

IN THE SUPREME COURT OF THE STATE OF OREGON

STATE OF OREGON,

Plaintiff-Respondent,
Petitioner on Review,

v.

WILLIAM RICK DELONG,

Defendant-Appellant,
Respondent on Review,

Douglas County Circuit Court Case
No. 09CR1050FE

CA A146907

SC S062176

BRIEF ON THE MERITS OF *AMICI CURIAE*
OREGON JUSTICE RESOURCE CENTER,
ALBINA MINISTERIAL ALLIANCE
COALITION FOR JUSTICE AND POLICE
REFORM, THE PORTLAND CHAPTER OF
THE NATIONAL LAWYERS GUILD, INC.,
AND AMERICAN CIVIL LIBERTIES UNION
FOUNDATION OF OREGON, INC.

On Review of the Opinion of the Court of Appeals
On an appeal from a judgment of the Circuit Court for Douglas County
Honorable Joan G. Seitz, Judge

Court of Appeals Opinion filed: January 29, 2014
Author of Opinion: Nakamoto, Judge
Concurring: Armstrong, Presiding Judge, and Egan, Judge

PETER GARTLAN, #870467

Chief Defender

DANIEL C. BENNETT, #073304

Senior Deputy Public Defender

Office of Public Defense Services

1175 Court Street NE

Salem, OR 97301

(503) 378-3349

Dan.Bennett@opds.state.or.us

Attorneys for Respondent on Review

William Rick DeLong

SHAUNA M. CURPHEY, #063063

Curphey & Badger, P.A.

520 SW Sixth Avenue, Ste. 1040

Portland, OR 97204

(503) 241-2848

scurphey@curphey.com

SARA F. WERBOFF, #105388

Janet Hoffman & Associates

1000 SW Broadway, Ste. 1500

Portland, OR 97205

(503) 222-1125

sara@jhoffman.com

JORDAN R. SILK, #105031

Schwabe, Williamson & Wyatt, P.C.

1211 SW Fifth Avenue, Ste. 1900

Portland, OR 97204

(503) 222-9981

jsilk@schwabe.com

Attorneys for Amici Curiae Oregon

Justice Resource Center, The AMA

Coalition, the Portland Chapter of the

National Lawyers Guild, and the

American Civil Liberties Union

Foundation of Oregon

ELLEN F. ROSENBLUM, #753239

Attorney General

ANNA M. JOYCE, #013112

Solicitor General

MICHAEL A. CASPER, #062000

Assistant Attorney General

400 Justice Building

1162 Court Street NE

Salem, OR 97301

(503) 378-4402

michael.casper@doj.state.or.us

Attorneys for Petitioner on Review

State of Oregon

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OREGON, INC.**

INTRODUCTION

The Oregon Justice Resource Center (OJRC) is a non-profit organization founded in 2011. OJRC works to “dismantle systemic discrimination in the administration of justice by promoting civil rights and enhancing the quality of legal representation to traditionally underserved communities.” OJRC Mission Statement, www.ojrc.info/mission-statement. The OJRC Amicus Committee is comprised of Oregon attorneys from multiple disciplines and law students from Lewis & Clark Law School, where OJRC is located.¹

The Albina Ministerial Alliance Coalition (AMA Coalition) is an umbrella group of individuals and community organizations at the forefront of community organizing for police accountability and oversight in Portland. The AMA Coalition was founded in 2003 after Kendra James, a young African-American woman, was shot during a traffic stop. In 2010, the AMA Coalition participants coalesced around the following goals: (1) A federal investigation

¹ Undersigned counsel would like to thank and specifically credit law students Justin Withem and Michael Beilstein for their excellent research assistance.

by the Justice Department to include criminal and civil rights violations, as well as a federal audit of patterns and practices of the Portland Police Bureau (PPB); (2) Strengthening the Independent Police Review Division and the Citizen Review Committee with the goal of adding power to compel testimony; (3) A full review of PPB's excessive-force and deadly-force policies and training with diverse citizen participation for the purpose of making recommendations to change policies and training; (4) Lobbying the Oregon State Legislature to narrow the language of the State statute for deadly force used by police officers; (5) Establishing a special prosecutor for police excessive-force and deadly-force cases. The AMA Coalition pursues these goals with an emphasis on teamwork among its diverse members and on the principles of non-violent direct action enunciated by Dr. Martin Luther King, Jr.

The National Lawyers Guild, Inc. (NLG) is a non-profit corporation formed in 1937 as the nation's first racially integrated voluntary bar association, with a mandate to advocate for fundamental principles of human and civil rights including the protection of rights guaranteed by the United States Constitution. Since then the NLG has been at the forefront of efforts to develop and ensure respect for the rule of law and basic legal principles. The Portland Chapter of NLG seeks to implement these goals in Portland and in Oregon as a whole, with a particular emphasis on police accountability and reform. It serves as a legal

support to local progressive organizations and a progressive voice within the local legal community.

The American Civil Liberties Union Foundation of Oregon, Inc. (ACLU) is a nonprofit, nonpartisan, corporation dedicated to maintaining the civil rights and liberties guaranteed or reserved to the people by the Oregon and United States constitutions; to that end, the ACLU has appeared in numerous cases in this and other Oregon courts as *amicus curiae* concerning civil liberties generally.

Amici curiae wish to be heard by this court because the preservation of the robust individual rights and liberties afforded by the Oregon Constitution is a primary concern of all *amici*. Ensuring the strength of those rights and liberties is critically important in the context of police-citizen encounters like the one in this case. In such cases, police officer and citizen meet without the presence of a judicial officer to ensure that constitutional limits proscribe the police's investigation. And no advocate is present to ensure that the investigation's subject is fully aware of his or her rights—and fully confident in his or her entitlement to assert them.

As demonstrated by the empirical data discussed below, the power dynamics at play in the average police-citizen encounter exert significant pressure over the individual and frequently deprive the individual of his or her ability to determine dispassionately whether to protect his or her rights and to

decline to cooperate with law enforcement. In short, the empirical data shows that in the average police-citizen encounter, an individual's consent to the police's exercise of authority is a product of the social context in which that authority is exercised. *Amici* respectfully submit that, in crafting the rule that will govern this case and all others like it, the reality of the pressures that are brought to bear on individuals during police-citizen encounters should guide this court in determining when consent is the product of a prior illegality. In recognizing the ways in which individuals feel compelled to consent to police authority, this court can ensure the vindication of the individual rights guaranteed by this state's constitution.

SUMMARY OF ARGUMENT²

Amici urge this court to adopt the test proposed by defendant, Mr. Delong, as the correct test for determining when evidence is derived from a *Miranda* violation. A police officer violates an individual's rights under Article I, section 12, of the Oregon Constitution by questioning that individual in circumstances that are inherently compelling without first providing *Miranda* warnings and obtaining a knowing, intelligent, and voluntary waiver of the individual's Article I, section 12, right to remain silent. *State v. Vondehn*, 348 Or 462, 474, 236 P3d 691 (2010). When a police officer fails to provide

² *Amici* adopt defendant's questions presented and proposed rules of law.

Miranda warnings, a court must suppress statements and physical evidence derived from that constitutional violation. *Id.*; *State v. Jarnagin*, 351 Or 703, 713, 351 P3d 535 (2012). Physical evidence derives from a *Miranda* violation when the evidence's discovery is a foreseeable result of unlawful custodial interrogation. Such evidence must be suppressed unless the state demonstrates that some intervening factor has severed the causal connection between the Article I, section 12, violation and the discovery of the physical evidence. Defendant's proposed test presents a sensible, workable solution that protects adequately the important individual rights afforded by the Oregon Constitution. To hold otherwise would allow the State to use evidence obtained as a result of an inherently coercive interaction to convict individuals of crimes. Oregon law forbids that outcome.

Abundant social science evidence supports the conclusion that an individual's unwarned statements during a custodial interrogation are the product of an inherently coercive interaction. Studies of the factors that affect obedience—including the authority of a person in uniform, the social context, physical proximity, and the time pressure on the individual to provide a response—establish the coercive power police officers wield. And, more specifically, research on individuals' compliance with officers' requests—and the reasons for that compliance—demonstrates further the coercive nature of police interactions with civilians. Those studies all point to the same

conclusion: The social dynamics at play during the police encounter in this case were inherently compelling, such that defendant's purported "consent," and the physical evidence obtained as a result of that consent, derived from the officer's violation of defendant's *Miranda* rights under Article 1, section 12.

ARGUMENT

I. The officers' discovery of physical evidence derived from their violation of Article I, section 12.

Amici agree with defendant's proposed rule: evidence obtained as a foreseeable result of unlawful custodial interrogation derives from that unlawful interrogation and must be suppressed. As this court has recognized, "when a suspect is subjected to custodial interrogation, [*Miranda*] warnings are necessary 'because of the inherent level of coercion that exists in such interrogations.'" *State v. Vondehn*, 348 Or 462, 471, 236 P3d 691 (2010). Social science research supports that conclusion. Research on the social psychology of obedience and the effect of social context on meaning demonstrates the coercive power brought to bear on individuals during police interactions. Studies on the rate of compliance with police officers and the reasons that people submit to authority further demonstrate that police wield coercive power even in settings far less restrictive than the one defendant faced here. Thus, the state's suggestion that defendant's statement that officers could

search his car was not derived from the *Miranda* violation ignores the reality of police-citizen encounters.³

A. Individuals' deference to authority figures imbues police officers with tremendous coercive power.

Decades of social-psychology research demonstrates that “momentary situational pressures and norms (*e.g.*, rules of deference to an authority) can exert a surprising degree of influence on people’s behavior.” Thomas Blass, *Understanding Behavior in the Milgram Obedience Experiment: The Role of Personality, Situations, and Their Interactions*, 60 J Personality & Soc Psychol 398, 409 (1991). Stanley Milgram pioneered this research with his now-famous study in which test subjects, upon prompting by the test administrator, delivered what they believed were a series of increasingly severe electric shocks to another person. Stanley Milgram, *Behavioral Study of Obedience*, 67 J Abnormal & Soc Psychol 371, 371-78 (1963). Eighty-seven percent of participants continued to deliver shocks even after the other person protested by pounding on the wall, and 65 percent continued on until the very end, beyond the “danger: severe shock” level to “XXX.” *Id.* Subsequent studies in which the “victim” engaged in continuous screaming and pleading or complained

³ That reality is, apparently, often ignored. “A vast scientific literature has established that * * * observers do not reliably appreciate the strength and consequences of situational constraints on an actor’s behavior.” Janice Nadler, *No Need to Shout: Bus Sweeps and the Psychology of Coercion*, 2002 Sup Ct Rev 153, 168-70 (2002).

about a heart condition yielded similarly high rates of obedience. Blass, 60 J Personality & Soc Psychol at 402.⁴ A review of Milgram's obedience studies attributed the high rates of compliance, in part, to the incremental nature of the shock procedure and the fact that the subjects did not choose the situation in which they found themselves. *Id.* at 406. The "strong" situation presented by the experiment, combined with the psychological inhibition caused by its incremental nature, rendered it "virtually impossible" for the subjects "to respond in a detached, uninvolved manner." *Id.*

Milgram's research reveals the pressures at play here. Defendant did not choose to be questioned while in handcuffs in the back of a patrol car; the officer controlled his movements. Moreover, the scenario unfolded incrementally. Officer Robeson stopped defendant's car, asked for his license and registration, asked him to leave his vehicle, searched him, handcuffed him, and placed him in the back of his patrol car. Tr 5, 6, 9. He then had defendant fill out an "FI form," which asked for "name, race, date of birth, physical, driver's license number, employer," and other similar information. Tr 7. Thus, Officer Robeson's question "if there was anything we should be concerned about," Tr 8, must be considered in light of the authority he already had exerted

⁴ Rates of obedience have not changed systematically over time. Thomas Blass, *The Milgram Paradigm After 35 Years: Some Things We Now Know About Obedience to Authority*, 29 J of Applied Soc Psychol 955, 969 (1999).

successfully over the course of the encounter. Similarly, defendant's response to Robeson's question must be considered in light of the fact that defendant already had submitted to Robeson's authority by acceding to the series of commands Robeson gave defendant as the encounter unfolded.

1. Police officers' status as authority figures leads individuals to interpret officers' statements as commands.

Studies demonstrate that the social context of a statement plays an important role in its meaning, particularly when a speaker employs indirect language. "Higher status people frequently direct the actions of others, and hence others expect the remarks of higher status speakers (in the appropriate contexts) to act as directives." Thomas Holtgraves, *Communication in Context: Effects of Speaker Status on the Comprehension of Indirect Requests*, 20 J of Experimental Psychol: Learning, Memory, & Cognition 1205, 1214-15 (1994). For example, in a study that compared listeners' comprehension of indirect requests by a high-status speaker with those of a speaker of equal status, listeners readily understood a remark by a person of higher status as a directive to act. *Id.* at 1214. In another study, subjects perceived a peer's statement "don't be late again" as more coercive than the statement "try not to be late again"; but when an authority figure (such as the subject's boss) made the same statements, there was no difference in perceived coercion. Janice Nadler, *No Need to Shout: Bus Sweeps and the Psychology of Coercion*, 2002 Sup Ct Rev

153, 189 (2002) (citing Jennifer L. Vollbrecht, Michael E. Roloff & Gaylen D. Paulson, *Coercive Potential and Face Threatening Sensitivity: The Effects of Authority and Directives in Social Confrontations*, 8 Intl J Conflict Mgmt 235, 236 (1997)).⁵ Thus, “power relationships dictate that when the police make a ‘request’ and they could apparently compel the suspect to carry out the request, the suspect will view the request as a command.” Peter Tiersma, *The Judge as Linguist*, 27 Loy LA L Rev 269, 282 (1993).

This research is particularly relevant here, because the officer made an indirect statement when he asked defendant “if there was anything we should be concerned about.” Tr 8. Although, at face value, the officer posed a question, in light of the social context, defendant readily could have interpreted that “question” as a directive. Thus, defendant’s response—that the officers could search his car if they wanted to—was an acknowledgement of the officer’s power. Viewed in the context of the social dynamics at work, defendant’s response was far from a freely extended invitation to search his vehicle.

