev at 327–29 (cited in note 8) (“What the recent vehicle stop cases suggest that Fourth Amendment law needs is not a special rule to protect minority groups, but more attention to the special concerns of minority groups in the formulation and application of all Fourth Amendment rules.”); Jeffrey Fagan, et al, *Stops and Stares: Street Stops, Surveillance, and Race in the New Policing*, 43 Fordham Urban L J 539, 560 (2016) (“Recent empirical evidence on police stops supports perceptions among minority citizens that police disproportionately stop African American and Hispanic motorists, and that once stopped, these citizens are more likely to be searched or arrested.”).

truth.”¹² But he did want courts to consider target race along with other factors when assessing the voluntariness of a search.¹³ Along somewhat similar lines, a note from 2001 argued that the law should incorporate a “reasonable Black person” standard into Fourth Amendment doctrine governing *Terry* stops.¹⁴ The note is also thoughtful, but it limits its analysis to *Terry* stops. Another project that gathered empirical data on “free to leave” expectations proposed a “reasonable person of similar age” and “reasonable person of the same gender” standard in response to observed differences on those dimensions.¹⁵ As best we can tell, no research paper has ever drilled down empirically to examine the extent of racial *and other demographic* differences across a host of germane criminal procedure contexts.

II. CURRENT LAW AND TRENDS

Though there are obvious problems under the Fourteenth Amendment with treating citizens differently based on their demographic characteristics, current doctrine actually requires it in some domains. As we note in the Introduction, in *J.D.B.*, the Supreme Court held that courts must consider the age of a juvenile when determining whether an interrogation is custodial and therefore triggers the *Miranda* rights of the person being questioned.¹⁶ Writing for the Court, Justice Sonia Sotomayor said that youth would always be associated with increased susceptibility to outside pressure and that a child’s age would be relatively easy for police to discern.¹⁷ It would, then, not be fair to hold children to the same standard as adults. She distinguished tailoring based on age versus tailoring driven by other factors, noting that requiring the police to anticipate “the idiosyncrasies of every individual suspect and divining how those particular traits affect each person’s subjective state of mind” would be unwise.¹⁸

Reasonable as that sounds, such tailoring raises substantial line-drawing problems. Justice Samuel Alito’s dissent in *J.D.B.*

¹² Maclin, 26 Valp U L Rev at 272 (cited in note 11).

¹³ Id.

¹⁴ Mia Carpiniello, Note, *Striking a Sincere Balance: A Reasonable Black Person Standard for “Location plus Evasion” Terry Stops*, 6 Mich J Race & L 355, 357–58 (2001).

¹⁵ See David K. Kessler, *Free to Leave? An Empirical Look at the Fourth Amendment’s Seizure Standard*, 99 J Crim Law & Crimin 51, 85 (2009). For further discussion see note 38 and accompanying text.

¹⁶ *J.D.B.*, 564 US at 277.

¹⁷ Id at 275, 279.

¹⁸ Id at 271.

expressed alarm at the majority's rejection of what had previously been an "objective reasonable-person standard,"¹⁹ and he harbored "little doubt that today's decision will soon be cited by defendants—and perhaps by prosecutors as well—for the proposition that all manner of other individual characteristics should be treated like age and taken into account in the *Miranda* custody calculus."²⁰ Justice Alito named a suspect's intelligence, cultural background, and education as potentially relevant factors after *J.D.B.*²¹

Custodial interrogations are hardly the only body of doctrine in which courts have embraced personalization. Individuals walking through "high crime neighborhoods" have, as a legal matter, fewer protections against police stops and seizures than those walking through ritzier areas.²² So too do individuals who happen to be near the US border with Mexico.²³ And it should not be remotely surprising that African Americans and Latinos, respectively, are more likely to be searched as a result of these quasi-personalized Fourth Amendment doctrines.²⁴ Though it may not be readily apparent, this is a form of personalization precisely because of the nonrandom draw of people who are likely to be present in these spaces. Proximity to the southern border or to high crime neighborhoods is a proxy for race, however imperfect.²⁵ If Fourth Amendment and Equal Protection doctrine can stomach those kinds of racially disparate effects, surely there is some room for doctrinal moves that are likely to *enhance* the privacy rights of racial minorities rather than diminishing them.

This question of personalization has already arisen in consent searches. Here the federal courts are also generally resistant to personalization, employing a reasonable person standard rather than examining subjectively what the defendant in a criminal case actually knew or expected when asked for permission to

¹⁹ *Id.* at 287 (Alito dissenting).

²⁰ *J.D.B.*, 564 US at 292 (Alito dissenting).

²¹ *Id.* at 291–92 (Alito dissenting).

²² See David A. Harris, *Factors for Reasonable Suspicion: When Black and Poor Means Stopped and Frisked*, 69 Ind L J 659, 671–72, 681 (1994).

²³ See, for example, *United States v Martinez-Fuerte*, 428 US 543, 562–63 (1976) (allowing stops of automotive travelers at fixed checkpoints near the border without individualized suspicion).

²⁴ See Harris, 69 Ind L J at 680–81 (cited in note 22).

²⁵ See Lindsey Barrett, *Reasonably Suspicious Algorithms: Predictive Policing at the United States Border*, 41 NYU Rev L & Soc Change 327, 356–58 (2017). See also Harris, 69 Ind L J at 681 (cited in note 22).

search.²⁶ But some state supreme courts interpret their state constitutions to focus on the particular defendant's subjective understanding, opening up space for personalization.²⁷ And personalization was, and to some extent still is, common in assessing the voluntariness of confessions. Courts historically examined confession voluntariness using a totality of the circumstances test²⁸ and have considered individualizing factors like suspect age, health, and intelligence.²⁹ Indeed, prior to *Miranda*, the voluntariness test used to determine the admissibility of confessions was very context sensitive, with the courts applying a standard that could be tailored to the circumstances of any particular interrogation—one that critics felt rendered the law insufficiently predictable.³⁰

Consider some tantalizing personalization possibilities: If men feel more freedom to refuse an officer's request to search their belongings than do women, perhaps in cases when it is ambiguous whether consent was given, the tie-breaking rule should be "yes" for men but "no" for women. If highly educated people know their *Miranda* rights but poorly educated people do not, then maybe a failure to *Mirandize* someone before an interrogation should lead to exclusion of evidence obtained from a poorly educated person in custody but not of the same evidence obtained from someone with more education. We could even imagine some versions of *Miranda* working better for certain kinds of people, a criminal procedure application of previous proposals for personalized disclosure.³¹ Maybe reasonable expectations of privacy should be determined based on the expectations held by people demographically similar to the defendant in a particular surveillance case. And, to return to the custodial interrogation context and Justice Alito's slippery slope,³² maybe age, education, cultural background, and intelligence *can be* incorporated into judicial determinations of whether a suspect has been taken into custody.

²⁶ See, for example, *United States v Drayton*, 536 US 194, 202 (2002).

²⁷ See, for example, *State v Blair*, 396 P3d 908, 914 (Or 2017).

²⁸ See, for example, *Spano v New York*, 360 US 315, 323 (1959).

²⁹ See Paul Marcus, *It's Not Just about Miranda: Determining the Voluntariness of Confessions in Criminal Prosecutions*, 40 Valp U L Rev 601, 627–34 (2006). But see Richard A. Leo, *Questioning the Relevance of Miranda in the Twenty-First Century*, 99 Mich L Rev 1000, 1021 (2001) (arguing that the focus has shifted largely to the voluntariness of *Miranda* waivers).

³⁰ See, for example, Eve Brensike Primus, *The Future of Confession Law: Toward Rules for the Voluntariness Test*, 114 Mich L Rev 1, 10–12 (2015).

³¹ See Porat and Strahilevitz, 112 Mich L Rev at 1470–76 (cited in note 4).

³² See text accompanying note 20.

III. THE STUDY

The greater our ability to predict the heterogeneity in the overall population, the greater the potential upside from personalizing law. We therefore sought to determine how easy it is to predict people's attitudes, beliefs, and expectations on criminal procedure-related issues from demographic and personality factors.