2. The presence of a uniform influences obedience.

Additional studies on situational factors that affect obedience demonstrate that compliance rates increase when the requestor is wearing a

⁵ Another example further illustrates that dynamic. “If an ordinary citizen, taking a tour of the White House, asks a guard standing in front of the door to the Oval Office, ‘May I enter this room?’ it is simply a request. If the President asks, he is ordering the guard to step aside.” Peter Tiersma, *The Judge as Linguist*, 27 Loy LA L Rev 269, 281 (1993).

uniform. In one study, the experimenter (dressed variously as a civilian wearing a sport coat and tie, a milkman, and an unarmed security guard) asked individuals to perform a simple task. Ric Simmons, *Not “Voluntary” But Still Reasonable: A New Paradigm for Understanding the Consent Searches Doctrine*, 80 Ind LJ 773, 808 (2005) (citing Leonard Bickman, *The Social Power of a Uniform*, 4 J Applied Soc Psychol 47 (1974)). Compliance rates were much higher when the experimenter dressed as a security guard. *Id.* Thirty-three percent of the subjects gave a dime to a stranger in response to the civilian, for example, whereas 89 percent complied with the guard. *Id.* Another study—in which the experimenter dressed as a blue-collar worker, a business executive, or a firefighter—demonstrated a similarly high level of compliance when the experimenter wore the firefighter’s uniform relative to when he wore civilian clothes. David K. Kessler, *Free to Leave? An Empirical Look at the Fourth Amendment’s Seizure Standard*, 99 J Crim L & Criminol 51, 63 (2009) (citing Brad J. Bushman, *Perceived Symbols of Authority and Their Influence on Conformity*, 14 J Applied Soc Psychol 501, 502-06 (1984)).

In the context of this case, the officer’s uniform further reinforced a social dynamic in which defendant was significantly more likely to view Officer Robeson’s “question” as a command. The preceding studies accordingly suggest a source other than defendant’s independent consent for defendant’s submission to the officers’ search of his vehicle. That is, the

studies discussed above, despite factual differences, “provide[] a viable explanation” as to why “people follow or obey a ‘request’ made by police officers in authority positions in situations where there is not only no ostensible benefit to do so, there is likely harm.” Mary Strauss, *Reconstructing Consent*, 92 J Crim L & Criminol 211, 239-40 (2002).

3. Research on police interactions confirms that people rarely comply freely with officers’ requests.

Research that has directly examined the reasons why individuals comply with officers’ requests further demonstrates the coercive power that officers wield during encounters with civilians. A study of stop data from Maryland and Ohio revealed that, of the 9,028 motorists whom police asked for consent to search their cars, 89.3 percent granted it. Steven Chanenson, *Get the Facts, Jack! Empirical Research and the Changing Constitutional Landscape of Consent Searches*, 71 Tenn L Rev 399, 452 (2004) (citing Illya D. Lichtenberg, *Voluntary Consent or Obedience to Authority: An Inquiry into the “Consensual” Police-Citizen Encounter* 199 (1999) (unpublished Ph.D. dissertation, Rutgers University)). In a random survey of 54 of the individuals whom police had asked for consent to search, 47 out of the 49 people who “consented” indicated that they did so only out of fear of what consequences would follow if they refused. Nadler, 2002 Sup Ct Rev at 202 (citing Lichtenberg, *Voluntary Consent* at 251, 268). Moreover, when asked whether

they thought the police would have honored a refusal to allow a search, only one respondent answered yes. *Id.* at 203 (citing Lichtenberg, *Voluntary Consent* at 271-72).

Another survey, with a larger response rate, yielded similar results. Over 400 respondents indicated on a scale from one to five—with one being “not free” and five being “completely free”—whether they would feel free to leave or say no to a police officer during an encounter on a sidewalk or on a bus. Kessler, 99 J Crim L & Criminology at 69. Half of the respondents selected one or two, and almost 80 percent selected three (the midpoint) or less on the scale. *Id.* at 75.⁶

Yet another study, based on observations of encounters between experimenters dressed as university security officers and passersby, further confirmed the coercive power an officer can wield. Alisa M. Smith, Erik Dolgoff & Dana Stewart Speer, *Testing Judicial Assumptions of the Consensual Encounter: An Experimental Study*, 14 Fla Coastal L Rev 285, 300 (2013). The security officers asked the test subjects in a normal tone of voice, “Please come here, I’d like to speak with you,” then (if the subject complied), “May I have your name?” then (if the subject complied), “May I see your identification?”

⁶ Women and people under 25 years old reported that they would feel less free to leave than did men and people over the age of 25. Kessler, 99 J Crim L & Criminol at 75.

and then (if the subject complied), “Why are you on campus?” *Id.* at 301.

Every one of the 83 subjects complied completely with every request. *Id.* at 303. “Not a single individual questioned the officers on their authority to approach, stop, question or ask for identification.” *Id.* Moreover, 60 percent of the subjects indicated that they submitted to the inherent authority of the officers, and another 11 percent did so to avoid trouble, conflict, or being chased. *Id.* at 320. Thus, the authors concluded, “Even without physical restraint, force or commands, reasonable people are constrained to comply with authority.” *Id.*

B. Officers’ coercive power is amplified in minority communities.

Although race is not a factor in this case, this court’s decision necessarily will have a disproportionate impact on minority communities because minorities, and in particular African-American men, are routinely targeted by law enforcement. *See* Tracey Maclin, “*Black and Blue Encounters*”—*Some Preliminary Thoughts About Fourth Amendment Seizures: Should Race Matter?*, 26 Val U L Rev 243 (1991) (compiling data). Indeed, a recent nationwide study by the federal Bureau of Justice Statistics found that black drivers are stopped more frequently than white drivers and are more than twice as likely to be searched. *See* Lynn Langton & Matthew Durose, *Special Report: Police Behavior During Traffic and Street Stops, 2011*, Bureau of Justice Statistics 9 (Sept 2013). Oregon reflects the national data; a study of the

Portland Police Bureau stop data reveals that, although African-Americans comprise 6.3 percent of the city's population, 11.8 percent of all traffic stops and 22.1 percent of all pedestrian stops involve African-Americans. *See* Sgt Greg Stewart & Emily Covelli, Portland Police Bureau, *Stops Data Collection: The Portland Police Bureau's Response to the Criminal Justice Policy and Research Institute's Recommendations*, at 11, 15-17, 29 (2014). Consistent with the national data, African-Americans in Oregon are also more likely to be searched by police, and are more likely than white drivers to give consent to search. *Id.* at 15.

Maclin addresses why African-Americans more frequently consent by noting, in response to the assertion that an individual is free to disregard a police officer's requests, that

“[t]his is what the law is supposed to be; black men, however, know that a different ‘law’ exists on the street. Black men know that they are liable to be stopped at anytime, and that when they question the authority of the police, the response from the cops is often swift and violent.”

Maclin, 26 Val U L Rev at 253. Owing to a long and sordid history⁷ of violent encounters between police officers and African-American men, that community in particular feels pressured to cooperate for fear of physical reprisals. *See id.* at 255 (“Black males learn at an early age that confrontations with the police should be avoided; black teenagers are advised never to challenge a police officer, *even when the officer is wrong.*” (Emphasis added)).

Although those concerns are heightened for black men, as the foregoing suggests, *no* citizen is immune from the coercive pressures that are inherent in every police-citizen encounter and which are brought to bear as a result of all the factors discussed above. In deciding the rule of law that will apply to this case and others that follow it, it is critical for this court to recognize the actual coercive forces at work in police-citizen encounters, to understand the imbalance of power in those encounters, and to be vigilant in vindicating individual rights protected by the Oregon Constitution by suppressing evidence when it derives from a constitutional violation. And, although judicial

⁷ That history continues to repeat itself. On August 9, 2014, an unarmed black teenager named Michael Brown was shot in the streets of Ferguson, Missouri, by Police Officer Darren Wilson. Brown was one of five unarmed black men killed by police officers between July and August of 2014. *See* Josh Harkinson, *4 Unarmed Black Men Have Been Killed By Police in the Last Month*, Mother Jones, *available at* <http://www.motherjones.com/politics/2014/08/3-unarmed-black-african-american-men-killed-police> (last accessed Aug 13, 2014) (noting the deaths of Eric Garner, John Crawford, Ezell Ford, and Dante Parker).

intervention and suppression of unlawfully obtained evidence is required in *all* cases, this court also should be mindful that minority communities face even greater pressures in police-citizen encounters, and face those pressures more frequently than others.

II. Suppression is required to vindicate individual rights

As this court recently confirmed in *State v. Unger*, 356 Or 59, 74, ____ P3d ____ (2014), suppression of unlawfully obtained evidence is necessary to vindicate the violation of an individuals' rights under the Oregon Constitution. As defendant ably explains in his Brief on the Merits, evidence that is discovered as a foreseeable result of a *Miranda* violation derives from that violation. And, as explained in the preceding sections, consent does not attenuate a preceding *Miranda* violation; rather, it *results* from it. Oregon law therefore requires suppression of the physical evidence discovered as a foreseeable result of a prior *Miranda* violation. *Vondehn*, 348 Or at 475-76.

It is incumbent on the courts to vindicate individual rights, because, as the research discussed above demonstrates, individuals rarely feel free to vindicate their own rights when confronted by a display of authority. In an ideal world an individual who is, in fact, free to leave will simply walk away. In an ideal world, a suspect, such as Mr. Delong, would already know that he has a right to refuse to answer a police officer's questions and would not need to be informed of that right. But we do not live in an ideal world. The reality is

that individuals do not always understand their rights or do not always feel free to assert those rights even if they do understand them. *See* Daniel J. Steinbock, *The Wrong Line Between Freedom and Restraint: The Unreality, Obscurity, and Incivility of the Fourth Amendment Consensual Encounter Doctrine*, 38 San Diego L Rev 507, 535 (2001) (noting that “far from feeling free to terminate an encounter, the reasonable person, by all indications, submits to the legitimate and coercive authority of the police. He or she is, in brief, on the short end of an asymmetric power relationship.”).⁸

A. In light of police officers’ coercive power, defendant’s statement that officers could search his car was a foreseeable result of the unlawful custodial interrogation.

Defendant in this case faced a far more coercive environment than those in the studies discussed in Section I above. Defendant was subjected to a custodial interrogation while handcuffed in the back of a police car. Officer Robeson’s question regarding the contents of defendant’s car came after an

⁸ Importantly, innocent individuals routinely are subject to police coercion with no judicial oversight, which results in “widespread interference with personal liberty without any objective justification.” Steinbock, 38 San Diego L Rev at 535. Regardless of the flagrancy of an officer’s conduct, individuals still tend to experience most interactions as coercive. In the study of individuals whom police asked for consent to search their car, discussed in Section I.A.3, above, the majority of the individuals reported feeling “violated” and “really bitter” about the experience and continued to think about the experience about once per day. Nadler, 2002 Sup Ct Rev at 211-12. Thus, clear judicial statements regarding the limits of police coercion are necessary guidance for law enforcement and are essential for statewide protection.

incremental series of commands and restraints on defendant's liberty that maximized the coercive power of Officer Robeson's authority, as explained in Section I.A above. Moreover, as explained in Section I.A.1 above, Officer Robeson's status as an authority figure rendered his question about the contents of defendant's car more akin to a command than a request. Additionally, as Section I.A.2 explains, Officer Robeson's uniform bolstered his coercive power. Finally, Officer Robeson never told defendant that he had a constitutional right to refuse to say anything at all to the police officers.

In light of those circumstances, the state's argument that defendant freely chose to allow the officers to search his car is untenable. Empirical evidence confirms that individuals comply with police officers' requests as "the result of submission, rather than consent." Smith, Dolgoff & Speer, 14 Fla Coastal L Rev at 321. This court itself has recognized the inherent level of coercion that exists in custodial interrogations. *Vondehn*, 348 Or at 472. Thus, defendant's statement that the officers could search his car was a foreseeable result of the officers' *Miranda* violation.

B. No intervening circumstances diluted the officer's coercive power over defendant.

Social science research demonstrates that resisting the coercive power of an authority figure is most difficult when one is under pressure and in a face-to-face interaction. People forced to make decisions under pressure fail to

consider all the relevant information and alternatives and tend to rely “on implicit cultural theories and norms.” Nadler, 2002 Sup Ct Rev at 195-96 (citing Chi-yue Chiu et al., *Motivated Cultural Cognition: The Impact of Implicit Cultural Theories on Dispositional Attribution Varies as a Function of Need for Closure*, 78 J Personality & Soc Psychol 247, 255-56 (2000)).⁹

Moreover, even small stressors, such as the presence of another person in the room, can trigger physiological responses that make people to feel threatened and compromise their ability to reason. *Id.* at 195 (citing Jim Blascovich & Joe Tomaka, *The Biopsychosocial Model of Arousal Regulation*, 28 Advances in Experimental Soc Psychol 1, 23-24 (1996).

The physical proximity of an authority figure also has an impact on the degree of coercive power he or she wields. In the Milgram experiments, the test administrator’s physical proximity to the subjects had “a pronounced effect.” Blass, 60 J Personality & Soc Psychol at 399. Only 23 percent of participants were fully obedient when the experimenter left the laboratory and

⁹ As an example, Nadler points to a local police department’s effort to curb underage drinking. 2002 Sup Ct Rev at 193-194. Police sent out forms to 2,700 households asking for homeowners’ consent to allow police to search their home if the police received a report of underage drinking. *Id.* (citing Robert Hanley, *An Anti-Drinking Campaign and How It Flopped: Police Want to Break Up Teen-Agers’ Beer Parties, but Parents Won’t Let Them In*, NY Times, Sept 28, 1994, at B1). Only 20 forms were signed and returned. *Id.* at 194. This stands in stark contrast to the high rates of compliance with police in face-to-face encounters.

gave orders over the phone, whereas 65 percent of the subjects were fully obedient in the original study, in which the administrator remained physically present. *Id.* Other studies have found that “people feel more pressure to comply with a request when the requester speaks to them from a close physical distance.” Nadler, 2002 Sup Ct Rev at 190-91 (citing Chris Segrin, *The Influence of Nonverbal Behaviors in Compliance-Gaining Processes*, in *The Nonverbal Communication Reader: Classic and Contemporary Readings* (Laura K. Guerrero, Joseph A. DeVito & Michael L. Hecht, eds.) (1990)).

Here, defendant made the statement that the officers could search his car if they wanted to while under time pressure and in close proximity to Officer Robeson. Defendant was still handcuffed in the back of the patrol car, with Officer Robeson speaking to him from the front. Tr 8, 28.¹⁰ Officer Robeson posed a question to defendant—whether there was anything in the car to be concerned about—that demanded an immediate response.

Thus, when defendant made the statement, he still faced the compelling atmosphere created by the *Miranda* violation. He remained in custody, and no time elapsed between the violation and his statement. No subsequent events diluted the coercive nature of the encounter. As a result, defendants’ statement,

¹⁰ Officer Robeson testified that, at the time, he was checking on the in-car computer. Tr 8. Officer Poe testified that Officer Robeson was sitting in his car while speaking to defendant. Tr 28.

and the resulting discovery of physical evidence, derived from the *Miranda* violation.

CONCLUSION

For the foregoing reasons, this court should adopt defendant's proposed rules and affirm the Court of Appeals.

Respectfully submitted,

/s/ Shauna M. Curphey

Shauna M. Curphey, OSB #063063

Sara F. Werboff, OSB #105388

Jordan R. Silk, OSB #105031

*Attorneys for Amici Curiae Oregon
Justice Resource Center, Albina
Ministerial Alliance Coalition for Justice
and Police Reform, The Portland
Chapter of the National Lawyers Guild,
Inc., and the American Civil Liberties
Union Foundation of Oregon, Inc.*

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WITH BRIEF LENGTH AND TYPE SIZE REQUIREMENTS**

Brief length

I certify that (1) this brief on the merits complies with the word-count limitation in ORAP 5.05(2)(b)(i) and (2) the word count of this brief (as described in ORAP 5.05(2)(a)) is 5,033 words.

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I certify that the size of the type in this brief is not smaller than 14 point for both the text of the brief and footnotes as required by ORAP 5.05(4)(f).

/s/ Sara F. Werboff

Sara F. Werboff, OSB No. 105388

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Understanding Behavior in the Milgram Obedience Experiment: The Role of Personality, Situations, and Their Interactions

Thomas Blass
University of Maryland Baltimore County

Among the far-reaching implications that have been drawn from Milgram's obedience research is that situations powerfully override personal dispositions as determinants of social behavior. A focused review of the relevant research on the Milgram paradigm reveals that the evidence on situational determinants of obedience is less clear than is generally recognized; contrary to the commonly held view, personality measures *can* predict obedience; another kind of dispositional variable, enduring beliefs, is also implicated in the obedience process; and approaches suggested by interactionist perspectives can provide some integration of the literature. The article concludes with a discussion of the broader inferences about obedience and social behavior called for by this review and the enduring significance of Milgram's obedience research.

It is now 30 years since Milgram first began his series of experiments to study the dynamics of obedience to authority (Milgram, 1963, 1964a, 1964c, 1965a, 1965b, 1965c, 1967, 1974). Despite the passage of time, their position of prominence in psychology has not faded, as citation counts (e.g., Institute for Scientific Information, 1981; Kasmer, Haugtvedt, & Steidley, 1988; Perlman, 1984), peer opinion (Diamond & Morton, 1978), or even an informal perusal of recent introductory-level texts will reveal.

The continuing salience of the obedience work can be attributed to its many distinctive features. First, of course, is the unexpected enormity of the basic findings themselves—that 65% of a sample of average American adult men were willing to punish another person with increasingly higher voltages of electric shock all the way to the maximum (450 volts) when ordered to by an experimenter who did not possess any coercive powers to enforce his commands (Milgram, 1963). When asked to predict the outcome of the obedience experiment, neither a group of Yale seniors (Milgram, 1963) nor a group of psychiatrists (Milgram, 1965c) were even remotely close to predicting the actual result: Their predicted obedience rates were 1.2% and .125%, respectively.

Second, Milgram's obedience studies are distinctive because they represent one of the largest integrated research programs in social psychology: Milgram conducted at least 21 variations of his basic experimental paradigm (see Milgram, 1974, p. 207).

Third, very few works can match the obedience studies in the

fervor with which they have been debated. Over the years, the obedience research has been a target of both criticism (e.g., Baumrind, 1964; Bettelheim, cited in Askenasy, 1978; Kelman, 1967; Masserman, 1968; Mixon, 1971; Orne & Holland, 1968; Warwick, 1982; Wrightsman, 1974) and praise (e.g., Askenasy, 1978; Brown, 1986; Crawford, 1972; Elms, 1972, 1982; Etzioni, 1968; Kaufmann, 1967; A. G. Miller, 1986; Ring, 1967; Ross, 1988; Zimbardo, 1974). More than any other research in social psychology, the obedience experiments have been embroiled from the beginning in a number of controversies in which they have played a central and enriching role. These include the ethics of research (e.g., Abse, 1973; Baumrind, 1964; Bickman & Zarantonello, 1978; Elms, 1982; Errera, 1972; Harris, 1988; Holmes, 1976; Kelman, 1967; Milgram, 1964b, 1973, 1974, 1977b; Ring, Wallston, & Corey, 1970; Schlenker & Forsyth, 1977; Sieber, 1984; Warwick, 1982), the social psychology of the psychological experiment (Holland, 1967; Milgram, 1968, 1972; Orne & Holland, 1968), and the deception versus role-playing controversy (Baumrind, 1964; Cooper, 1976; Forward, Canter, & Kirsch, 1976; Freedman, 1969; Geller, 1982; Ginsburg, 1979; Greenwood, 1983; Hendrick, 1977; A. G. Miller, 1972; Mixon, 1971). With regard to the latter, it is especially noteworthy that the strongest evidence in favor of role-playing as an alternative to the deception experiment comes from three role-playing versions of the obedience experiments that have found levels of obedience comparable to the originals (Geller, 1975, 1978; Mixon, 1971; O'Leary, Willis, & Tomich, 1970). An insightful examination of the obedience research emphasizing the controversies surrounding it can be found in A. G. Miller (1986).

Fourth, Milgram's obedience research is unusual in its relevance to disciplines outside of psychology. It has been discussed in publications devoted to topics as wide ranging as communication research (Eckman, 1977), philosophy (Patten, 1977), political science (Helm & Morelli, 1979), psychiatry (Erickson, 1968), education (Hamachek, 1976), and Holocaust studies (Berger, 1983; Sabini & Silver, 1980), and has even appeared in books of readings of English prose (Comley, Hamilton, Klaus, Scholes, & Sommers, 1984; Eastman et al., 1988).

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I would like to thank the following individuals for their painstaking work in translating the foreign-language journal articles cited in this article: Rosy Bodenheimer and Aron Siegman (German) and Pat Chiriboga (Spanish). Thanks also to Douglas Teti and Lisa Freund for their valuable assistance with data analysis.

Correspondence concerning this article should be addressed to Thomas Blass, Department of Psychology, University of Maryland Baltimore County, 5401 Wilkens Avenue, Catonsville, Maryland 21228.

Finally, the breadth and durability of interest in the obedience research is due, no doubt, to the fundamental and far-reaching implication about human nature that has been drawn from it—the apparent power of situational determinants to override personal dispositions. But whether or not broad lessons about the primacy of situational determination can be drawn from the obedience research hinges on a clearer understanding of just what has and has not been demonstrated in the Milgram-type experiment itself and how to best account for it. The goal of this article is to contribute to that understanding. Specifically, I will draw on the accumulated research on the obedience paradigm with a focus on the findings that bear most directly on the broad extrapolations about situational versus dispositional influences on social behavior that have been made from it.

First, I will review the evidence on situational determinants of obedience. The obedience experiments are widely regarded as among the prime examples of how behavior is powerfully responsive to situational variations. Yet, a survey of the relevant research and a closer look at Milgram's own studies will reveal that a more modest and differentiated perspective on the matter is called for. Second, I will review the evidence on personality correlates of obedience. As the flip side of the usual situational emphasis given to the obedience experiments, the role of personality has typically been given short shrift in discussions of the research. As will be seen, the evidence suggests that personality variables can predict obedience. However, some of the findings are either contradictory or weak and the evidence for theoretically dictated personality-obedience relationships is mixed. Third, I will examine the role of another type of dispositional variable—enduring beliefs. Specifically, I will show that enduring beliefs about ceding versus retaining personal control seem to be salient and predisposing factors in obedience to authority. Fourth, I will examine the contribution of a person by situation interactional approach toward understanding obedience. The primary value of interactionism is not in the number of interactional studies of obedience promoted—which turn out to be few. Rather, I will argue, it is in the identification of potential situational and dispositional moderators that can enhance the prediction of obedience to authority. I will conclude with a discussion of the broader implications for understanding obedience and social behavior called for by my analysis and the enduring significance of Milgram's obedience research.

Situational Determinants of Obedience

The obedience work has had a special appeal among social psychologists because of its congruence with and influence on the dominant approach (at least, until recently) in social psychology—the preference for looking at features of the immediate situation, rather than the characteristics the person brings into it, for causal explanations of behavior (see Blass, 1977a, 1984b). Over the years, the findings of the obedience studies have been held up as examples, par excellence, of the controlling power of the situation (e.g., Gaertner, 1976; Ross, 1977, 1988; Shaver, 1985; Zimbardo, 1974; but see also Sabini & Silver, 1983). For example, Helmreich, Bakeman, and Scherwitz (1973) stated:

The upset generated by a Milgram or a Zimbardo . . . in part stems from ethical concerns. But another part of their power lies precisely in their demonstration of how strong situational determinants are in shaping behavior. No resort to a correlation between "those" people who do "evil" things is allowed: the subjects were randomly assigned. (p. 343)

Actually, it is no surprise that the "message" of situational determination is so often drawn from the obedience studies, because Milgram himself emphasized such a perspective on his research. Thus, for example, in his final article dealing with obedience, Milgram (1984; also in Milgram, 1987) stated that "the crux of Milgram's inquiry is a set of experimental variations which examine the variables which increase or diminish obedience" (p. 446), echoing similar statements in his earlier writings (e.g., Milgram, 1964c, p. 9; 1965c, p. 60; 1974, p. 26). One of the strongest statements in this regard comes toward the end of Milgram's (1974) book:

The disposition a person brings to the experiment is probably less important a cause of his behavior than most readers assume. For the social psychology of this century reveals a major lesson: often, it is not so much the kind of person a man is as the kind of situation in which he finds himself that determines how he will act. (p. 205)

It should be noted, however, that in emphasizing situational determinants Milgram did not question the validity of personality traits as had some of the situationists early on in the history of the trait-situation debate (see Blass, 1977a, 1984a). In fact, in the paragraph preceding the above quote, Milgram (1974), after noting that he found only weak or inconsistent evidence concerning individual-difference correlates of obedience, stated: "I am certain that there is a complex personality basis to obedience and disobedience. But I know we have not found it" (p. 205).

Given the widespread agreement that the obedience experiments represent a powerful demonstration of situational influences, it makes sense to ask just how correct that consensus is from the vantage point of over 25 years of accumulated research on the Milgram obedience paradigm.

There is no question that modifications in the physical and social arrangements in the setting of the obedience experiment can have powerful effects. Thus, for example, Milgram found that when two confederates playing the role of subjects refused to continue partway into the shock series, the vast majority of subjects followed suit, with only 4 out of 40 giving the highest shock (Milgram, 1965a; 1974, Experiment 17, pp. 116–121). Closeness of the authority to the subject also had a pronounced effect. When the experimenter left the laboratory after the start of the experiment and then gave his orders over the phone, there was a significant drop in obedience. Only 9 out of 40 subjects, as opposed to 26 out of 40 in the comparison baseline condition, were fully obedient (Milgram, 1965c, 1974, Experiment 7, pp. 59–62). In every study that has compared a self-decision condition (i.e., where on each trial the subject can choose whether or not to shock and/or what shock level to give) with the more standard condition in which the subject is required to give the next higher voltage level on each subsequent trial, the self-decision condition finds a significant drop in the amount of punishment administered (Bock, 1972; Milgram, 1974, Experiment

11; Kilham & Mann, 1974; Mantell, 1971; Shalala, 1974; Shanab & Yahya, 1977, 1978). When certain incongruities in social structure are introduced into the obedience experiment, the amount of shocks subjects are willing to give is greatly diminished. Thus, not a single subject gave the 450-volt shock (a) when the experimenter called a halt to the experiment but the victim wanted to continue, (b) when the authority took the role of the victim and then wanted the shocks to stop, or (c) when one experimenter ordered a halt to the proceedings and another experimenter commanded the subject to continue (Milgram, 1974, Experiments 12, 14, and 15).

Yet, with a number of other experimentally manipulated variables, the evidence is either contradictory or inconsistent with the demonstrated effects of these variables in other related behavioral domains. Milgram (1974, Experiment 13) found that when another "subject" assumes authority in the absence of the experimenter, subjects are significantly less obedient (only 4 of 20 administered the maximum shock), presumably because a peer's commands do not carry the same force and legitimacy as those of the higher-status experimenter. The findings of Shalala (1974), in an obedience experiment with low-ranking military personnel at Fort Knox as subjects, support these results. Shalala found that when a peer (a private) served as the experimenter rather than a lieutenant colonel, there was a significant drop in obedience to the order to shock the learner. Yet, in two experiments in which the experimenter's authority was "delegitimized," his ability to command obedience still remained substantial. Both Rosenhan (1969) in the United States and Mantell (1971) in West Germany conducted obedience experiments that contained a condition in which the experimenter is discovered to be merely an undergraduate working without professional supervision. The findings were very similar to each other. In Rosenhan's experiment, 53% of the subjects gave the maximum shock, whereas 52% of Mantell's subjects did so. In both experiments, 85% of the subjects in the baseline condition were fully obedient, a significantly higher rate than the 53% and 52% rates found in the "delegitimization" conditions in the two experiments. Yet these latter figures still represent a majority of subjects obeying the experimenter, and these figures are not significantly lower than those found by Milgram in the condition comparable to Mantell and Rosenhan's baseline conditions (i.e., 62.5%, the voice-feedback condition, Milgram, 1965c; 1974, Experiment 2, p. 35).