We sampled a census-representative mix of twelve hundred adult Americans and included a wide array of demographic and personality measures.³³ These included traditional factors, such as race, sex, age, educational attainment, self-assessed social class, and region; these are the kinds of information that can be readily deduced from public records. We also administered two psychological inventories: a shortened version of the Big Five questionnaire as well as a measure of authoritarianism.³⁴ This kind of information is not directly available to law enforcement, but one can predict core personality traits somewhat accurately using big data.³⁵ Our survey instrument used written instructions and questions rather than oral ones, both for practical reasons and because the literature suggests that, in at least one of the

³³ Full demographics are reported in Appendix A. The sample was recruited by Toluna, a well-regarded survey research firm that we have used in our previous research. The survey was administered online using Qualtrics. For gender identity, participants could select male or female or fill in their own answer. Two participants filled in an answer. Because this is such a small number that we could not add a category for them, we left them in the analysis and coded them as Female = 0. The original sample contained 1,253 participants, but data from 53 were discarded due to abnormally fast completion times (less than half the median).

³⁴ The Big Five scale used was developed by Samuel D. Gosling, Peter J. Rentfrow, and William B. Swann Jr, *A Very Brief Measure of the Big-Five Personality Domains*, 37 *J Rsrch Personality* 504, 525 (2003). The Big Five categorizes people among five essential measures of personality—extraversion, neuroticism, conscientiousness, openness to new experiences, and agreeableness. The authoritarianism scale is authoritarian submission, developed by John Duckitt, et al, *A Tripartite Approach to Right-Wing Authoritarianism: The Authoritarianism-Conservatism-Traditionalism Model*, 31 *Polit Psychology* 685, 711–12 (2010).

³⁵ See, for example, Gokul Chittaranjan, Jan Blom, and Daniel Gatica-Perez, *Mining Large-Scale Smartphone Data for Personality Studies*, 17 *Personal & Ubiquitous Computing* 433, 447–48 (2013); Baiyun Chen and Justin Marcus, *Students' Self-Presentation on Facebook: An Examination of Personality and Self-Construction Factors*, 28 *Computers in Hum Behav* 2091, 2097 (2012); Tracii Ryan and Sophia Xenos, *Who Uses Facebook? An Investigation into the Relationship between the Big Five, Shyness, Narcissism, Loneliness, and Facebook Usage*, 27 *Computers in Hum Behav* 1658, 1662–63 (2011). Notably, no study has perfectly predicted these dimensions from other data. But granting the model this information poses a conservative test for our generally skeptical take on personalization.

contexts we are studying, written information is more likely to be retained by research subjects.³⁶

Our data collection focused on three broad classes of criminal procedure issues.³⁷ The first involves perceived freedom to choose. When a law enforcement officer approaches and makes a request to conduct an interview or perform a search, does the target believe she can say no? Prior research in this area has found that people often do not feel free to decline, but this work has been conducted on convenience samples and has not been well-suited to detecting demographic differences.³⁸ Based on a considerable literature in psychology, one might expect to find that people of the middle and upper classes would view themselves as having more choice.³⁹ Personality, as measured through the “Big Five” framework that has dominated social psychological research for decades, could also be predictive.⁴⁰ One might also predict that—consistent with prior findings from nonrepresentative samples—gender will matter, as women tend to score higher on the psychological personality dimension of “agreeableness”⁴¹ and may feel more physically vulnerable to requests from members of a predominantly male police force.

³⁶ See Richard Rogers, et al, *Investigating the Effects of Repeated Miranda Warnings: Do They Perform a Curative Function on Common Miranda Misconceptions?*, 31 *Behav Sci & L* 397, 403 (2013).

³⁷ The study began with the demographic measures and then moved to the consent search and expectation of privacy measures, which were administered in random order. After those measures were completed, participants received blocks of personality questions and the *Miranda* items. They then completed one final consent search scenario and a few sensitive demographic questions that asked whether they or a close associate had ever been arrested.

³⁸ See, for example, Kessler, 99 *J Crim L & Crimin* at 68–69 (cited in note 15) (using sample consisting of approximately four hundred residents of Boston stopped on the street). David Kessler’s work did show some demographic effects, including one consistent with the pattern we observe on gender. See *id* at 77. But Kessler is quick to point out that the sample was nonrepresentative in many respects. See *id* at 72–73.

³⁹ See, for example, Michael W. Kraus, Jacinth J.X. Tan, and Melanie B. Tannenbaum, *The Social Ladder: A Rank-Based Perspective on Social Class*, 24 *Psychological Inquiry* 81, 84 (2013) (“For upper-class individuals, chronic elevated rank perceptions may increase expectations of personal control, choice, and autonomy within interactions, even in the absence of actual control or autonomy.”).

⁴⁰ For discussion of the use of the Big Five to measure revealed preferences and behavioral tendencies, see Murray R. Barrick and Michael K. Mount, *The Big Five Personality Dimensions and Job Performance: A Meta-analysis*, 44 *Personnel Psychology* 1, 17–19 (1991).

⁴¹ See, for example, Yanna J. Weisberg, Colin G. DeYoung, and Jacob B. Hirsh, *Gender Differences in Personality across the Ten Aspects of the Big Five*, 2 *Frontiers in Psychology* 1, 8 (2011).

We ran two regression models for each major dependent variable in the consent searches and expectations of privacy domains. The first model contained only basic demographic information, most of which would be accessible to the government in one form or another. The second model added a variety of personality factors, testing whether somewhat more sophisticated tailoring would produce substantially greater explanatory power. For the *Miranda* vignette, we ran further models to test whether our differing versions of the *Miranda* warning produced varying levels of comprehension.

A. Consent Searches and Feelings regarding Police Interactions

The study included three consent-search vignettes. One of these involved a car search. After completing the ticket-writing business of a traffic stop, a police officer says, “Do you mind if I search the trunk of your car?” The others involved being stopped and asked to answer questions in two different locations: on the street and on an intercity bus that was stopped at a station. In each case, participants were presented with a vignette based on a hypothetical police interaction and asked how free they would feel to decline the officer’s request on a five-point scale that ranged from 1 (“Not at all free” to refuse to answer questions or to refuse permission) to 5 (“Completely free” to refuse to answer questions or to refuse permission).

For simplicity of data analysis, these three items were averaged together to create a composite ($\alpha = 0.75$), and we conducted a series of regression analyses to predict this composite score from a variety of demographic factors. Though several factors are related to perceived freedom to decline, most of these relationships are quite weak. Being African American or Hispanic increases perceived freedom to decline by about 0.2 points on the five-point scale. Having been arrested oneself or having a close friend or family member who had been arrested also increases perceived freedom by a similar magnitude.⁴² These effects (unlike education

⁴² In our sample, 21 percent of respondents reported having been arrested and 49 percent reported having had a close associate who had been arrested. Some other recent research estimates that arrest is more common than that, with a recent study suggesting that about 30 percent of US adults have been arrested by the age of thirty-four. See J.C. Barnes, et al, *Arrest Prevalence in a National Sample of Adults: The Role of Sex and Race/Ethnicity*, 40 Am J Crim Just 457, 460 (2015). The prevalence of youth arrests has

levels, interestingly enough) all move participants in the direction of greater doctrinal accuracy—people can refuse in each of these cases—so this may be a case of those with greater reason to know their rights being more aware of the legal rule. The strongest effect, however, is on gender, as Table 1 reveals. Women perceive less freedom to decline by between 0.3 and 0.4 points, much larger than the other effects. Social class, which the psychological literature had led us to believe would be relevant, did not have a significant effect.

TABLE 1: PERCEIVED FREEDOM TO REFUSE A SEARCH

	Model 1 ($R^2 = 0.038$)				Model 2 ($R^2 = 0.060$)			
	Unstd Coeff		Std Coeff		Unstd Coeff		Std Coeff	
(Constant)	2.698	(0.249)			3.387	(0.451)		
Female	-0.381	(0.073)	-0.156	***	-0.317	(0.076)	-0.130	***
Hispanic	0.223	(0.100)	0.070	*	0.207	(0.100)	0.065	*
African American	0.259	(0.111)	0.072	*	0.200	(0.112)	0.055	
Education	-0.019	(0.037)	-0.017		-0.036	(0.037)	-0.032	
Social Class	0.075	(0.042)	0.058		0.043	(0.042)	0.033	
Age	0.005	(0.002)	0.066	*	0.004	(0.002)	0.052	
Midwest	0.013	(0.109)	0.004		0.007	(0.109)	0.003	
South	0.014	(0.097)	0.006		0.021	(0.097)	0.008	
West	0.062	(0.111)	0.020		0.050	(0.111)	0.016	
Self/Friend Arrested	0.198	(0.071)	0.081	**	0.198	(0.071)	0.081	**
Agreeableness					-0.094	(0.038)	-0.085	*
Extroversion					0.039	(0.027)	0.042	
Conscientious					0.077	(0.037)	0.071	*
Neuroticism					-0.105	(0.032)	-0.114	***
Openness					-0.010	(0.033)	-0.010	
Authoritarianism					-0.066	(0.044)	-0.048	
Conservatism					0.025	(0.022)	0.036	

Note: Northeast was used as the regional reference group. *** indicates $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Conservatism was measured on a scale ranging from 1 (Very Liberal) to 7 (Very Conservative). The Big Five scales ranged from 1 to 7; authoritarianism ranged from 1 to 6.