Both common sense and evidence from studies on aggression (e.g., Baron, 1971, 1973; Rogers, 1980) suggest that under certain conditions the possibility of future retaliation by the recipient of electric shock should reduce the amount of punishment the subject would administer. The only study using the Milgram obedience paradigm to examine the role of retaliation was a doctoral dissertation by Costanzo (1976). Subjects in her retaliation condition were told that after the completion of the first session, they would switch roles with the victim. Hence, presumably, these subjects anticipated retaliation. For subjects in the no-retaliation condition, this information was omitted from the instructions. Anticipated retaliation had no effect whatsoever on obedience; overall, 81% of the subjects obeyed the order to give the maximum shock.

Another example of an experimental variable not showing effects in the obedience experiment, though one might expect

them on the basis of findings in other behavioral domains, comes from obedience studies in which the subject gets to observe a model before his or her own turn to participate. The imitative effects of models have been demonstrated with both negative (e.g., Geen, 1978) and positive (e.g., Rushton, 1979) forms of social behavior. Yet, an obedient model does not seem to add to the authority's power to elicit obedience. The previously mentioned study by Rosenhan (1969) contained a condition in which the subject first watched an obedient, though protesting, model continue to 450 volts. The rate of obedience in this condition was 88%, a trivial increase over the rate of 85% in the baseline, standard condition. In another condition, a disobedient, "humane" model stopped after 210 volts, telling the experimenter that he had to discontinue because the learner was in too much pain. Here the model's influence was more discernible: The obedience rate of the observing subjects was only 58%. The difference between this rate and the 85% obedience rate in the baseline condition approaches significance, $\chi^2 = (1, N = 39) = 3.54, p = .06$, by my analysis. (All subsequent data analyses of Milgram's findings reported in this article are also mine.) Powers and Geen (1972) also found that an obedient model had a less pronounced effect on a subject's level of obedience than a disobedient one. The strongest evidence against the facilitative effects of an obedient model comes from an experiment conducted with Australian college students by Kilham and Mann (1974). Their focus was on comparing obedience in subjects when they merely had to transmit the experimenter's orders versus when they played the standard role of having to shock the victim (executants). When a subject was in the transmitter condition, a confederate played the role of executant. When the executant was a real subject, a confederate played the role of a transmitter. The latter was, in essence, an obedient model. Despite having this feature of modeled obedience, this experiment yielded the lowest obedience rate reported in the literature for a standard condition—28%. It should be noted that although the lack of an effect of an obedient model in the Rosenhan (1969) study might have been due to a ceiling effect, that possibility is clearly not applicable to the Kilham and Mann results.

We have looked at a number of situational determinants whose role in influencing obedience has been studied. The evidence concerning these effects is, as has been shown, mixed and certainly not as uniformly pervasive as the widespread and consensual situational emphasis given the obedience studies in the literature would suggest.

Our survey of various situational factors has taken us, in some instances, to variants of the obedience experiments conducted by researchers other than Milgram. But the data that are among the most persuasive in raising doubts about the all-powerful role claimed for situational effects comes from among the earliest and most central findings reported by Milgram, the four-part proximity series (Milgram, 1965c; 1974, Experiments 1-4, pp. 32-43). In this set of experiments, Milgram tried to vary the degree of salience of the victim to the subject. The first condition was the *remote* condition—the first obedience study reported by Milgram (1963)—in which the subject received only minimal feedback from the learner, who was situated in an adjacent room. This feedback was in the form of pounding on the wall following the 300 and 315 voltage shocks. The second

condition, the *voice-feedback* condition, introduced tape-recorded vocal protests that increased in intensity as the shocks increased in voltage. The third condition, the *proximity* condition, reduced the psychological and physical distance between teacher and learner even further by seating the learner within a few feet of the teacher. Now, the learner was not only audible but also visible to the subject. The final condition, in which the subject's involvement in punishing the learner was most direct and unambiguous, was the *touch-proximity* condition. Here, the victim received "shocks" only when he placed his hand on a shock plate. After 150 volts, he refused to do so, and the subject had to force the learner's hand down onto the shock plate in order for him to get the punishment. As the victim was made increasingly salient to the subject, obedience dropped. Sixty-five percent of subjects gave the maximum shock in the remote condition, 62.5% in the voice-feedback condition, 40% in the proximity condition, and only 30% in the touch-proximity condition. Milgram described these results as follows: "Obedience was significantly reduced as the victim was rendered more immediate to the subject" (Milgram, 1965c, p. 62; 1974, pp. 34-36). Milgram did not supply any results of data analyses to accompany this statement. My own analysis yields $\chi^2(3, N = 160) = 14.08, p < .01$, for the overall effect across all four conditions.

However, closer scrutiny of condition-by-condition differences reveals a puzzling set of results. The first one, not even requiring a test of significance, is the fact that the remote and voice-feedback conditions yielded almost identical rates of obedience. In the remote condition, 26 subjects out of 40 administered the maximum shock, whereas 25 of 40 did so in the voice-feedback condition. This occurred even though, in the voice-feedback condition, the evidence of the learner's suffering is much more prolonged, pronounced, and unambiguous and therefore much harder to put out of mind than in the remote condition. Specifically, the voice-feedback condition consisted of the introduction of vocal complaints from the learner beginning after the 75-volt shock was administered and continuing with rising intensity and urgency. For example, after receiving the 180-volt shock, the learner cried "I can't stand the pain" and at 270 volts, his response was described by Milgram as "definitely an agonized scream" (Milgram, 1974, p. 23). In the remote condition, by contrast, the voice of the victim was not heard at all, the only complaint taking the form of banging on the wall on two occasions—after the 300- and 315-volt shocks were administered.

Also not significant was the difference in obedience rates between the proximity and touch-proximity conditions, the third and fourth experimental variations in the four-part proximity series. In the proximity condition, 16 of 40 subjects were fully obedient, whereas the obedience rate was 12 of 40 in the touch-proximity condition, $\chi^2(1, N = 80) = .879$. Again, the small decrease in amount of obedience does not seem to be commensurate with the amount of increased involvement in the punishment of the victim. In the proximity condition, the teacher and learner were seated near each other; in the touch-proximity condition, after 150 volts, the teacher was in physical contact with the learner, having to force the latter's hand onto the shock plate in order to administer the shocks. Milgram described an experimental session in this condition as follows:

"The scene is brutal and depressing: [the subject's] hard, impassive face showing total indifference as he subdues the screaming learner and gives him shocks" (1974, p. 46). Altogether in the four-part proximity series, the following differences in obedience rates are significant: remote versus proximity condition—26 out of 40 versus 16 out of 40, $\chi^2(1, N = 80) = 5.01, p < .05$; remote versus touch-proximity condition—26 out of 40 versus 12 out of 40, $\chi^2(1, N = 80) = 9.82, p < .01$; voice-feedback versus proximity condition—25 out of 40 versus 16 out of 40, $\chi^2(1, N = 80) = 4.05, p < .05$; and voice-feedback versus touch-proximity condition—25 out of 40 versus 12 out of 40, $\chi^2(1, N = 80) = 8.50, p < .01$. It was also possible to conduct a further analysis, using maximum shock levels administered as the dependent measure, because Milgram (1974) provided a frequency distribution of break-off points for each of the conditions. Table 2 in Milgram (1974, p. 35) shows a continuous drop in subjects' break-off points as one goes from the remote condition ($M = 27.00$) through the voice-feedback ($M = 24.53$) and proximity ($M = 20.80$) conditions to the touch-proximity condition ($M = 17.88$). A one-way between-groups analysis of variance reveals that the overall effect across the four conditions is highly significant, $F(3, 156) = 11.09, p < .0001$. A follow-up test of between-condition differences, using the Newman-Keuls procedure, yields exactly the same pattern of results as was found for the obedience-rate scores; that is, the differences between the remote and voice-feedback conditions and between the proximity and touch-proximity conditions were not significant, and all others were.

Furthermore, the reliability of one of the significant effects within the proximity series can be questioned. Miranda, Caballero, Gomez, and Zamorano (1981) carried out an obedience experiment in Spain that was modeled closely on Milgram's procedures (e.g., they inscribed the same fictitious manufacturer's name on their "shock generator" as Milgram had used on his machine). These researchers did not find any difference in level of obedience between a voice-feedback and a proximity condition, contrary to Milgram's findings. It should be noted, however, that the small number of subjects used (24 altogether), ceiling effects, and possible cultural differences in responsiveness to situational cues could have all operated against obtaining an effect.¹

The question of reliability aside, does the pattern of results in the proximity series make sense? Milgram suggests a number of mechanisms that might account for the effects of changes in visibility and proximity of the victim to the subject (e.g., empathic cues, denial, and narrowing of the cognitive field) (see Milgram, 1965c, pp. 63-65; 1974, pp. 36-40). But why variations in amount and intensity of feedback (Experiment 1 vs. 2) or absence versus presence of physical contact (Experiment 3 vs. 4) did not also have effects still remains a puzzle.

There are additional findings of Milgram that are also problematic for the contention that situational factors are the preeminent determinants of obedience to authority—those of Experiment 5, the new baseline condition (Milgram, 1974, pp. 55-57, 60; also reported earlier in Milgram, 1965a). Experiment 5 was

¹ The latter factor is considered in more detail later in the section on interactionism.

similar to Experiment 2, the voice-feedback condition, with the addition of information indicating that the victim had had a heart condition. (There was also concurrently a change in location from a fancy laboratory to a more modest one.) The victim first revealed the heart problem while he was being strapped into the "electric chair." Then, the victim made further mention of it at three different voltage levels. For example, after "receiving" 330 volts, he shouted "Let me out of here. Let me out of here. My heart's bothering me. Let me out, I tell you . . ." (see Milgram, 1974, pp. 55-57). Logically, one would expect a victim with a heart condition to be perceived as being at greater risk than a victim who, though also protesting vehemently, does not mention a heart problem. Thus, the stimulus situations are clearly different in the voice-feedback and the new baseline conditions and yet the rates of obedience are very similar—62.5% and 65%, respectively. To sum up, the kind of findings just reviewed lead to the following question. Beyond the revelatory nature of situational obedience effects—that actions that were thought to be inflexibly rooted in one's conscience are more malleable than expected—one can ask: How much about the situational determinants of obedience has been demonstrated in an orderly way? Just how far has our knowledge of situational determinants been advanced when two knocks on the wall (Experiment 1), continuous screaming and pleading (Experiment 2), and the addition of complaints about the heart (Experiment 5) by the victim all yield similarly high obedience rates (62.5%–65%)? When the heart-complaint condition conducted by a new experimenter (Experiment 6) yields only a 50% obedience rate, which is the same order of magnitude as the Bridgeport replication (Experiment 10; 47.5%)? When making the victim visible and seating him close to the subject significantly reduces obedience (Experiment 2 vs. Experiment 3; 62.5% vs. 40%) but the introduction of the requirement of using direct, physical force does not cause any further substantive lowering of obedience (Experiment 4; 30%)?

In order to understand such a pattern of results and to be able to generalize from them, one has to be able to specify the underlying features of the situation that do and do not lead to changes in rates of obedience and to conceptualize them in terms of more molar constructs. If one cannot do this, not only will one be unable to use these findings to predict obedience in other settings, but an adequate explanation of the findings themselves—other than in terms of some idiosyncratic details of each experimental variation (e.g., elegant vs. more functional laboratory at Yale; a "dry, hard, technical-looking" vs. a "soft and unaggressive" experimenter; Milgram, 1974, Experiment 6, p. 58)—will remain elusive.

Role of Personality in Obedience

Although many studies of obedience, following Milgram's lead, have focused primarily on situational factors, there are a number of obedience studies that have incorporated personality variables either as the main focus of the research or in addition to an experimentally manipulated variable.

Before examining the findings of these studies, however, it is necessary to ask what the basis is for expecting personality or other stable dispositional variables to be predictive of obedience. The answer is straightforward: In most cases where

there are significant situational effects on obedience, the behavior of all subjects is still not accounted for. That is, even when a significantly higher proportion of subjects are obedient in experimental condition A than B, there are typically subjects who deviate from the overall pattern (i.e., subjects in condition A who are disobedient and ones in B who are obedient). In other words, that there are individual differences in obedience is a fact because in most obedience studies, given the same stimulus situation, one finds both obedience and disobedience taking place. Thus, attempts at finding personality correlates of obedience-disobedience can be seen as efforts aimed at "capturing" individual differences in reactions to authority within a systematic framework or construct. As I will show, individual differences in obedience have sometimes yielded to and other times eluded capture by measured personality variables.

One would not expect a personality measure that has only a tenuous theoretical relationship to the target behavior to be an effective predictor of that behavior (Blass, 1977a). Thus, it is no surprise that Eysenck's measure of introversion-extraversion was not found to be related to obedience in a previously mentioned experiment modeled closely on Milgram's procedures conducted in Spain (Miranda et al., 1981), because relationships to authority are not a salient feature of the construct (see Wilson, 1977, pp. 180-181).

A personality variable that is quite clearly theoretically relevant to obedience to authority is authoritarianism, a personality syndrome seen by its authors as made up of nine interrelated variables, one of which is *authoritarian submission*, defined as a "submissive, uncritical attitude toward idealized moral authorities of the ingroup" (Adorno, Frenkel-Brunswick, Levinson, & Sanford, 1950, p. 228). Thus, quite appropriately, the first published study that examined the relationship between personality and obedience in the Milgram paradigm (Elms & Milgram, 1966; see also Elms, 1972) found obedient subjects to be significantly more authoritarian on the F-Scale than disobedient subjects. Participants in that study were 40 subjects from among the 160 participants in the four-part proximity series who had gone "against the tide" of situational pressures: 20 were drawn from among those who had been defiant in the remote or voice-feedback conditions and another 20 came from the group of subjects who had been obedient in the proximity or touch-proximity conditions. The two groups did not differ significantly on the standard MMPI scales, but defiant subjects scored significantly higher on a social responsibility scale derived from the MMPI.

More recently, a dissertation by F. D. Miller (1975) studied obedience to an order for self-inflicted pain—that the subject should grasp some live electrical wires for 5 min while working on arithmetic problems. There was a small but significant correlation between subjects' degree of authoritarianism, as measured by a 10-item version of the F-Scale, and obeying orders to shock oneself, with the more authoritarian subjects being more obedient. This relationship was apparently quite tenuous, as it washed out when a dichotomous rather than a three-step measure of obedience was used. And, finally, the work of Altemeyer (1981, 1988) on his construct of right-wing authoritarianism (RWA) has relevance for the authoritarianism-obedience relationship. Altemeyer's RWA scale incorporated a reconceptualization of authoritarianism—it consists of only the three attitudinal clusters of authoritarian submission,

authoritarian aggression, and conventionalism, rather than the nine dimensions of the original F-Scale—as well as psychometric refinements (i.e., balanced item wording and high interitem correlations). Altemeyer found that scores on the RWA scale correlated significantly with the average intensity of shock administered in a self-decision verbal-learning task (1981, pp. 200–202). Next to the shock box was another one that had a large red push button on it. A warning above it read: “Danger. Very severe shock. Do not push this button unless you are instructed to do so.” When the experiment was over, the experimenter ordered the subject to push the red button “to administer an extra strong shock as punishment for not trying” (Altemeyer, 1981, p. 273). Here, subjects’ level of authoritarianism became irrelevant because the vast majority of subjects complied—obedience rates ranged from 86% to 91% across four studies.

Another personality variable, besides authoritarianism, that has potential theoretical relevance for obedience to authority is Rotter’s construct of interpersonal trust (Rotter, 1971). Trust is a personality variable that should be relevant to behavior in the obedience experiment according to two theoretical perspectives differing from Milgram’s, those of Mixon (1971, 1972, 1976, 1979) and Orne and Holland (Holland, 1967; Orne & Holland, 1968).

Mixon argues that if subjects were sure that the “learner” was being harmed, virtually everybody would be disobedient. Subjects in a scientific study have every reason to expect that safeguards have been taken to protect participants from harm, and they trust the experimenter when he gives the assurance that “Although the shocks can be extremely painful, they cause no permanent tissue damage.” Thus, according to Mixon, the assumption that nothing of a harmful nature could take place in a scientific experiment leads obedient subjects to see themselves as inflicting painful shocks but not permanent injury on the “learner.” However, they do not question that the shocks are genuine (Mixon, 1976, 1979). For Orne and Holland (Holland, 1967; Orne & Holland, 1968), however, trust that safety precautions have been taken together with the “demand characteristics” of the experimental setting—a cool, unperturbed experimenter urging the subject on despite the victim’s protests—tip the subject off that the shocks are not real.

Thus, trust in the benign purposes of the experimenter is the key to understanding the obedient subjects’ behavior, according to both Mixon and Orne and Holland, although they disagree about its assumed consequences: For Mixon, subjects’ trustfulness leads them to believe that the shocks are painful but not harmful; for Orne and Holland, that they are not real at all. According to their perspectives, one would therefore expect that the more trusting people are, the higher should be their level of obedience. The evidence, however, provides only mixed support for this conjecture. Holland (1967) found no relationship between trust, as measured by Rotter’s Interpersonal Trust Scale, and amount of obedience. (Holland also administered the Crowne and Marlowe [1960] Social Desirability Scale but it, too, failed to relate significantly to obedience.) On the other hand, F. D. Miller (1975) found that more trusting subjects—as measured by his Attitudes Toward Experiments and Experimenters scale—were also significantly more likely to follow instructions to receive electric shock than less trusting subjects.

Another individual-difference dimension that has been studied—and shown to have some relationship to obedience—is level of moral judgment as conceptualized by Kohlberg’s cognitive-developmental theory. Milgram (1974) reported that among 34 Yale undergraduates who had participated in his pilot studies, Kohlberg found that those who defied the experimenter were at a higher stage of moral development than those who were obedient to his orders. Milgram described these findings as “suggestive, though not very strong” (Milgram, 1974, p. 205; see also Kohlberg, 1969, and Kohlberg & Candee, 1984).

A cognitive correlate of obedience of a different sort was identified by Burley and McGuinness (1977), namely, social intelligence. According to these authors, a person with a high degree of social intelligence “may develop a clearer perception of the situation utilizing the situational cues to guide his behavior.” It also involves the ability to “effectively . . . influence the outcomes of situations either via the generation of a variety of output or by the generation of the one correct solution” (Burley & McGuinness, 1977, pp. 767–768). They found that subjects who did not comply with the experimenter’s commands to give increasingly more intense shocks on a 15-step shock generator scored significantly higher on a measure of social intelligence than those who did comply. Although the study does open up the possibility that individual differences on an ability or skill dimension might mediate obedience to authority, it is weakened by the fact that the social intelligence test used dates from 1927, and thus is unlikely to be up to contemporary psychometric standards.

Haas (1966) provided evidence that individual differences in hostility can account for variations in obedience. In his study, a group of lower-level company management staff were ordered by top management to critically evaluate their superiors and to specifically indicate which of them they felt should be fired. It was stressed that their recommendations would serve as “the final basis for action.” The participants’ recommendations were classified into six categories representing different degrees of obedience to the destructive orders of management. These ranged from refusal to participate to full obedience represented by a recommendation to fire. Haas (1966) found a significant positive correlation ($r = .52, p = .01$) between the managers’ degree of obedience and their hostility, as measured by the Siegel (1956) Manifest Hostility Scale, a measure composed mostly of items from the MMPI. Altogether, only 6 of 44 persons (13.6%) obeyed the order to recommend dismissals, and 9 (20.5%) refused to participate altogether.

Role of Beliefs in Obedience

There is another group of dispositional variables—besides the personality measures just reviewed—that have demonstrated a significant relationship to obedience to authority. These are measures tapping presumably stable beliefs about the determinants of one’s lot in life. There are four relevant studies, with three of them pointing to a link between these kinds of beliefs and obedience to authority. Two of them involved Rotter’s (1966) internal versus external control (I-E) dimension and the third used measures of religious orientation.

One of the studies using the I-E scale was F. D. Miller’s (1975) doctoral dissertation. In that study, subjects were required by

the experimenter to shock themselves by grasping live electrical wires and to collate booklets and address labels. One experimental variable was the experimenter's apparent social status within the psychology department (high vs. low bureaucratic authority). Miller found that a composite measure of compliance with these three requests varied as an interactive effect of the authority's bureaucratic status and subject's I-E score. Externals obeyed more in a high than a low bureaucratic authority condition, but internals were unaffected by the manipulation of the experimenter's status.

The other study that found interactive effects involving I-E was Holland's (1967) dissertation. In an attempt to apply Orne's demand characteristics perspective to the obedience experiment, Holland (1967) created three experimental conditions. Condition 1 was a methodological replication of Milgram's voice-feedback condition. In Condition 2, subjects were told by one experimenter that they were not regular, naive subjects but rather controls who were "to watch carefully what goes on, what happens to you and what is said to you, so that you can figure out what this experiment is really all about" (p. 101). They were, however, to act as real subjects would so that the second experimenter would not be able to tell that they were just playing the role of naive subjects. In Condition 3, subjects were told that the learner would actually be receiving only one tenth of the voltages indicated by the shock labels and to hide this knowledge from the second experimenter and feign being regular subjects. Holland (1967) reported that although Condition 2 yielded a somewhat lower rate of obedience than the other conditions, the three conditions did not differ significantly among themselves and from Milgram's results in number of subjects who were fully obedient. He also found that the second experimenter could not reliably identify which of the three conditions a particular subject was in. Both of these findings are interpreted by Holland (1967) and Orne and Holland (1968) as supportive of their contention that subjects in Milgram's experiments also saw through the deception but successfully hid their knowledge from the experimenter.

Complicating this interpretation are two additional findings of Holland (1967), however. First is the fact that among the subjects who were rated low in suspiciousness, 70% were fully obedient—a rate similar to Milgram's. Second, when highest voltage administered was the dependent measure, Holland found the subjects in his second condition to be significantly less obedient than those in Conditions 1 and 3 and Milgram's subjects. That is, what appeared only as a nonsignificant trend with number of subjects who were obedient turned into a full-blown statistically significant effect when the break-off point was the dependent measure. Holland also reported that neither Rotter's I-E and trust scales nor Crowne and Marlowe's Social Desirability Scale predicted obedience. However, Holland (1967) did not conduct any statistical analyses factorially combining each personality variable with the three experimental conditions. I was able to conduct such a series of analyses because Holland (1967) provided the raw data for all his subjects in an appendix. Specifically, I conducted a series of 2 (Personality Score: high vs. low) \times 3 (Condition) between-groups analyses of variance in which each personality variable, in turn, was combined factorially with the conditions variable. In one set of analyses, obedience/disobedience² served as the dependent vari-

able; in the other set, the dependent variable was the maximum shock administered. I found a conditions main effect paralleling Holland's findings; that is, obedience, as measured by the highest shock given, was significantly lower in the second condition than in either of the other two conditions. But I also found a significant I-E \times Condition interaction that clearly qualified the main effect. It showed that the drop in obedience in the second condition was largely due to the internals' obedience scores. Externals, however, did not show any drop in obedience at all.³ If one assumes that subjects in Condition 2 felt most coerced and manipulated by the experimenter, this finding is consistent with the results of other studies on the relationship between I-E and social influence. After reviewing such studies, Strickland (1977) concluded:

Internals not only appear to resist influence but react more strongly than externals to the loss of personal freedom. Internals do this in some cases by engaging in behaviors which are oppositional to the responses desired by the experimental agent who is attempting to manipulate or change behavior. (p. 232)

A final study involving the I-E dimension did not find it related to obedience. In a recent obedience study conducted in Austria by Schurz (1985), subjects were instructed to apply increasingly painful "ultrasound" stimulation to a "learner," which at its highest levels on a 20-step continuum could supposedly cause skin damage. Altogether, 45 of 56 subjects (80%) pressed all 20 switches on the switchbox, but neither Levenson's (1974) IPC scale, a three-factor version of Rotter's I-E scale, nor scores on D. N. Jackson's (1967) Personality Research Form were predictive of obedience. However, disobedient subjects had significantly higher pulse rates at the time they refused to continue and a greater tendency to accept responsibility for their actions than the obedient subjects.

The study that used measures of religious orientation in relation to obedience to authority was a doctoral dissertation by Bock (1972). Bock examined the joint effects of different types of authority (scientific vs. religious) and individual differences in religiousness as measured by scales tapping various dimensions of Christian religious orientation.

Bock conducted a "heart-problem" voice-feedback obedience experiment that systematically varied the kind of author-

² In the three analyses of variance in which obedience versus disobedience was the dependent variable, disobedience was coded as 1 and obedience as 2. None of the effects in the three analyses reached significance. Loglinear analyses (logit models) were also conducted on these data and similarly yielded nonsignificant outcomes. In each analysis, the model of equiprobability was nonsignificant, indicating that there was no significant variation across the cells in the design.

³ This pattern was essentially duplicated with obedience/disobedience as the dependent variable. Among internal subjects, only 2 out of 8 were fully obedient in Condition 2, compared with obedience rates of 8 out of 10 and 9 out of 12 in Conditions 1 and 3, respectively. Among externals, however, all three conditions yielded similarly high obedience rates: 7 out of 10, 9 out of 12, and 7 out of 8 in Conditions 1, 2, and 3, respectively. A one-way analysis of variance of the scores of only the internal subjects yielded a significant effect, whereas a similar analysis of the externals' scores did not. These results should be interpreted with caution, however, because the overall interaction *F* did not attain significance ($p < .11$).

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ity the experimenter represented. In the "scientific authority" condition, the experimenter was presented as a graduate student in psychology; in the "religious authority" condition, he was introduced to the subjects (all of whom were Christians) as a minister at a local church. A third condition, the "neutral authority condition," involved a self-decision punishment procedure in which the experimenter introduced himself as a salesman who knew very little about the experiment other than the procedure. Bock found a significant authority main effect such that both the scientific authority condition ($M = 20.68$) and the religious authority condition ($M = 16.92$) yielded significantly higher shock levels than the self-decision condition ($M = 9.24$). The difference in obedience levels between the scientific and religious authority conditions was not significant.

To assess individual differences in religiousness, Bock had his subjects complete three measures. The primary one was King and Hunt's multidimensional measure of religious orientation (King & Hunt, 1972), consisting of separate scales tapping a large domain of Christian religious response including religious beliefs, attitudes, knowledge, and behavior. One factor, for example, is called Creedal Assent, measuring the degree of endorsement of traditional Christian theology. An example of another factor is Devotionalism, which is composed of eight items focusing on inner religious experience. Bock conducted 16 factorial analyses of variance, in each case a different religious dimension being combined with the authority variable. In addition to the authority condition main effect already described, Bock found nine of the analyses to yield significant religious dimension main effects and eight significant Religious Dimension \times Authority Condition interactions.

The interactions indicated that the significant authority main effect (i.e., that both the scientific and religious authority elicited significantly higher amounts of obedience than the neutral authority) was moderated by each of the eight religious dimensions. That is, they revealed that the significant authority main effect did not hold equally across the whole range of positions on the religious variables. Examining the patterns of shock scores elicited by the religious and scientific authorities relative to scores in the neutral authority condition, one finds, generally speaking, that they tend to increase as one goes from the least religious, through the moderately religious, to the highly religious subjects. Taking into account the significant simple effects, the nature of the interactions can be described as follows: Among the highly and moderately religious subjects, the scientific authority was always more effective than the neutral authority, and in most cases, so too was the religious authority. However, among the least religiously oriented subjects either no authority (religious or scientific) was more effective in eliciting obedience than the neutral authority condition (in five cases) or at most only one—the scientific authority—was more effective (three cases).

Bock also administered another measure of religiousness—Allport and Ross's (1967) Religious Orientation Scale (ROS). Besides the significant main effect of type of authority, Bock found a significant Religious Orientation \times Authority Condition interaction indicating that although scores on the ROS did not bear a relationship to obedience in the religious and neutral authority conditions, they did have a differential effect in the scientific authority condition. Intrinsically religious subjects

were most obedient, followed by the indiscriminantly pro-religious and the extrinsically religious, with the indiscriminantly antireligious showing the least obedience. In fact, among the latter, neither the scientific nor the religious authority was any more effective than the neutral authority.⁴

To sum up, the dispositional variables just reviewed tapped, directly or indirectly, beliefs about external controlling influences on one's life. In the case of the religious dispositional variables in Bock's study, the beliefs related to divine influence and authority, whereas in the case of locus of control (studies by Miller, Holland, and Schurz) the source of external influence was more amorphous or varied (e.g., chance, luck, or fate). What three out of four of these studies suggest is that beliefs about ceding versus retaining personal control seem to be salient and predisposing factors in obedience to authority. The evidence, in this regard, is clearest with religious variables, that is, variables centered around the belief that one's life is under divine control: Bock found that the higher scorers on many of the King-Hunt religious variables or the more intrinsically religious on Allport's ROS were more accepting of the commands of an authority. But those who scored low on a number of the King-Hunt measures or were indiscriminantly antireligious as measured by the ROS tended to reject any authority, be it scientific or religious.