Notably, even this broad array of demographic factors explains only 3.8 percent of the variance in perceived freedom to

increased substantially over the past several decades. See Robert Brame, et al, *Cumulative Prevalence of Arrest from Ages 8 to 23 in a National Sample*, 129 *Pediatrics* 21, 23 (2012).

decline. A model that incorporates psychological factors—the Big Five, authoritarianism, and conservatism—as well as demographics has greater predictive power. Model 2 shows significant effects on Big Five neuroticism and agreeableness—both the more neurotic and the more agreeable see less freedom to decline—and on conscientiousness, which is associated with increased perceived freedom. But even adding the full array of personality factors increases the variance explained to only 6.0 percent. Our ability to explain individual differences is therefore modest.

To make this more concrete, let us look at the gender effect on the individual scenarios. There we see between 45 percent and 63 percent of men saying they would generally have felt free to leave or refuse to answer questions, but only between 35 percent and 50 percent of women. Table 2 displays these results.

TABLE 2: PERCEIVED FREEDOM TO REFUSE A SEARCH BASED ON GENDER

					Position Relative to Midpoint			
					Male		Female	
	Male	Female	Under	Over	Under	Over		
Street Stop	3.26	(1.46)	2.93	(1.44)	31%	45%	39%	35%
Stop on Bus	3.28	(1.47)	2.94	(1.50)	31%	47%	40%	36%
Trunk Search	3.78	(1.45)	3.39	(1.53)	19%	63%	30%	50%

Note: Perceived freedom was measured on a 1 to 5 scale with a midpoint of 3. Higher numbers indicate greater perceived freedom.

Whether this difference between genders has normative significance depends on where one sets one's threshold. For both women and men, a substantial minority say they *would not* feel free to decline in the street and bus stop questions. Slightly more men say that they would feel free versus not, and for women the numbers are narrowly in the other direction. If one ignores gender differences, then the overall result is a 5 percentage-point difference in favor of perceiving freedom. That isn't wildly out of line with the results for either individual gender. On the car stop, gender has less practical impact. More women and more men feel that they can decline the officer's request to search the car's trunk than feel that they must acquiesce.

These results highlight a recurring problem in privacy surveys. We do not yet have an overarching theory about thresholds, whether we mean global thresholds (Do enough people want privacy here that we should protect it?) or personalization thresholds (Are people different enough that we should distinguish between them?). Here, we think the percentage of variance explained is a useful measure on the personalization threshold. People differ in their perceptions of freedom to choose, but even considering all of these factors explains only a small piece of the puzzle. The issue of global thresholds is very likely context dependent, and we will hold our thoughts on that question for another day.

In addition to the question of perceived freedom to refuse, we were also interested in the overall dynamic of the police interaction. How nervous do different people think they would feel upon being stopped by the police? Here we were limited in part by the hypothetical nature of the questions. Asking how nervous people think they would feel when being stopped by the police is different than measuring their emotional reactions as they occur during a real interaction. Nevertheless, the data are interesting, and we report these analyses in Appendix B. First, the race effects observed on the other measures we discuss below are absent on expected nervousness. This is particularly surprising given the literature on traffic stops of African Americans.⁴³ Women and more educated respondents, however, report that they think they would be more nervous during the interaction. But these bare demographics explain only 2.2 percent of the variance. When personality variables are included in the analysis, we observe an effect of neuroticism—associated with increased expected nervousness—as well as smaller effects for agreeableness (increased nervousness) and extroversion (decreased). People who score higher on authoritarianism also expect that they will be less nervous. This model explains 10.9 percent of the observed variation, or 8.7 percent more than the mere demographics. So psychological measures of personality help explain who expects to feel more anxious when encountering the police, but only to a limited degree.⁴⁴

Given the increased attention paid to the possibility of police misconduct in the wake of the Black Lives Matter movement, we

⁴³ See notes 11–15 and accompanying text.

⁴⁴ Notably, one of the neuroticism items asked participants whether they would describe themselves as “anxious, easily upset.” This is as direct a measure as you would ever be likely to have.

also asked respondents about two other topics: whether it was likely the police officer would draw a gun during the interaction, and whether—if given the chance—it was likely the officer would plant evidence. These questions were answered on five-point scales ranging from 1 (Extremely Unlikely) to 5 (Extremely Likely). Because the mere presentation of these possibilities had the potential to bias other responses, these questions were asked only for one search, the car stop, and that vignette was administered at the very end of our survey. We present these results in Appendices C and D. For both types of misconduct, African Americans and younger respondents thought it much more likely that police would behave in these ways, and those scoring high on authoritarian personality dimensions thought such police behavior was much less likely. The African American effect here is rather large. The overall mean for the weapon question was 2.13. Being African American increased the estimate to 2.87. Similarly, on the estimate for the planting evidence question, the jump was from 2.28 to 2.98. These are the only questions for which bare demographics explain a meaningful amount of the variance (10.7 percent for the weapon being drawn, 7.4 percent for planting evidence). The previously observed effects of gender and neuroticism were entirely absent. Even more interesting, there were no effects related to the displayed race of the officer on any of these questions.⁴⁵

There is a puzzle when we aggregate this data. While African Americans viewed police drawing a weapon or planting evidence as markedly more likely, they did not describe themselves as being more nervous during the hypothetical police questioning or police request scenarios that appeared earlier in the survey. Wouldn't the fear of a deadly encounter or being framed make one more nervous in interactions with the police? It would seem so, but we can envision three reasons that may explain why that result did not show up in the data. First, it may be that members of

⁴⁵ For the car stop, the vignette was accompanied by a picture of a white or African American male officer leaning over a car's driver-side door, with random variation of the police officer's race. An analysis of variance (ANOVA) was conducted using officer race and the survey participant's identification as either African American or not as predictors. Neither officer race nor the interaction between officer race and participant race approached significance for any of these measures (all F s < 1). For the street stop, a picture of a white officer in front of a fence was displayed to all participants. No picture was shown for the bus stop.

different racial groups are more or less reluctant to admit nervousness even if they feel it.⁴⁶ In that sense, there could be a limit to what one can do with surveys rather than physiological measures like saliva cortisol levels or expensive technologies like fMRI.⁴⁷ Second, it could be that statements about police mistrust or misconduct provided a particularly powerful prime to African Americans in the environment of late 2017. That is, the mention of police officers drawing weapons unnecessarily or planting evidence to incriminate an innocent suspect had a substantial effect on African Americans (relative to members of other groups), causing them to assign comparatively high ratings on those measures. Perhaps African Americans would have reported substantially more nervousness if we'd asked them to rate their anxiety *after* we asked about police misconduct. Finally, it may be that a higher percentage of African Americans have become desensitized to encounters with police, in part because they have been stopped and pulled over more regularly than members of other racial groups.⁴⁸ African Americans may have adopted coping mechanisms out of necessity or developed adaptive preferences, resulting in a kind of fatalism that calms nerves even amid terrifying scenarios springing to mind during encounters with law enforcement.

B. Reasonable Expectations of Privacy

The subject of reasonable expectations of privacy has been a focus of our research for the past several years.⁴⁹ Borrowing from the Supreme Court's Fourth Amendment decision in *Katz v United States*,⁵⁰ our general approach is to describe a potential

⁴⁶ See Jennifer K. Bosson, Ethan L. Haymovitz, and Elizabeth C. Pinel, *When Saying and Doing Diverge: The Effects of Stereotype Threat on Self-Reported versus Non-verbal Anxiety*, 40 *J Exper Soc Psychology* 247, 253 (2004) (reporting on an experiment in which gay men who were reminded of their stigmatized status before interacting with children did not express greater anxiety about those interactions when surveyed but did display greater nonverbal indicators of anxiety); Robert J. Edelman and Sarah R. Baker, *Self-Reported and Actual Physiological Responses in Social Phobia*, 41 *British J Clinical Psychology* 1, 10 (2002) (noting experimental subjects' discrepancies between self-assessed anxiety and physical manifestations of anxiety).

⁴⁷ We wonder how feasible it would be to simulate an arrest of an experimental subject in an fMRI machine. An officer entering a room containing a giant magnet might be wise to leave her weapon behind!

⁴⁸ See, for example, Patricia Warren, et al, *Driving while Black: Bias Processes and Racial Disparity in Police Stops*, 44 *Criminology* 709, 728 (2006).

⁴⁹ See generally Matthew B. Kugler and Lior Jacob Strahilevitz, *The Myth of Fourth Amendment Circularity*, 84 *U Chi L Rev* 1747 (2017); Kugler and Strahilevitz, 2015 *S Ct Rev* 205 (cited in note 6).

⁵⁰ 389 US 347 (1967).

search and ask whether it would violate privacy expectations for law enforcement to conduct it.⁵¹ For instance, in this study we asked, “Would it violate people’s reasonable expectations of privacy if law enforcement: Installed a video camera to watch a public park where criminal activity has recently occurred?” Participants respond to these questions on a 1 (Definitely Not) to 5 (Definitely Yes) scale. For this survey we included the questions used in one of our prior papers⁵² plus several new ones. Our past work in this area has not explored demographic differences in privacy expectations in any sustained way, though in one prior paper we noted a relationship between expectations and age: those who were older had lower expectations of privacy in the GPS tracking context.⁵³ Demographic factors that correlate with divergent expectations of privacy are the subject of a recent article by Professor Bernard Chao and coauthors.⁵⁴ They find that African Americans were more likely to regard police conduct as violating reasonable expectations of privacy, as were Caucasians who had previously been searched or investigated by law enforcement.⁵⁵

Table 3 displays the means for each of our reasonable expectation of privacy questions as well as the percentage of subjects placing themselves below and above the midpoint for each. Again, the ratings on the individual items were averaged to create a single composite ($\alpha = 0.85$), and the same multiple regression was conducted on these data. This analysis is presented in Table 4.

⁵¹ A majority of the Supreme Court recently reaffirmed the vitality of the *Katz* test in determining whether police surveillance amounts to a search. See *Carpenter*, 138 S Ct at 2227.

⁵² Kugler and Strahilevitz, 2015 S Ct Rev at 260 (cited in note 6).

⁵³ *Id* at 252.

⁵⁴ Chao, et al, 106 Cal L Rev at 310–15 (cited in note 6).

⁵⁵ *Id* at 310–11.

TABLE 3: REASONABLE EXPECTATION OF PRIVACY RATINGS

	M		Below	Above	Ratio: (Above/Below)
Remote activate webcam	4.17	(1.39)	15%	76%	4.98
Obtain emails from ISP	3.71	(1.42)	20%	61%	3.02
Facial recognition at Super Bowl	2.58	(1.52)	53%	29%	0.55
Camera in public park	2.22	(1.49)	65%	22%	0.34
Cell-site location data	3.29	(1.45)	30%	46%	1.53
Stingray cellphone tracking	3.53	(1.44)	24%	53%	2.19
Uber trip history	3.36	(1.46)	28%	49%	1.76
Camera through home window	3.94	(1.41)	18%	69%	3.79
Drone backyard pictures	3.65	(1.44)	22%	59%	2.67

Note: Numbers in parentheses are standard deviations. The questions appeared in random order and are reproduced verbatim in Appendix E.

Here, age and educational attainment are the strongest predictors of privacy expectations. The younger and the more educated had higher privacy expectations than the older and less educated. As Table 4 shows, sex, race and ethnicity, and arrest experience are entirely unrelated to privacy expectations. When the personality factors are added in Model 2, there is a moderately strong effect of authoritarianism—more authoritarian participants had lower privacy expectations. This effect reduced the strength of the age and education effects, making education non-significant.

TABLE 4: PREDICTORS OF PRIVACY EXPECTATIONS

	Model 1 ($R^2 = 0.025$)				Model 2 ($R^2 = 0.058$)			
	Unstd Coeff		Std Coeff		Unstd Coeff		Std Coeff	
(Constant)	3.369	(0.137)			3.664	(0.331)		
Female	0.055	(0.059)	0.028		0.067	(0.061)	0.034	
Hispanic	0.074	(0.082)	0.029		0.021	(0.081)	0.008	
African American	0.115	(0.090)	0.039		0.063	(0.090)	0.021	
Education	0.073	(0.027)	0.080	**	0.038	(0.027)	0.042	
Age	-0.006	(0.002)	-0.096	**	-0.004	(0.002)	-0.077	*
Midwest	0.009	(0.089)	0.004		0.010	(0.088)	0.004	
South	-0.062	(0.079)	-0.031		-0.027	(0.079)	-0.013	
West	-0.093	(0.091)	-0.038		-0.085	(0.090)	-0.035	
Self/Friend Arrested	0.092	(0.058)	0.047		0.064	(0.057)	0.033	
Agreeableness					0.003	(0.031)	0.003	
Extroversion					0.024	(0.022)	0.031	
Conscientious					0.032	(0.030)	0.037	
Neuroticism					0.014	(0.026)	0.019	
Openness					0.037	(0.027)	0.044	
Authoritarianism					-0.173	(0.036)	-0.155	***
Conservatism					-0.023	(0.018)	-0.041	

Note: Northeast was used as the regional reference group. *** indicates $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Conservatism was measured on a scale ranging from 1 (Very Liberal) to 7 (Very Conservative). The Big Five scales ranged from 1 to 7; authoritarianism was 1 to 6.

Despite these significant differences, the full model including all the personality variables explains only 5.8 percent of the variance in privacy expectations. The bare demographics in Model 1 explain only 2.5 percent. These are interesting results from a social scientific standpoint, but they have little policy effect. A white, non-Hispanic college graduate who is 25 should be expected under Model 1 to average about 3.51 on these privacy expectation questions. A white, non-Hispanic high school graduate who is 65 should be expected to average 3.14. Effectively, these changes in age and education move the estimate up or down approximately 0.19 on our five-point scale. Consider the consequences of adding or subtracting 0.19 from each of the searches. The only search that gets meaningfully closer to the midpoint is

historical cell-site data, which goes from 3.29 to 3.10. This would still be significantly above the midpoint,⁵⁶ indicating that privacy is more expected than not. The other eight searches would barely come closer.

The data obtained here do concern only a subset of possible search activities. These activities were originally chosen because they represented interesting questions at the intersection of law and technology. It is possible that, for other searches, we may see some gender differences. For example, some searches of the body may implicate different gender norms.⁵⁷

In our other scholarship, we have proposed that judges rely on social science evidence to inform their judgments about whether reasonable expectations of privacy exist under *Katz*. A personalized approach to criminal procedure law might contemplate expectations of privacy that are tied to the gender, race, or some other attribute of the surveillance's target. The data we present here indicate that—at least for this broad array of searches—it would not be useful to consider a citizen's demographic or personality characteristics in determining what expectations of privacy society ought to regard as reasonable. Some demographic and personality differences in privacy expectations are statistically significant, but they are not powerful enough to warrant divergent legal treatment, even on the assumption that such personalization is normatively unproblematic.

C. *Miranda*

Another body of criminal procedure law in which individual differences may be relevant involves knowledge of one's right to counsel and Fifth Amendment rights. *Miranda v Arizona*⁵⁸ requires law enforcement to inform arrestees of these constitutional rights before beginning an interrogation that will be used to collect admissible evidence against the arrestee.⁵⁹ In some limited

⁵⁶ $t(1199) = 2.38, p < 0.01$.

⁵⁷ See, for example, *Wilcher v City of Wilmington*, 139 F3d 366, 376 (3d Cir 1998) (discussing monitoring the bathroom use of male and female firefighters during drug testing). We thank Victoria Schwartz for this observation.

⁵⁸ 384 US 436 (1966).