The evidence regarding the salience for obedience to authority of beliefs about retaining versus relinquishing personal control over one's life as tapped by Rotter's locus of control measure is somewhat less clear and more complicated. My reanalysis of Holland's results revealed that the drop in maximum shock given in his Condition 2 (problem-solving set) subjects was largely due to the internals' scores in that condition. This finding is consistent with the theoretical view of the internal as one who believes that his or her outcomes are under personal control but is complicated by the fact that it was not duplicated with the same degree of statistical clarity when the dependent measure was the proportion of subjects who were fully obedient. Miller found that externals were more obedient to a higher than a lower status experimenter, whereas internals were not differentially affected by the status of the authority. Again, though this finding is consistent with theoretical expectations based on the locus of control construct, it is potentially limited by the atypical form of obedience involved (i.e., self-inflicted pain). Whether or not there would be a similar status by I-E interaction in the more usual obedience situation remains an open question.

Role of Interactionism in Obedience

The trait-situation debate divided personality and social psychology for many years, beginning with Mischel's (1968, 1969)

⁴ Bock (1972) also gave his subjects a third religious measure, the Inventory of Religious Belief, a 15-item "unidimensional measure of [Christian] doctrinal position" (p. 53). Unlike an earlier study (Bock & Warren, 1972) that found a curvilinear relationship (with religious moderates being most obedient), Bock (1972) found the scale to correlate positively with amount of shock given. However, he did not examine the joint effects of scale score and authority condition in a factorial design.

criticisms of personality traits. However, many, if not most, social and personality psychologists would now consider the trait-situation controversy as having been laid to rest, and the development that has been largely responsible for its demise is interactionism—the perspective that, in its most general sense, stresses the importance of viewing behavior as a product of both personal and situational factors (Blass, 1984a, 1987). Although there are some who have expressed reservations (e.g., Ajzen, 1987; Epstein, 1980; Nisbett, 1977), there is now widespread agreement among personality and social psychologists of a variety of theoretical perspectives (e.g., Bem, 1983; Bowers, 1973; Endler, 1984; Eysenck & Eysenck, 1980; and various chapters in Blass, 1977b, and Magnusson & Endler, 1977) about the desirability of a Person \times Situation interactional approach. Given this wide consensus, it would seem appropriate to examine the relationship between the obedience research and interactionism.

In particular, one can ask: Can obedience to authority be added to the roster of behavioral domains in which the use of interactional designs and findings of personality by situation interactions demonstrate the resulting predictive gain (Blass, 1977a, 1984b)? As one who has used interactional designs and a moderator-variable approach (Blass, 1969, 1974; Blass, Alperstein, & Block, 1974; Blass & Bauer, 1988) and advocated their use in the study of social behavior (Blass, 1977a, 1984b), I was especially attentive, as I reviewed the obedience literature, for personality by situation designs and outcomes.

Despite my vigilance, my search was rewarded with only a modest yield. Even if one includes studies incorporating dispositional variables other than personality measures, there are only eight studies in which a disposition by situation interaction was a possibility, that is, in which an experimental manipulation and an individual-difference variable were combined in a factorial design.

One of these studies (Miranda et al., 1981) that had incorporated a personality variable—introversion/extraversion—yielded no significant effects whatsoever. Four studies (Costanzo, 1976; Kilham & Mann, 1974; Shanab & Yahya, 1977, 1978) had included subject gender as a factor in the design, but only one, the Kilham and Mann study, yielded a significant Subject Sex \times Treatment interaction. Men were more obedient than women only when they were actually administering the shocks (executant role), but there were no male-female differences in obedience when they were simply transmitting the experimenter's orders to the shocker.

Altogether, there were only three studies whose interactional designs served to advance our understanding of obedience to authority. These were the three experiments described in the previous section (Bock, 1972; Holland, 1967; F. D. Miller, 1975) implicating beliefs about external, controlling influences as a predisposing factor in obedience to authority.

Clearly, in terms of the actual number of studies promoted, interactionism has had only limited impact. Its main contribution to obedience to authority lies elsewhere, however. One of the ways that interactionist perspectives have contributed to the resolution of the trait-situation debate is by the introduction of moderator variables to help specify some conditions for improving the predictability of social behavior. That is, theorizing and research precipitated by the trait-situation controversy has

helped identify both situational moderators that can interact with personality variables and personality moderators that can interact with situational variables to yield improved prediction of behavior. Examining this research, one can identify a number of moderator variables that are especially relevant to the obedience experiments. That is, the moderator variable perspective can suggest factors that might account for the difficulties encountered in this article in explaining and predicting obedient behavior in a coherent and consistent fashion by both situational and personal determinants.

Situational Moderators

Strong versus weak situations. A number of writers have argued that strong situations are less conducive for the predictiveness of personality variables than weak situations (Ickes, 1982; Kenrick & Funder, 1988; Mischel, 1977; Monson, Hesley, & Chernick, 1982). Quite clearly, the Milgram obedience paradigm epitomizes a "strong" situation. Its high degree of experimental realism requires subjects to attend to its demands and makes it virtually impossible to respond in a detached, uninvolved manner. Furthermore, behavioral alternatives are unambiguous and limited—the subject can only increase the voltage or quit the experiment. Consistent with the applicability of the strong/weak distinction to the obedience experiment is the fact that dispositional measures of aggressiveness have been shown to be predictive of behavior only in the Buss-type aggression paradigm, that is, self-decision experiments in which subjects can choose from among a set of shock intensities on each trial (Larsen, Coleman, Forbes, & Johnson, 1972; Scheier, Buss, & Buss, 1978; Wilkins, Scharff, & Schlottmann, 1974; Youssef, 1968), a "weaker," less constraining situation than the Milgram paradigm.

Chosen versus imposed situations. One of the tenets of the interactionist position is that not only do situations affect the person, but persons also influence situations by their choice or creation of situations conducive to the expression of their personalities (Bowers, 1973; Olweus, 1977; Stagner, 1976; Wachtel, 1973). A number of researchers (Emmons, Diener, & Larsen, 1986; Feather & Volkmer, 1988; Gormly, 1983; Leary, Wheeler, & Jenkins, 1986; Snyder & Gangestad, 1982) have indeed shown that personality variables can predict situation choices and preferences. Furthermore, it has been shown that dispositional measures are better predictors of behavior within freely chosen situations than in ones not of the person's choosing (Emmons et al., 1986; Snyder, 1983). Even though Milgram's subjects, as well as those in most replications, were volunteers, it is highly unlikely that many would have chosen to be in an obedience experiment had its exact details been disclosed to them beforehand. And once the experiment is under way and its (presumably) distasteful procedures become evident to the subject, "binding factors" (Milgram, 1974, pp. 146–152)—psychological inhibiting mechanisms, such as the incremental nature of the shock procedure—keep subjects in the situation even if they want to leave it. Thus, we have another factor—the fact that subjects did not choose the situation they find themselves in—that can be expected to weaken the link between personality and behavior in the Milgram experiments.

Heightened versus diminished self-awareness. In 1972, Duval

and Wicklund introduced their theory of objective self-awareness, which built on a basic distinction in the individual's focus of attention. According to Duval and Wicklund, a person's conscious attention can be directed either inward at aspects of the self or outward toward his or her surroundings. A heightened state of self-focus has been typically created by laboratory props such as mirrors and television cameras. Research has shown that one of the consequences of a heightened state of self-awareness is to increase the accuracy of self-reports (see Gibbons, 1983, for a review). That is, there is evidence that when subjects complete an attitudinal or personality measure during a heightened state of self-focus, the measure becomes a stronger predictor of behavior than is the case without the manipulation of attention toward the self. The conditions that prevail within the setting of a Milgram obedience experiment are typically conducive of an *inhibition* of self-awareness, rather than an enhancement of it. The subject's attention is focused outward rather than inward, absorbed in the mechanical details of the procedure. In fact, it has been suggested (Carver, 1975) that the considerable amount of physical activity required to work a shock machine might actually artificially depress the subject's self-awareness. (The subject's high degree of task absorption and narrowing of focus, as well as some other experimental details, have even led some writers [Hunt, 1979; Rosenbaum, 1983] to suggest that he or she is very similar to a hypnotized subject.) Also drawing attention away from the self is the subject's attunement to the experimenter's commands and to the learner's answers and complaints. There is some disagreement about the degree to which the experimenter rather than the learner claims the subject's attention (J. M. Jackson, 1982, pp. 22-23; Milgram, 1974, p. 144). What is clear, however, is that features of the typical Milgram-type obedience experiment are anything but promotive of self-awareness. And to the extent that this is true, the conditions are not optimal for the emergence of strong disposition-behavior relationships.

Dispositional Moderators

It has been shown earlier in this article that although situational factors have affected obedience, they have not done so in a coherent and predictable way.

The trait of consistency-variability. A possible solution is provided by the fact that the disposition to be cross-situationally consistent or inconsistent is itself an individual-difference variable. Allport, in 1937, had already mentioned efforts "to determine whether consistency (or its opposite, variability) is itself a consistent attribute of personality" (p. 356). Within contemporary interactionist perspectives, this idea is most centrally embedded in the personality construct of self-monitoring (Snyder, 1974, 1979). According to Snyder, low self-monitors, but not high self-monitors, are expected to show trait-like consistency in their behaviors. The latter are more attuned to situational cues for behavioral guidance, and their actions will therefore be more variable from situation to situation. Thus, the fact that situational manipulations have not always affected obedience in a reliable and predictable fashion could be due to the fact that the samples involved were likely a mixture of high and low self-monitors. On the basis of the theory of self-monitoring, if subjects were divided into high and low self-monitors, one

would expect high self-monitors to show differential responsibility to the situational variations in an obedience experiment, whereas the low self-monitors would maintain a more consistent level of obedience despite changes in some features of the experiment.

Cross-national differences: Modal personality. One can also extend the idea of dispositional moderators to provide a possible explanation for cross-national differences in obedience that I have identified in this article. For example, in the four-part proximity series, Milgram (1974) found visibility of the victim to significantly reduce the level of obedience of his (American) subjects. In Spain, however, Miranda et al. (1981) were not able to replicate this finding. In their study, obedience was equally high in both a condition in which the teacher could see the learner and one in which he could not. Perhaps the modal personality (Inkeles & Levinson, 1969) of Spanish individuals is more cross-situationally consistent than that of Americans, or, more generally, what constitutes equivalence classes of situational stimulus characteristics can be expected to vary somewhat from culture to culture. This idea, that there might be cross-cultural differences in cross-situational consistency and variability, is derived from Kurt Lewin's theorizing about the social-psychological differences he observed between the United States and Germany in the pre-World War II years. Lewin (1948; originally published in 1936) discussed how changes in the immediate situation differentially affected Americans and Germans. He felt that the typical American "shows a greater difference in his behavior in accordance with the given situation than the [typical German]." The latter, he argued, "carries more of his specific individual characteristics to every situation. His behavior will therefore be less modified in altered situations" (pp. 30-31).

A dispositional explanation of a different sort might also account for another cross-cultural difference in obedience. In Australia, Kilham and Mann (1974) found a significantly lower rate of obedience (28%) than Milgram (1974) did in a comparable voice-feedback condition (Experiment 2; 62.5%) with his American subjects, $\chi^2(1, N = 90) = 10.77, p < .01$.⁵ On the basis of Mann's (1973) findings of Australian-American differences in attitudes toward obeying military commands, Kilham and Mann suggested that their finding of lower obedience rates might be due to "national differences in obedience ideology that contribute to a predisposition to obey or defy authority" (p. 702).

Conclusions

The guiding focus of this article was the historically important question of the relative efficacy of personality and situational factors in accounting for social behavior, as applied to the accumulated body of research on obedience to authority using Milgram's paradigm. I believe the findings argue for the two factors being on a more equal footing than past scholarly wisdom would have it. My article has shown that obedience can vary as a function of both personality variables and situational factors but that there are problems associated with both kinds

⁵ Chi-square was computed by me.

of determinants. The findings on personality predictors of obedience revealed some of them to be weak or contradictory and that the evidence for theoretically based personality-obedience links was mixed. One can also argue that some of the evidence (e.g., Haas's, 1966, study with management personnel) is too far afield from the original Milgram experiments to have a bearing on them. The obedience studies focusing on situational determinants revealed that many experimental manipulations were effective, though not always reliably so. Others were not, even though logic or findings from related behavioral domains would suggest that they should be. And among the situational effects that do emerge, there is a lack of coherent and predictable patterns, making the extraction of the relevant underlying dimensions difficult.

Among Mischel's (1968, 1969) criticisms of transsituational personality dispositions or traits was his contention that situational variables are stronger predictors of behavior than individual differences (Mischel, 1968, pp. 81-83; 1969, p. 1014), a position he modified in later writings (Mischel, 1973, pp. 255-256; 1984). One of the first contributions of interactionist writings was to argue and demonstrate empirically that the "proportion of variance" question was a pseudoissue (e.g., Bowers, 1973; Endler, 1973; Sarason, Smith, & Diener, 1975), with persons and situations accounting for equally small proportions of variance.

My detailed analyses of studies dealing with one of the most widely discussed topics in social psychology—obedience to authority—puts some flesh on the figures provided by the "proportion of variance" surveys and analyses. My review has shown that although amount of obedience can vary as a function of situational manipulations and differ among individuals within the same setting, neither the proposed situational dimensions (e.g., immediacy or salience of victim) nor the personality variables studied as potential individual-difference correlates (e.g., authoritarianism) have accounted for the variations in a consistent, orderly, and predictable manner. Situational and personality perspectives on the obedience findings are on equal footing because their problem is essentially the same: discovering the constructs that can account for variations in obedience in a coherent way. In the case of situational manipulations, this translates into finding the appropriate underlying situational dimensions that seem to be operationalized by the experimental treatments. In the case of individual differences in obedience found within the same stimulus situation, it is the question of the measured personality correlate, be it a trait or another type of disposition, that provides the best theoretical and empirical fit for the data.