⁵⁹ *Id.* at 467. The large body of empirical research on *Miranda* includes Paul G. Cassell and Bret S. Hayman, *Police Interrogation in the 1990s: An Empirical Study of the Effects of Miranda*, 43 UCLA L Rev 839, 860, 895 (1996) (reporting that *Miranda* rights were invoked in 16.3 percent of cases studied and that suspects with a prior criminal record did not invoke their rights significantly more often); Richard A. Leo, *Inside the Interrogation Room*, 86 J Crim L & Crimin 266, 275–77 (1996); Richard A. Leo, *The Impact of Miranda*

respects, the law already personalizes *Miranda* warnings. For example, arrestees who do not speak English should be read their *Miranda* rights in a language they understand, and courts sometimes struggle with issues involving mistranslations.⁶⁰ To take another example, a New Hampshire Supreme Court case called *State v Benoit*⁶¹ strongly encourages law enforcement officers in that state to read juvenile arrestees a version of *Miranda* warnings that is designed to be comprehensible to minors.⁶² Police officers who do not read the special juvenile version of *Miranda* to juveniles run the risk that any subsequent confession will be inadmissible.⁶³ This form of *Miranda* personalization is predicated on research suggesting that children have a harder time understanding the content of the *Miranda* warnings than adults do.⁶⁴

Although the Supreme Court has been relatively forgiving of minor variations in the content of the *Miranda* warnings and resists the idea of requiring the police to follow an invariable script,⁶⁵ lower courts on occasion view deviations from the standard script to be constitutionally problematic. Consider the Seventh Circuit's 2012 opinion in *United States v Wysinger*:⁶⁶

Agent Rehg veered slightly from the standard warning language in a few respects. A potentially serious misstatement of the *Miranda* warning occurred when Agent Rehg told Wysinger that he had the “right to talk to a lawyer for advice before we ask any questions or have one—have an attorney with you during questioning.” Taken literally, Agent Rehg

Revisited, 86 J Crim L & Crimin 621, 632–45 (1996); Richard Rogers, et al, “Everyone Knows Their Miranda Rights”: *Implicit Assumptions and Countervailing Evidence*, 16 Psychology, Pub Pol & L 300, 307–13 (2010).

⁶⁰ See Richard Rogers, et al, *Spanish Translations of Miranda Warnings and the Totality of the Circumstances*, 33 L & Hum Behav 61, 61–62 (2009).

⁶¹ 490 A2d 295 (NH 1985).

⁶² *Id* at 304, 306–07.

⁶³ The court says that, if the special warnings are not used, the courts will presume that the warnings the juvenile received were inadequate. *Id* at 304.

⁶⁴ See Raymond Chao, *Mirandizing Kids: Not as Simple as A-B-C*, 21 Whittier L Rev 521, 526 (2000); Thomas Grisso, *Juveniles' Capacities to Waive Miranda Rights: An Empirical Analysis*, 68 Cal L Rev 1134, 1151–60 (1980); Larry E. Holtz, *Miranda in a Juvenile Setting: A Child's Right to Silence*, 78 J Crim L & Crimin 534, 550–51 (1987).

⁶⁵ See, for example, *Duckworth v Eagan*, 492 US 195, 204–05 (1989); *Florida v Powell*, 559 US 50, 60–64 (2010).

⁶⁶ 683 F3d 784 (7th Cir 2012).

told Wysinger that he could talk to an attorney before questioning or during questioning. In fact, Wysinger had a right to consult an attorney both before and during questioning.⁶⁷

Largely (though not entirely) on the basis of this subtle deviation, the court deemed Wysinger's statements during an interrogation inadmissible.⁶⁸

This variation in the content of *Miranda* warnings, with courts considering enhanced-clarity warnings given to juveniles in *Benoit*, standard warnings for adults, and inadequate warnings in cases like *Wysinger*, lends itself to the possibility that different types of warnings may be ideal for different kinds of people.⁶⁹ Perhaps the *Benoit* warnings are particularly helpful for poorly educated respondents or those with less experience in the criminal justice system. And maybe the ambiguous *Wysinger* warnings have a significant impact on members of those groups but affect neither highly educated respondents nor people who watch a lot of police procedural shows on television. We sought to test those hypotheses.

Participants in this study were presented with a scenario describing a car accident after which they were arrested for reckless driving. In our control condition (no explicit *Miranda* warning), participants were simply told "A police officer . . . places you under arrest for reckless driving after surveying the crash scene. At the police station, the officer begins asking you questions about the collision after informing you of your legal rights." This subtle reference informs members of the control group that they have been informed of their legal rights without telling them what any of those rights actually are. This instruction is necessary because, in a hypothetical situation in which respondents were not read their rights, the police's ability to do anything with subsequent statements made in interrogations would be substantially curtailed, changing the correct answers to the true/false *Miranda* comprehension questions we administered to subjects, as we describe below.

In the three other experimental conditions, an explicit *Miranda* warning is given. The text of these warnings is included in Appendix F. One warning was intended to be the "standard" version

⁶⁷ Id at 798 (emphasis omitted).

⁶⁸ Id at 800–03.

⁶⁹ See Porat and Strahilevitz, 112 Mich L Rev at 1470–76 (cited in note 4) (discussing the personalization of information disclosures).

of *Miranda*: “You have the right to remain silent. Anything you say can be used against you in a court of law. . . .” Another was an “enhanced” and slightly longer version based on *Benoit*, which went into greater detail. And the third and last was the “degraded” version of *Miranda* held insufficient in *Wysinger* because it inappropriately implied that the suspect had to choose between having an attorney present before questioning *or* during questioning.

We devised a test of *Miranda* knowledge consisting of thirteen true/false questions. The text of these appears in Appendix G. Though overall performance was quite good, there was a fair bit of variation by question. For example, nearly everyone in all four experimental conditions (92 percent) knew it was true that “you do not have to say anything to the police, but you can speak to the police if you want to,” but only 54.5 percent of participants correctly stated that it was false that “the police can interview you, but they can only use what you say to prosecute you if you make a signed confession.”⁷⁰

Participants’ scores on this *Miranda* quiz were analyzed using the same basic regression as in the other sections. The results, which are presented in Table 5 below, were surprising. First, *none* of the *Miranda* warnings significantly improved performance (see the coefficients for the “Any Warning” variable). We prevented subjects from skipping past the *Miranda* instructions for at least thirty seconds (though they could have spent a longer time with them if they wished), yet still there was no improvement. And time spent with neither the *Miranda* instructions nor the *Miranda* quiz page, which reprinted the instructions, correlated with performance. Also, our “enhanced” and “degraded” *Miranda* variants were no different from the standard warning.

Second, the version of *Miranda* that was chosen made no significant difference in terms of subjects understanding their constitutional rights—even when we focus on responses to those questions for which a clear warning would seem to accomplish the most. Two of our true/false questions stand out in this respect.

⁷⁰ Several papers by psychologists have previously examined research subjects’ comprehension of their *Miranda* rights. See, for example, Richard Rogers, et al, *General Knowledge and Misknowledge of Miranda Rights: Are Effective Miranda Advisements Still Necessary?*, 19 *Psychology, Pub Pol & L* 432, 434–38 (2013) (showing that, in a free recall task, 87 percent of subjects knew they had a right to remain silent and 80 percent of subjects knew they had a right to counsel); Rogers, et al, 31 *Behav Sci & L* at 403–04 (cited in note 36); Darby B. Winningham, Richard Rogers, and Eric Y. Drogin, *Miranda Misconceptions of Criminal Detainees: Differences Based on Age Groups and Prior Arrests*, 17 *Intl J Forensic Mental Health* 13, 18–20 (2018).

The first asked respondents whether this statement was true or false: “If you a request a lawyer, you must choose whether to have the lawyer present either before questioning or during questioning. A lawyer cannot be present both before and during questioning.” Respondents who saw the enhanced *Miranda* warning (which stated, in pertinent part: “You have the right to talk to a lawyer before any questioning. You have the right to have the lawyer with you while you are being questioned. The lawyer will help you decide what you should do or say.”) were not significantly more likely to answer the question correctly than those who saw the defective *Wysinger* warning. And neither group performed significantly better than the control group members, who were shown no explicit warning.⁷¹

Similarly, when asked whether this statement is true or false—“If you do not answer any of the police’s questions you may be punished for being in contempt of court.”—respondents who saw the enhanced warning (which read in pertinent part: “You do not have to talk to anyone or answer any questions we ask you. You will not be punished for deciding not to talk to us.”) did not significantly outperform those who saw the standard *Miranda* warning or the defective warning (both of which reminded subjects of their “right to remain silent”), nor did they provide correct answers significantly more frequently than those who received no explicit warning.⁷²

Third, in terms of demographic correlates, greater education and, especially, age were strongly related to improved *Miranda* knowledge,⁷³ and being African American was related to weaker knowledge. Because the dependent variable here is a percentile, one can think of the unstandardized coefficients below in terms of percentage points. For every year older a participant was, the participant did 0.2 percent better. This means that it would take a difference of about forty years to score one full question differently on the test (each question counting for about 7.7 percent of

⁷¹ The percentages of respondents providing incorrect answers were 30.1 percent of those given no warning, 29.5 percent of those given the standard warning, 28.5 percent of those given the enhanced warning, and 29.9 percent of those given the defective warning. None of these differences is statistically significant.