More broadly speaking, I believe my findings can serve a clarifying and corrective function vis-à-vis situationist perspectives on the determinants of social behavior much like those of others throughout the history of the trait-situation debate (e.g., Bem & Allen, 1974; Block, 1968; Bowers, 1973; Hogan, DeSoto, & Solano, 1977; Kenrick & Funder, 1988; Sarason et al., 1975).

My review also complements a clever statistical approach to this question of whether situational or personality determinants are more powerful taken by Funder and Ozer (1983). The situationist claim regarding the low predictive power of personality traits—with validity coefficients of .20-.30 being described as the norm (Mischel, 1968, p. 78) and .40 as the maximum (Nis-

bett, 1980, p. 124)—carries with it the complementary implication that situational factors are typically stronger predictors. Funder and Ozer (1983) refuted this claim by converting a number of well-known outcomes of situational manipulations—including two of Milgram's (1974)—into linear correlations. Specifically, they computed the relationship between the degree of subject-victim proximity and amount of obedience in the four-part proximity series (Milgram, 1974; Experiments 1 to 4) and found it to be equal to an r of .42, whereas the correlation between presence versus absence of the authority and obedience (Experiments 5 vs. 7) was found to be equal to .36.

Obedience studies involving Person \times Situation interactions, though few in number, did highlight the importance of underlying beliefs—about external, controlling influences—as a salient, predisposing factor in obedience to authority. The small number of interactional studies of obedience reported is probably a result of the historical cooccurrence of two developments. The early and mid-1970s marked both the advent of contemporary interactionism and of federal regulations and American Psychological Association (APA) guidelines on research with human subjects. So just when many personality and social psychologists were becoming sensitized to the value of person by situation designs, the doors were closing on Milgram obedience experiments of any sort. In fact, the last time Milgram-type obedience experiments conducted in the United States were reported in the literature was 1976 (Costanzo, 1976; Holmes, 1976).⁶ Rather than in sheer number of studies promoted, interactionist perspectives have made a contribution by providing some integration of the literature through the suggestion of a number of moderator variables that, when applied to the obedience experiment, helped identify factors (most of them inherent in the features of the Milgram obedience paradigm) that make predicting obedience from situational or dispositional factors difficult.

The complexities of predicting obedience that I have identified in this article do not diminish the enduring significance of Milgram's obedience research. After 30 years, it still remains the prime example of creative experimental realism used in the service of a question of deep social and moral significance. It has been without parallel in social psychology, and perhaps psychology as a whole, as a catalyst of productive scholarly and public debate. Milgram (1977a) once commented admiringly on the fact that the conformity paradigm of Solomon Asch, his mentor, produced many variations: "For me, Asch's experiment rotates as a kind of permanent intellectual jewel. Focus analytic light on it, and it diffracts energy into new and interesting patterns" (p. 152). When one considers the number of issues the obedience work has been applied to, the amount of controversy it has generated, and the differing ways the findings have been

⁶ Geller's journal report of his role-playing versions of three of Milgram's obedience experiments was published later, in 1978, but it was based on his dissertation, which came out in 1975. It should be noted that although obedience experiments have apparently not been conducted in the United States since the mid-1970s, replications have continued to be carried out in other countries (i.e., Burley & McGuinness, 1977; Meeus & Raaijmakers, 1986, 1987; Miranda, Caballero, Gomez, & Zamorano, 1981; Schurz, 1985; Shanab & Yahya, 1977, 1978; Shelton, 1982).

interpreted, Milgram's metaphor of "a kind of permanent intellectual jewel" can just as appropriately be applied to his own obedience paradigm. It is a reflection on the universality of the themes the obedience research speaks to, such as the human propensity for evil and hierarchical role relationships, that interest in it has not been confined to academia. From the beginning, journalists (e.g., Reinert, 1970; Sullivan, 1963) and political and social commentators (e.g., Karnow, 1971; Krauthammer, 1985) have found relevance in it. And it continues to inspire research and analysis (Blass, 1990a, 1990b; Meeus & Raaijmakers, 1986, 1987; A. G. Miller, 1986) and influence conceptualizations about obedience-related phenomena (Haritos-Fatouros, 1988; Kelman & Hamilton, 1989).

The dramatic demonstration that people are much more prone to obey the orders of a legitimate authority than one might have expected remains an enduring insight, but one that is in need of some qualification: Milgram (1963, 1965c) did indeed find drastic underestimations of full obedience (with 3% of the subjects, at the most, expected to obey), but others (e.g., Kaufmann & Kooman, 1967; Mixon, 1971) have obtained findings suggesting that greater accuracy in predicting the outcome of an obedience experiment is possible. Milgram also showed how difficult it is for people to translate their intentions into actions even when moral principles might be at stake, and that momentary situational pressures and norms (e.g., rules of deference to an authority) can exert a surprising degree of influence on people's behavior. According to Milgram, they wield their power through the unexpected amount of inhibitory anxiety generated by their breach.

Almost as provocative as his finding of the extreme willingness of individuals to obey a legitimate authority is Milgram's contention that this comes about through the person's acceptance of the authority's definition of reality. As he (Milgram, 1965c, p. 74) put it: "Men who are in everyday life responsible and decent were seduced by the trappings of authority, by the control of their perceptions, and by the uncritical acceptance of the experimenter's definition of the situation, into performing harsh acts."

Although one can question the exact parallels between the actions of Milgram's subjects and those of the Nazis under Hitler, the obedience studies have clearly contributed to a continued awareness of the Holocaust and to attempts at understanding its causes. This becomes increasingly important at a time when witnesses to the Holocaust are gradually dying out and a revisionism, denying the Nazis' murder of 6,000,000 Jews, is on the rise. Hopefully, such "consciousness raising" can help prevent any future attempts at genocide. The potential value of the obedience experiments in this regard is no trivial matter—especially to those of us who are survivors of the Holocaust.

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Behavioral Study of Obedience

Stanley Milgram

(1963)

This article describes a procedure for the study of destructive obedience in the laboratory. It consists of ordering a naive *S* to administer increasingly more severe punishment to a victim in the context of a learning experiment. Punishment is administered by means of a shock generator with 30 graded switches ranging from *Slight Shock* to *Danger: Severe Shock*. The victim is a confederate of the *E*. The primary dependent variable is the maximum shock the *S* is willing to administer before he refuses to continue further. 26 *Ss* obeyed the experimental commands fully, and administered the highest shock on the generator. 14 *Ss* broke off the experiment at some point after the victim protested and refused to provide further answers. The procedure created extreme levels of nervous tension in some *Ss*. Profuse sweating, trembling, and stuttering were typical expressions of this emotional disturbance. One unexpected sign of tension — yet to be explained — was the regular occurrence of nervous laughter, which in some *Ss* developed into uncontrollable seizures. The variety of interesting behavioral dynamics observed in the experiment, the reality of the situation for the *S*, and the possibility of parametric variation within the framework of the procedure, point to the fruitfulness of further study.¹

OBEDIENCE is as basic an element in the structure of social life as one can point to. Some system of authority is a requirement of all communal living, and it is only the man dwelling in isolation who is not forced to respond, through defiance or submission, to the commands of others. Obedience, as a determinant of behavior, is of particular relevance to our time. It has been reliably established that from 1933–45 millions of innocent persons were systematically slaughtered on command. Gas chambers were built, death camps were guarded; daily quotas of corpses were produced with the same efficiency as the manufacture of appliances. These inhumane policies may have originated in the mind of a single person, but they could only be carried out on a massive scale if a

very large number of persons obeyed orders.

Obedience is the psychological mechanism that links individual action to political purpose. It is the dispositional cement that binds men to systems of authority. Facts of recent history and observation in daily life suggest that for many persons obedience may be a deeply ingrained behavior tendency, indeed a prepotent impulse overriding training in ethics, sympathy, and moral conduct. C. P. Snow (1961) points to its importance when he writes:

When you think of the long and gloomy history of man, you will find more hideous crimes have been committed in the name of obedience than have ever been committed in the name of rebellion. If you doubt that, read William Shirer's *Rise and Fall of the Third Reich*. The German Officer Corps were brought up in the most rigorous code of obedience . . . in the name of obedience they were party to, and assisted in, the most wicked large scale actions in the history of the world [p. 24].

While the particular form of obedience dealt with in the present study has its antecedents in these episodes, it must not be thought all obedience entails acts of aggression against others. Obedience serves numerous productive functions. Indeed, the very life of society is predicated on its existence. Obedience may be ennobling and educative and refer to acts of charity and kindness as well as to destruction.

General Procedure

A procedure was devised which seems useful as a tool for studying obedience (Milgram, 1961). It consists of ordering a naive subject to administer electric shock to a victim. A simulated shock gen-

¹Milgram, S. (1963). Behavioral study of obedience. *Journal of Abnormal and Social Psychology*, 67, 371–378.

erator is used, with 30 clearly marked voltage levels that range from 15 to 450 volts. The instrument bears verbal designations that range from *Slight Shock* to *Danger: Severe Shock*. The responses of the victim, who is a trained confederate of the experimenter, are standardized. The orders to administer shocks are given to the naive subject in the context of a "learning experiment" ostensibly set up to study the effects of punishment on memory. As the experiment proceeds the naive subject is commanded to administer increasingly more intense shocks to the victim, even to the point of reaching the level marked *Danger: Severe Shock*. Internal resistances become stronger, and at a certain point the subject refuses to go on with the experiment. Behavior prior to this rupture is considered "obedience," in that the subject complies with the commands of the experimenter. The point of rupture is the act of disobedience. A quantitative value is assigned to the subject's performance based on the maximum intensity shock he is willing to administer before he refuses to participate further. Thus for any particular subject and for any particular experimental condition the degree of obedience may be specified with a numerical value. The crux of the study is to systematically vary the factors believed to alter the degree of obedience to the experimental commands.

The technique allows important variables to be manipulated at several points in the experiment. One may vary aspects of the source of command, content and form of command, instrumentalities for its execution, target object, general social setting, etc. The problem, therefore, is not one of designing increasingly more numerous experimental conditions, but of selecting those that best illuminate the *process* of obedience from the sociopsychological standpoint.

Table 1.

Distribution of Age and Occupational Types in the Experiment

Occupations	Ages			% of Total (occupations)
	20-29	30-39	40-50	
Workers, skilled and unskilled	4	5	6	37.5
Sales, business and white-collar	3	6	7	40.0
Professional	1	5	3	22.5
% of total (Age)	20	40	40	

Related Studies

The inquiry bears an important relation to philosophic analyses of obedience and authority (Arendt, 1958; Friedrich, 1958; Weber, 1947), an early experimental study of obedience by Frank (1944), studies in "authoritarianism" (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950; Rokeach, 1961), and a recent series of analytic and empirical studies in social power (Cartwright, 1959). It owes much to the long concern with *suggestion* in social psychology, both in its normal forms (e.g., Binet, 1900) and in its clinical manifestations (Charcot, 1881). But it derives, in the first instance, from direct observation of a social fact; the individual who is commanded by a legitimate authority ordinarily obeys. Obedience comes easily and often. It is a ubiquitous and indispensable feature of social life.

Method

Subjects

The subjects were 40 males between the ages of 20 and 50, drawn from New Haven and the surrounding communities. Subjects were obtained by a newspaper advertisement and direct mail solicitation. Those who responded to the appeal believed they were to participate in a study of memory and learning at Yale University. A wide range of occupations is represented in the sample. Typical subjects were postal clerks, high school teachers, salesmen, engineers, and laborers. Subjects ranged in educational level from one who had not finished elementary school, to those who had doctorate and other professional degrees. They were paid \$4.50 for their participation in the experiment.

However, subjects were told that payment was simply for coming to the laboratory, and that the money was theirs no matter what happened after they arrived. Table 1 shows the proportion of age and occupational types assigned to the experimental condition.

Personnel and Locale

The experiment was conducted on the grounds of Yale University in the elegant interaction laboratory. (This detail is relevant to the perceived legitimacy of the experiment. In further variations, the experiment was dissociated from the university, with consequences for performance.) The role of experimenter was played by a 31-year-old high school teacher of biology. His manner was impassive, and his appearance somewhat stern throughout the experiment. He was dressed in a gray technician's coat. The victim was played by a 47-year-old accountant, trained for the role; he was of Irish-American stock, whom most observers found mild-mannered and likable.

Procedure

One naive subject and one victim (an accomplice) performed in each experiment. A pretext had to be devised that would justify the administration of electric shock by the naive subject. This was effectively accomplished by the cover story. After a general introduction on the presumed relation between punishment and learning, subjects were told:

But actually, we know *very little* about the effect of punishment on learning, because almost no truly scientific studies have been made of it in human beings.

For instance, we don't know how *much* punishment is best for learning — and we don't know how much difference it makes as to who is giving the punishment, whether an adult learns best from a younger or an older person than himself — or many things of that sort.

So in this study we are bringing together a number of adults of different occupations and ages. And we're asking some of them to be teachers and some of them to be learners.

We want to find out just what effect different people have on each other as teachers and learners, and also

what effect *punishment* will have on learning in this situation.

Therefore, I'm going to ask one of you to be the teacher here tonight and the other one to be the learner.

Does either of you have a preference?

Subjects then drew slips of paper from a hat to determine who would be the teacher and who would be the learner in the experiment. The drawing was rigged so that the naive subject was always the teacher and the accomplice always the learner. (Both slips contained the word "Teacher.") Immediately after the drawing, the teacher and learner were taken to an adjacent room and the learner was strapped into an "electric chair" apparatus.

The experimenter explained that the straps were to prevent excessive movement while the learner was being shocked. The effect was to make it impossible for him to escape from the situation. An electrode was attached to the learner's wrist, and electrode paste was applied "to avoid blisters and burns." Subjects were told that the electrode was attached to the shock generator in the adjoining room.

In order to improve credibility the experimenter declared, in response to a question by the learner: "Although the shocks can be extremely painful, they cause no permanent tissue damage."

Learning Task The lesson administered by the subject was a paired-associate learning task. The subject read a series of word pairs to the learner, and then read the first word of the pair along with four terms. The learner was to indicate which of the four terms had originally been paired with the first word. He communicated his answer by pressing one of four switches in front of him, which in turn lit up one of four numbered quadrants in an answer-box located atop the shock generator.