⁷² The percent of respondents providing incorrect answers were 21.3 percent of those given no warning, 21.0 percent of those given the standard warning, 15.6 percent of those given the enhanced warning, and 15.4 percent of those given the defective warning. These results were again not statistically significant.

⁷³ Our results here replicate those of Rogers, et al, 19 *Psychology*, Pub Pol & L at 438 (cited in note 70), which studied a sample drawn from the Dallas jury pool.

the total score). This is a sample of entirely adult participants, however, and these data show that additional years are beneficial even among those above 18 years of age. Arrest experience was also related to improved knowledge (almost 5 percent better on the test),⁷⁴ as was conscientiousness (1.5 percent for each point of a possible 7). Authoritarian personality propensity was related to weaker knowledge (-2.5 percent for each point of a possible 6). Although we hypothesized that subjects who regularly watch police procedural television shows might perform better on the true/false test than subjects who did not, the result actually went in the opposite direction, though it was not significant.

We ran a further series of regressions examining whether the effects of age and education—two of the better predictors—depended on the *Miranda* warning provided. There were no significant interactions between those factors and either getting any warning or getting the enhanced or defective warnings.⁷⁵ This means that those warnings were not having significantly different effects on people depending on their age or educational attainment.

⁷⁴ Since arrest experience would, if direct, involve being given the *Miranda* warning, we added this in a separate step in this analysis rather than including it in the first model as we did for the other domains.

⁷⁵ The model included all of the controls from Model 3.

TABLE 5: PREDICTORS OF KNOWLEDGE OF ONE'S *MIRANDA* RIGHTS

	Model 1		Model 2		Model 3		Model 4	
	Unstd Coeff	Std Coeff	Unstd Coeff	Std Coeff	Unstd Coeff	Std Coeff	Unstd Coeff	Std Coeff
(Constant)	0.632 (0.021)		0.616 (0.022)		0.596 (0.025)		0.559 (0.052)	
Female	0.000 (0.009)	-0.001	0.000 (0.009)	0.000	0.004 (0.009)	0.012	0.001 (0.009)	0.003
Hispanic	-0.007 (0.013)	-0.018	-0.008 (0.013)	-0.019	-0.006 (0.013)	-0.014	-0.013 (0.013)	-0.031
African American	-0.026 (0.014)	-0.055	-0.026 (0.014)	-0.054	-0.029 (0.014)	-0.062 *	-0.037 (0.014)	-0.078 **
Education	0.016 (0.004)	0.109 ***	0.016 (0.004)	0.107 ***	0.018 (0.004)	0.120 ***	0.014 (0.004)	0.093 **
Age	0.002 (0.000)	0.237 ***	0.002 (0.000)	0.238 ***	0.002 (0.000)	0.248 ***	0.002 (0.000)	0.247 ***
Midwest	0.014 (0.014)	0.035	0.016 (0.014)	0.040	0.012 (0.014)	0.030	0.012 (0.014)	0.031
South	0.013 (0.012)	0.038	0.013 (0.012)	0.041	0.006 (0.012)	0.019	0.011 (0.012)	0.033
West	0.018 (0.014)	0.046	0.019 (0.014)	0.049	0.015 (0.014)	0.039	0.019 (0.014)	0.047
Any Warning			0.019 (0.013)	0.052	0.019 (0.012)	0.050	0.019 (0.012)	0.050
Enhanced Warn			-0.002 (0.012)	-0.006	-0.001 (0.012)	-0.002	-0.004 (0.012)	-0.011
Defective Warn			0.004 (0.012)	0.012	0.006 (0.012)	0.017	0.003 (0.012)	0.009
Self or Friend Arrested					0.051 (0.009)	0.159 ***	0.048 (0.009)	0.151 ***
Watch Crime Shows					-0.005 (0.003)	-0.041	-0.004 (0.003)	-0.029
Agreeableness							0.005 (0.005)	0.034
Extroversion							-0.004 (0.003)	-0.037
Conscientious							0.015 (0.005)	0.107 **
Neuroticism							0.006 (0.004)	0.048
Openness							0.006 (0.004)	0.046
Authoritarianism							-0.024 (0.006)	-0.134 ***
Conservatism							-0.001 (0.003)	-0.005
R^2	0.08		0.08		0.11		0.14	

Note: Northeast was used as the regional reference group. *** indicates $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. The normal *Miranda* warning serves as the reference category for the enhanced and defective warnings. Conservatism was measured on a scale ranging from 1 (Very Liberal) to 7 (Very Conservative). The Big Five scales ranged from 1 to 7; authoritarianism was 1 to 6.

These data provide interesting insights about the benefits of personalization, which would seem to be insubstantial in the context of tailored *Miranda* warnings. But the data are revealing in other respects as well. In the present day and age, neither the *Miranda* warnings themselves nor an enhanced version of those warnings significantly improve respondents' understanding of the legal rights about which *Miranda* purports to inform them. This result is consistent with recent scholarship that suggests little benefit from repeated *Miranda* warnings to detainees and with broader scholarship that expresses general skepticism about the value of information disclosures aimed at lay audiences.⁷⁶ As one study of juveniles' understanding of their *Miranda* rights puts it, warnings that "explain components and protections more fully [have] resulted in much longer warnings that likely decrease *Miranda* understanding rather than improving it."⁷⁷

The lack of variation among those receiving enhanced warnings, degraded warnings, standard warnings, and no warnings at all suggests that cases like *Wysinger*, which use relatively minor deviations from the standard *Miranda* warnings to exclude confessions, are wrongly decided.⁷⁸ By the same token, Supreme Court cases like *Duckworth v Eagan*⁷⁹ and *Florida v Powell*,⁸⁰ which take a relatively permissive approach toward imperfect readings of *Miranda* rights, are likely correct. Had the arrestees in those cases heard a typical or even enhanced version of the *Miranda* warnings, it likely would have not changed their understandings of their rights, nor would it have been likely to change their subsequent behavior. In sum, a secondary but important takeaway of this Essay is that the kinds of legal issues routinely presented in inadequate *Miranda* warning cases like *Wysinger*, *Duckworth*, and *Powell* are amenable to empirical resolution: one can examine how a standard warning and the nonstandard warning given in a

⁷⁶ See, for example, Rogers, et al, 31 Behav Sci & L at 405–07 (cited in note 36). See also Omri Ben-Shahar and Carl E. Schneider, *More than You Wanted to Know: The Failure of Mandated Disclosure* 55–57 (Princeton 2014).

⁷⁷ Winningham, Rogers, and Drogin, 17 Intl J Forensic Mental Health at 22 (cited in note 70).

⁷⁸ One could argue that strict adherence to the approved version of *Miranda* is valuable for other reasons, perhaps out of a general preference for bright-line rules. These data do not speak to the merits of those arguments, but they do suggest that small variations will not degrade comprehension.

⁷⁹ 492 US 195 (1989).

⁸⁰ 559 US 50 (2010).

particular case each affects subjects' respective understandings of their legal rights.

Despite this dreary picture of warning efficacy, we do not think that these results necessarily mean that *Miranda* is useless. First, it's striking that lay respondents overwhelmingly knew the answers to basic factual questions about *Miranda*, with respondents answering the least challenging true/false questions correctly even when they were not presented with any warnings. Recall that 92 percent knew the most basic part of *Miranda*: that they did not need to talk to the police. We believe these results indicate significant legal knowledge on the part of the lay public, knowledge that plausibly has been transmitted through previous exposure to the *Miranda* warnings (though watching lots of *Law & Order* episodes appears not to have helped).⁸¹

Other research suggests that jail inmates generally understand their basic *Miranda* rights, though their knowledge is imperfect, and very difficult questions that aren't directly covered by the warnings trip them up.⁸² This penetration of the *Miranda* warnings may mean that eliminating the *Miranda* requirement would, given enough time, result in citizens having a diminished understanding of their legal rights.⁸³ Second, it is fair to wonder about the external validity of our experiments. Respondents may respond differently to a hypothetical in which they imagine themselves being taken into custody than they would were they actually placed under arrest. It may be that people are prone to forget their rights when under such strain or that warnings are listened to even more attentively, altering comprehension.⁸⁴ Alternatively, it is possible that real-world *Miranda* warnings are directed at people who are so distraught and flooded with emotions that the

⁸¹ Some research has suggested that pop culture is now generally omitting *Miranda* warnings, with them being absent from most episodes of recent police procedurals. See Ronald Steiner, Rebecca Bauer, and Rohit Talwar, *The Rise and Fall of the Miranda Warnings in Popular Culture*, 59 *Cleve St L Rev* 219, 231–35 (2011).

⁸² See Rogers, et al, 31 *Behav Sci & L* at 406 (cited in note 36). For example, research suggests that detainees often do not realize that *Miranda* applies to noncustodial situations or that, during an interrogation, law enforcement can falsely claim that eyewitnesses have identified them at crime scenes. See Rogers, et al, 19 *Psychology, Pub Pol & L* at 437 (cited in note 70). These misconceptions are unfortunate, but they are not errors that the *Miranda* warnings themselves are designed to correct.

⁸³ Our previous research suggests it is unlikely that citizens' understandings would change on a dime, and any immediate changes might not persist. See Kugler and Strahilevitz, 84 *U Chi L Rev* at 1794 (cited in note 49).

⁸⁴ There is a literature on this that tries to simulate mock arrests for research subjects, see Rogers, et al, 19 *Psychology, Pub Pol & L* at 439 (cited in note 70), but no research that examines subjects actually being interrogated on suspicion of having broken the law.

warnings do even less to enhance citizens' understandings of their legal rights.⁸⁵

CONCLUSION

A data-driven approach to personalization may look attractive in comparison to the status quo, in which judges and justices are forced to rely on their own, perhaps idiosyncratic, views about what's reasonable or custodial. And we should not kid ourselves—the criminal justice system already tolerates a degree of disparate treatment across protected classes. Personalization based on race, age, and sex is not constitutionally unthinkable even though it raises hard normative questions and should generate careful constitutional scrutiny.

Personalization could, in principle, benefit otherwise disadvantaged minority groups. Our data could have shown that African Americans see police requests for permission to search as more coercive or that those of low educational attainment need carefully tailored *Miranda* warnings. At least in theory, the upside of a personalized Fourth Amendment could be significant for these groups—it might, paradoxically, promote equal treatment under the law. Had those been our findings, the next step would have been to address those hard questions.

Our data do not support this story, however. Though we find several statistically significant relationships between demographic characteristics and relevant outcomes, the effect sizes are small. Even were it normatively and logistically costless to personalize on all these dimensions, little would be gained. Demographic and personality factors have only modest effects on expectations and beliefs relevant to criminal procedure doctrine. Personalization of this sort does not appear to be worth the trouble.

Our data are less relevant to another kind of personalization: the kind pursued by machine learning and the techniques of big data. We tested how basic demographic variables, including those representing protected class status, were related to outcomes. But one could take a black box approach, trying to sort people based on all possible factors. Though the federal courts have approved of some types of personalization at the individual level, neither

⁸⁵ For an exploration of related issues involving interrogation and a clever way to make progress in experimental settings, see Melissa B. Russano, et al, *Investigating True and False Confessions within a Novel Experimental Paradigm*, 16 *Psychological Sci* 481, 483–84 (2005).

courts nor scholars have examined in depth how criminal procedure could incorporate the kind of automated big data personalization that companies like Google and Netflix have introduced. Personalization by these methods would be less about protecting or tailoring for identified classes of people and more about customizing based on individual predilections.

Big data personalization presumably could be more accurate than what we did here. There is research suggesting that personalization via machine learning improves the relevance of search engine results by a little more than 9 percent.⁸⁶ And machine learning is starting to make inroads in predicting more complex kinds of human behavior too. For example, these techniques have shown some significant promise in making bail determinations, with a machine learning algorithm projected to be able to reduce crime by almost 19 percent while holding the release rate constant.⁸⁷ Machine learning's advantage over human decision-making seems less pronounced in the hiring and firing context, with recent research suggesting that using machine learning to decommission the bottom 10 percent of new law enforcement hires (replacing them with median hires) could reduce police shootings by approximately 5 percent.⁸⁸ That said, machine learning is not a panacea for prediction. To the extent that machine learning attaches relevance to factors that, unlike race, gender, age, or education levels, can be easily altered, it is susceptible to gaming strategies that can compromise its efficacy.⁸⁹

Despite these possibilities for the future, the magnitude of our effects in the present research is not large enough to justify personalization. This conclusion means that some very difficult

⁸⁶ Hema Yoganarasimhan, *Search Personalization Using Machine Learning* *5 (unpublished manuscript, July 2017), archived at <http://perma.cc/T2W4-CAQF>. See also Michael Crawford, et al, *Survey of Review Spam Detection Using Machine Learning Techniques*, 2 *J Big Data* 23, 39–40 (2015) (reviewing the literature on advances in detecting spam and fake reviews on sites like TripAdvisor and Yelp).

⁸⁷ See Jon Kleinberg, et al, *Human Decisions and Machine Predictions* *8 (NBER working paper, Feb 2017), archived at <http://perma.cc/8FPY-Y4A7>.

⁸⁸ See Aaron Chalfin, et al, *Productivity and Selection of Human Capital with Machine Learning*, 106 *Am Econ Rev* 124, 125 (2016). Professor Aaron Chalfin and coauthors' 4.81 percent reduction estimate arguably understates the effect somewhat because officer assignments may be nonrandom and systematically place better officers in more difficult situations.

⁸⁹ For discussion of the problems raised by adversarial machine learning, see Marco Barreno, et al, *The Security of Machine Learning*, 81 *Machine Learning* 121, 123–24 (2010); Battista Biggio, et al, *Evasion Attacks against Machine Learning at Test Time*, in Hendrik Blockeel, et al, eds, *Machine Learning and Knowledge Discovery in Databases III*, 387, 400–01 (Springer 2013).

normative questions about the appropriateness of further personalizing this body of constitutional law can wait for another day.

APPENDIX

A. Demographics of the Sample

Female (%)	52.3
<i>Age (years)</i>	
Median	46
Mean	46.86 (17.01)
<i>Political Orientation</i>	4.05 (1.73)
<i>Race/Ethnicity (%)</i>	
White	83.4
Black or AA	13.0
Indian or Native	1.1
SE Asian	4.5
Hawaiian/Pacific	0.6
Other or multiracial	7.4
Hispanic	17.6
<i>Education (%)</i>	
Less than HS	7.4
HS Diploma/GED	36.4
Two-Year College	29.3
Four-Year College	17.8
Graduate Degree	9.1
	1,200

Note: For age and political orientation, the numbers in parentheses represent standard deviations. Political orientation was measured on a seven-point scale ranging from 1 (Very Liberal) to 7 (Very Conservative).

B. Regression Table for Expected Nervousness during Searches

	Model 1				Model 2			
	Unstd Coeff		Std Coeff		Unstd Coeff		Std Coeff	
(Constant)	2.900	(0.187)			2.775	(0.439)		
Female	0.325	(0.081)	0.121	***	0.242	(0.081)	0.090	**
Hispanic	-0.099	(0.112)	-0.028		-0.111	(0.108)	-0.031	
African American	-0.214	(0.123)	-0.054		-0.135	(0.120)	-0.034	
Education	0.104	(0.037)	0.084	**	0.117	(0.036)	0.094	***
Age	-0.004	(0.002)	-0.057		-0.002	(0.002)	-0.026	
Midwest	-0.091	(0.122)	-0.028		-0.095	(0.117)	-0.029	
South	-0.188	(0.108)	-0.068		-0.138	(0.104)	-0.050	
West	-0.092	(0.124)	-0.027		-0.061	(0.119)	-0.018	
Self/Friend Arrested	0.111	(0.079)	0.041		0.077	(0.076)	0.028	
Agreeableness					0.116	(0.041)	0.094	**
Extroversion					-0.075	(0.029)	-0.073	*
Conscientious					-0.020	(0.040)	-0.017	
Neuroticism					0.255	(0.034)	0.252	***
Openness					-0.046	(0.035)	-0.041	
Authoritarianism					-0.190	(0.047)	-0.125	***
Conservatism					-0.023	(0.024)	-0.029	

Note: Higher numbers indicate greater nervousness. Northeast was used as the regional reference group. *** indicates $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Conservatism was measured on a scale ranging from 1 (Very Liberal) to 7 (Very Conservative). The Big Five scales ranged from 1 to 7; authoritarianism was 1 to 6. Model 1 $R^2 = 0.022$; Model 2 $R^2 = 0.109$.

C. Regression Table for Expectation the Officer Would Draw His Gun during the Car Stop

	Model 1				Model 2			
	Unstd Coeff	Std Coeff			Unstd Coeff	Std Coeff		
(Constant)	2.533	(0.159)			3.476	(0.383)		
Female	-0.076	(0.069)	-0.032		-0.032	(0.071)	-0.014	
Hispanic	0.195	(0.095)	0.062	*	0.167	(0.094)	0.053	
African American	0.747	(0.104)	0.211	***	0.749	(0.104)	0.211	***
Education	0.056	(0.031)	0.051		0.038	(0.032)	0.034	
Age	-0.013	(0.002)	-0.180	***	-0.010	(0.002)	-0.139	***
Midwest	-0.200	(0.103)	-0.069		-0.212	(0.102)	-0.073	*
South	-0.175	(0.092)	-0.071		-0.143	(0.091)	-0.058	
West	-0.145	(0.105)	-0.049		-0.155	(0.104)	-0.052	
Self/Friend Arrested	0.153	(0.067)	0.064	*	0.113	(0.066)	0.047	
Agreeableness					-0.071	(0.036)	-0.065	*
Extroversion					0.047	(0.025)	0.052	
Conscientious					-0.065	(0.035)	-0.061	
Neuroticism					0.030	(0.030)	0.034	
Openness					-0.004	(0.031)	-0.004	
Authoritarianism					-0.111	(0.041)	-0.082	**
Conservatism					-0.037	(0.021)	-0.053	

Note: Higher numbers indicate greater expectation of the event. Northeast was used as the regional reference group. *** indicates $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Conservatism was measured on a scale ranging from 1 (Very Liberal) to 7 (Very Conservative). The Big Five scales ranged from 1 to 7; authoritarianism was 1 to 6. Model 1 $R^2 = 0.107$; Model 2 $R^2 = 0.139$.

D. Regression Table for Expectation the Officer Would Plant Evidence during the Car Stop

	Model 1				Model 2			
	Unstd Coeff	Std Coeff			Unstd Coeff	Std Coeff		
(Constant)	2.533	(0.161)			3.430	(0.386)		
Female	-0.081	(0.070)	-0.034		-0.042	(0.071)	-0.018	
Hispanic	0.113	(0.096)	0.036		0.069	(0.095)	0.022	
African American	0.695	(0.106)	0.197	***	0.696	(0.105)	0.197	***
Education	0.015	(0.032)	0.014		-0.010	(0.032)	-0.009	
Age	-0.010	(0.002)	-0.140	***	-0.006	(0.002)	-0.090	**
Midwest	0.070	(0.105)	0.024		0.053	(0.103)	0.018	
South	0.004	(0.093)	0.002		0.042	(0.092)	0.017	
West	-0.008	(0.107)	-0.003		-0.008	(0.105)	-0.003	
Self/Friend Arrested	0.146	(0.068)	0.061	*	0.095	(0.067)	0.040	
Agreeableness					-0.048	(0.036)	-0.044	
Extroversion					0.015	(0.026)	0.017	
Conscientious					-0.068	(0.035)	-0.065	
Neuroticism					0.060	(0.030)	0.067	*
Openness					0.035	(0.031)	0.035	
Authoritarianism					-0.183	(0.042)	-0.136	***
Conservatism					-0.026	(0.021)	-0.038	

Note: Higher numbers indicate greater expectation of the event. Northeast was used as the regional reference group. *** indicates $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Conservatism was measured on a scale ranging from 1 (Very Liberal) to 7 (Very Conservative). The Big Five scales ranged from 1 to 7; authoritarianism was 1 to 6. Model 1 $R^2 = 0.074$; Model 2 $R^2 = 0.118$.

E. Reasonable Expectation of Privacy Questions

Participants were asked, “Would it violate people’s reasonable expectations of privacy if law enforcement:

- Used remote activation software to turn on the webcam on their laptop without their permission?
- Obtained from their Internet Service Provider copies of emails exchanged between them and someone else?
- Used facial recognition software to check whether any of the fans entering the Super Bowl stadium match images in a Department of Homeland Security database?
- Installed a video camera to watch a public park where criminal activity has recently occurred?
- Obtained from their cell phone company stored information about whether their cell phone was near a particular location on a particular day?
- Used a fake cell tower to trick their phone into giving the police more accurate information about where the phone is?
- Contacted Uber and obtained a map of every trip a customer has taken using that ride-hailing service for the last month?
- Used a high-powered lens to take photographs through a window of a home from across the street?
- Flew a camera-equipped drone over a house at a height of seventy feet to take pictures of the backyard, which is otherwise not viewable from the surrounding properties?”

F. *Miranda* Vignettes

Control:

Suppose that you have been driving a car and gotten into a serious collision with another vehicle. You are unharmed but a driver in another vehicle was not wearing a seatbelt and was killed in the collision. A police officer arrives quickly at the scene and places you under arrest for reckless driving after surveying the crash scene. At the police station, the officer begins asking you questions about the collision after informing you of your legal rights.

Standard warning:

Suppose that you have been driving a car and gotten into a serious collision with another vehicle. You are unharmed but

a driver in another vehicle was not wearing a seatbelt and was killed in the collision. A police officer arrives quickly at the scene and places you under arrest for reckless driving after surveying the crash scene.

When placing you under arrest, the officer tells you that he is required by law to inform you of your legal rights. He then says, "You have the right to remain silent. Anything you say can be used against you in a court of law. You have the right to talk to a lawyer for advice before we ask you any questions. You have the right to have a lawyer with you during questioning. If you cannot afford a lawyer, one will be appointed for you without cost to you before questioning." At the police station, the officer begins asking you questions about the collision.

Enhanced warning:

Suppose that you have been driving a car and gotten into a serious collision with another vehicle. You are unharmed but a driver in another vehicle was not wearing a seatbelt and was killed in the collision. A police officer arrives quickly at the scene and places you under arrest for reckless driving after surveying the crash scene.

When placing you under arrest, the officer tells you that he is required by law to inform you of your legal rights. He then says, "You do not have to talk to anyone or answer any questions we ask you. You will not be punished for deciding not to talk to us. If you say anything, what you say can be used in a court to prove that you may have broken the law. You have the right to talk to a lawyer before any questioning. You have the right to have the lawyer with you while you are being questioned. The lawyer will help you decide what you should do or say. If you decide you want a lawyer, we will not question you until you have been allowed to talk to the lawyer. If you want to talk to a lawyer and cannot afford one, we will get you a lawyer at no cost to you before any questioning begins." At the police station, the officer begins asking you questions about the collision.

Defective Warning:

Suppose that you have been driving a car and gotten into a serious collision with another vehicle. You are unharmed but a driver in another vehicle was not wearing a seatbelt and was killed in the collision. A police officer arrives quickly at

the scene and places you under arrest for reckless driving after surveying the crash scene.

When placing you under arrest, the officer tells you that he is required by law to inform you of your legal rights. He then says, "Before we ask any questions, you must understand you have a right to remain silent. Anything you say can be used against you in court. You have a right to talk to a lawyer for advice before we ask any questions or have an attorney with you during questioning. If you can't afford a lawyer, one will be appointed for you before we ask any questions." At the police station, the officer begins asking you questions about the collision.

G. *Miranda* Questions

These questions were administered in random order.

Question	Correct Response	% Correct
If you speak before asking for a lawyer, the police may use whatever you say to prosecute you if they believe you have broken the law.	TRUE	93.8
You do not have to say anything to the police, but you can speak to the police if you want to.	TRUE	92.0
If you decide you want to speak with the police without a lawyer present, you can later choose to stop talking and ask for a lawyer. If you do, the police have to stop questioning you until the lawyer arrives	TRUE	90.8
If you cannot afford a lawyer but want one anyway, the government will assign you a lawyer who does not have to share what you tell him with the police	TRUE	87.4
If you tell the police officer you want to speak with a lawyer, the police may not question you until after you have spoken with the lawyer unless you restart the conversation.	TRUE	86.2
If you request a lawyer, you do not need to tell the police what you know after you have spoken with the lawyer.	TRUE	77.1
You do not have to tell your lawyer everything you know if you request a lawyer.	TRUE	48.8
The police can interview you, but they can only use what you say to prosecute you if you make a signed confession.	FALSE	54.5
If you cannot afford a lawyer but want one anyway, the government will assign you a lawyer who works for the police department.	FALSE	66.4
If you a request a lawyer, you must choose whether to have the lawyer present either before questioning or during questioning. A lawyer cannot be present both before and during questioning.	FALSE	70.5
If you do not answer any of the police's questions you may be punished for being in contempt of court.	FALSE	81.7
You have to answer all the police officer's questions.	FALSE	85.9
If you request a lawyer, the police officer will prevent you from being released on bail.	FALSE	88.0