Shock Generator The instrument panel consists of 30 lever switches set in a horizontal line. Each switch is clearly labeled with a voltage designation that ranges from 15 to 450 volts. There is a 15-volt increment from one switch to the next going from left to right. In addition, the following verbal designations are clearly indicated for groups of four switches going from left to right: *Slight Shock*,

Moderate Shock, Strong Shock, Very Strong Shock, Intense Shock, Extreme Intensity Shock, Danger: Severe Shock. (Two switches after this last designation are simply marked XXX.)

Upon depressing a switch: a pilot light corresponding to each switch is illuminated in bright red; an electric buzzing is heard; an electric blue light, labeled “voltage energizer,” flashes; the dial on the voltage meter swings to the right; various relay clicks are sounded.

The upper left-hand corner of the generator is labeled *Shock Generator, Type ZLB, Dyson Instrument Company, Waltham, Mass. Output 15 Volts – 450 Volts.*

Details of the instrument were carefully handled to insure an appearance of authenticity. The panel was engraved by precision industrial engravers, and all components were of high quality. No subject in the experiment suspected that the instrument was merely a simulated shock generator.²

Sample Shock Each naive subject is given a sample shock on the shock generator, prior to beginning his run as teacher. This shock is always 45 volts, and is applied by pressing the third switch of the generator. The shock is applied to the wrist of the naive subject, and has its source in a 45-volt battery wired into the generator. This further convinces the subject of the authenticity of the generator.

Shock Instructions The subject is told to administer a shock to the learner each time he gives a wrong response. Moreover — and this is the key command — the subject is instructed to “move one level higher on the shock generator each time the learner flashes a wrong answer.” He is also instructed to announce the voltage level before administering a shock. This serves to continually remind subjects of the increasing intensity of shocks administered to the learner.

Preliminary and Regular Run Pretests revealed that the procedure of reading words and adminis-

tering shocks requires some practice before it can be handled smoothly. Therefore, immediately preceding the regular run, the teacher is given a preliminary series of 10 words to read to the learner. There are 3 neutral words in the practice series (*i.e.*, words that the learner answers correctly), so that shocks are administered for 7 of the words, with the maximum shock at 105 volts (moderate shock). Almost all subjects master the procedure by the time the preliminary run is over.

Subjects are then presented with a second list, and are told that the procedure is the same as for the first list; the experimenter adds, however:

When you get to the bottom of the list, repeat it over again, and continue giving shocks, until the learner has learned all the pairs correctly.

The experimenter instructs the subject to:

start from 15 volts and increase the shock level one step each time the learner gives a wrong answer.

Feedback from the Victim In all conditions the learner gives a predetermined set of responses to the word pair test, based on a schedule of approximately three wrong answers to one correct answer. In the present experimental condition no vocal response or other sign of protest is heard from the learner until Shock Level 300 is reached. When the 300-volt shock is administered, the learner pounds on the wall of the room in which he is bound to the electric chair. The pounding can be heard by the subject. From this point on, the learner’s answers no longer appear on the four-way panel.

At this juncture, subjects ordinarily turn to the experimenter for guidance. The experimenter instructs the subject to treat the absence of a response as a wrong answer, and to shock the subject according to the usual schedule. He advises the subjects to allow 5–10 seconds before considering no response as a wrong answer, and to increase the shock level one step each time the learner fails to respond correctly. The learner’s pounding is repeated after the

² A related technique, making use of a shock generator, was reported by Buss (1961) for the study of aggression in the laboratory. Despite the considerable similarity of technical detail in the experimental procedures, both investigators proceeded in ignorance of the other’s work. Milgram provided plans and photographs of his shock generator, experimental procedure, and first results in a report to the National Science Foundation in January 1961. This report received only limited circulation. Buss reported his procedure 6 months later, but to a wider audience. Subsequently, technical information and reports were exchanged. The present article was first received in the Editor’s office on December 27, 1961; it was resubmitted with deletions on July 27, 1962.

315-volt shock is administered; afterwards he is not heard from, nor do his answers reappear on the four-way signal box.

Experimenter Feedback At various points in the experiment the subject may turn to the experimenter for advice on whether he should continue to administer shocks. Or he may indicate that he does not wish to go on. It was important to standardize the experimenter's responses to these inquiries and reactions.

If the subject indicated his unwillingness to go on, the experimenter responded with a sequence of "prods," using as many as necessary to bring the subject into line.

Prod 1: Please continue. *or* Please go on.

Prod 2: The experiment requires that you continue.

Prod 3: It is absolutely essential that you continue.

Prod 4: You have no other choice, you *must* go on.

The prods were always made in sequence: Only if Prod 1 had been unsuccessful, could Prod 2 be used. If the subject refused to obey the experimenter after Prod 4, the experiment was terminated. The experimenter's tone of voice was at all times firm, but not impolite. The sequence was begun anew on each occasion that the subject balked or showed reluctance to follow orders.

Special prods. If the subject asked if the learner was liable to suffer permanent physical injury, the experimenter said:

Although the shocks may be painful, there is no permanent tissue damage, so please go on. [Followed by Prods 2, 3, and 4 if necessary.]

If the subject said that the learner did not want to go on, the experimenter replied:

Whether the learner likes it or not, you must go on until he has learned all the word pairs correctly. So please go on. [Followed by Prods 2, 3, and 4 if necessary.]

Dependent Measures

The primary dependent measure for any subject is the maximum shock he administers before he re-

fuses to go any further. In principle this may vary from 0 (for a subject who refuses to administer even the first shock) to 30 (for a subject who administers the highest shock on the generator). A subject who breaks off the experiment at any point prior to administering the thirtieth shock level is termed a *defiant* subject. One who complies with experimental commands fully, and proceeds to administer all shock levels commanded, is termed an *obedient* subject.

Further Records With few exceptions, experimental sessions were recorded on magnetic tape. Occasional photographs were taken through one-way mirrors. Notes were kept on any unusual behavior occurring during the course of the experiments. On occasion, additional observers were directed to write objective descriptions of the subjects' behavior. The latency and duration of shocks were measured by accurate timing devices.

Interview and Dehoax Following the experiment, subjects were interviewed; open-ended questions, projective measures, and attitude scales were employed. After the interview, procedures were undertaken to assure that the subject would leave the laboratory in a state of well being. A friendly reconciliation was arranged between the subject and the victim, and an effort was made to reduce any tensions that arose as a result of the experiment.

Results

Preliminary Notions

Before giving an account of the experimental results, it is instructive to consider how persons predict others will perform when commanded to administer powerful shocks to another person. Fourteen Yale seniors, all psychology majors, were provided with a detailed description of the experimental situation. They were asked to reflect carefully on it, and to predict the behavior of 100 hypothetical subjects. More specifically, they were instructed to plot the distribution of obedience of "100 Americans of diverse occupations, and ranging in age from 20 to 50 years," who were placed in the experimental situation.

There was considerable agreement among the respondents on the expected behavior of hypothetical subjects. All respondents predicted that only an insignificant minority would go through to the end of the shock series. (The estimates ranged from 0 to 3%; *i.e.*, the most “pessimistic” member of the class predicted that of 100 persons, 3 would continue through to the most potent shock available on the shock generator — 450 volts.) The class mean was 1.2%. The question was also posed informally to colleagues of the author, and the most general feeling was that few if any subjects would go beyond the designation *Very Strong Shock*.

Result of Experiment

Subjects Accept Situation With few exceptions subjects were convinced of the reality of the experimental situation, that they were administering shocks to another person, and the most potent shocks were extremely painful. In the post-experimental interview subjects were asked: “How painful to the learner were the last few shocks that you administered to him?” Subjects were instructed to indicate their answers on a printed 14-point scale ranging from “Not at all painful” to “Extremely painful.” The modal response was 14 (Extremely painful) with the mean at 13.42.

Signs of Extreme Tension Many subjects showed signs of nervousness in the experimental situation, and especially upon administering the more powerful shocks. In a large number of cases the degree of tension reached extremes that are rarely seen in sociopsychological laboratory studies. Subjects were observed to sweat, tremble, stutter, bite their lips, groan, and dig their fingernails into their flesh. These were characteristic rather than exceptional responses to the experiment.

One sign of tension was the regular occurrence of nervous laughing fits. Fourteen of the 40 subjects showed definite signs of nervous laughter and smiling. The laughter seemed entirely out of place, even bizarre. Full-blown, uncontrollable seizures were observed for 3 subjects. On one occasion we observed a seizure so violently convulsive that it was necessary to call a halt to the experiment. The sub-

ject, a 46-year-old encyclopedia salesman, was seriously embarrassed by his untoward and uncontrollable behavior. In the post-experimental interviews subjects took pains to point out that they were not sadistic types, and that the laughter did not mean they enjoyed shocking the victim.

Distribution of Scores It had been conjectured that persons would not, in general, agree to administer shocks beyond the level designated as *Very Strong Shock*. The distribution of maximum shocks for this experiment is shown in Table 2. The obtained distribution of scores deviates radically from the prediction.

Table 2.
Distribution of Breakoff Points

<i>Verbal Designation and Voltage Indication</i>		<i>Number of Subjects for whom this was maximum shock</i>
Slight Shock	15	0
	30	0
	45	0
	60	0
Moderate Shock	75	0
	90	0
	105	0
	120	0
Strong Shock	135	0
	150	0
	165	0
	180	0
Very Strong Shock	195	0
	210	0
	225	0
	240	0
Intense Shock	255	0
	270	0
	285	0
	300	5
Extreme Intensity Shock	315	4
	330	2
	345	1
	360	1
Danger: Severe Shock	375	4
	390	2
	405	1
	420	1
XXX	435	0
	450	26

Upon command of the experimenter, each of the 40 subjects went beyond the expected break-off point. No subject stopped prior to administer-

ing Shock Level 20. (At this level — 300 volts — the victim kicks on the wall and no longer provides answers to the teacher's multiple-choice questions.)

Of the 40 subjects, 5 refused to obey the experimental commands beyond the 300-volt level. Four more subjects administered one further shock, and then refused to go on. Two broke off at the 330-volt level, and 1 each at 345, 360, and 375 volts. Thus a total of 14 subjects defied the experimenter. It is instructive to reprint their remarks at the point of defiance, as transcribed from the tape recordings:

[0124] I think he's trying to communicate, he's knocking. ... Well it's not fair to shock the guy ... these are terrific volts. I don't think this is very humane. ... Oh, I can't go on with this; no, this isn't right. It's a hell of an experiment. The guy is suffering in there. No, I don't want to go on. This is crazy. [Subject refused to administer more shocks.]

[0123] He's banging in there. I'm gonna chicken out. I'd like to continue, but I can't do that to a man. ... I'm sorry I can't do that to a man. I'll hurt his heart. You take your check. ... No really, I couldn't do it.

These subjects were frequently in a highly agitated and even angered state. Sometimes, verbal protest was at a minimum, and the subject simply got up from his chair in front of the shock generator, and indicated that he wished to leave the laboratory.

Of the 40 subjects, 26 obeyed the orders of the experimenter to the end, proceeding to punish the victim until they reached the most potent shock available on the shock generator. At that point, the experimenter called a halt to the session. (The maximum shock is labeled 450 volts, and is two steps beyond the designation: *Danger: Severe Shock.*) Although obedient subjects continued to administer shocks, they often did so under extreme stress. Some expressed reluctance to administer shocks beyond the 300-volt level, and displayed fears similar to those who defied the experimenter; yet they obeyed.

After the maximum shocks had been delivered, and the experimenter called a halt to the proceedings, many obedient subjects heaved sighs of relief, mopped their brows, rubbed their fingers over their eyes, or nervously fumbled cigarettes. Some shook their heads, apparently in regret. Some subjects had remained calm throughout the experiment, and dis-

played only minimal signs of tension from beginning to end.

Discussion

The experiment yielded two findings that were surprising. The first finding concerns the sheer strength of obedient tendencies manifested in this situation. Subjects have learned from childhood that it is a fundamental breach of moral conduct to hurt another person against his will. Yet, 26 subjects abandon this tenet in following the instructions of an authority who has no special powers to enforce his commands. To disobey would bring no material loss to the subject; no punishment would ensue. It is clear from the remarks and outward behavior of many participants that in punishing the victim they are often acting against their own values. Subjects often expressed deep disapproval of shocking a man in the face of his objections, and others denounced it as stupid and senseless. Yet the majority complied with the experimental commands. This outcome was surprising from two perspectives: first, from the standpoint of predictions made in the questionnaire described earlier. (Here, however, it is possible that the remoteness of the respondents from the actual situation, and the difficulty of conveying to them the concrete details of the experiment, could account for the serious underestimation of obedience.)

But the results were also unexpected to persons who observed the experiment in progress, through one-way mirrors. Observers often uttered expressions of disbelief upon seeing a subject administer more powerful shocks to the victim. These persons had a full acquaintance with the details of the situation, and yet systematically underestimated the amount of obedience that subjects would display.

The second unanticipated effect was the extraordinary tension generated by the procedures. One might suppose that a subject would simply break off or continue as his conscience dictated. Yet, this is very far from what happened. There were striking reactions of tension and emotional strain. One observer related:

I observed a mature and initially poised businessman enter the laboratory smiling and confident. Within

20 minutes he was reduced to a twitching, stuttering wreck, who was rapidly approaching a point of nervous collapse. He constantly pulled on his earlobe, and twisted his hands. At one point he pushed his fist into his forehead and muttered: "Oh God, let's stop it." And yet he continued to respond to every word of the experimenter, and obeyed to the end.

Any understanding of the phenomenon of obedience must rest on an analysis of the particular conditions in which it occurs. The following features of the experiment go some distance in explaining the high amount of obedience observed in the situation.

1. The experiment is sponsored by and takes place on the grounds of an institution of unimpeachable reputation, Yale University. It may be reasonably presumed that the personnel are competent and reputable. The importance of this background authority is now being studied by conducting a series of experiments outside of New Haven, and without any visible ties to the university.
2. The experiment is, on the face of it, designed to attain a worthy purpose — advancement of knowledge about learning and memory. Obedience occurs not as an end in itself, but as an instrumental element in a situation that the subject construes as significant, and meaningful. He may not be able to see its full significance, but he may properly assume that the experimenter does.
3. The subject perceives that the victim has voluntarily submitted to the authority system of the experimenter. He is not (at first) an unwilling captive impressed for involuntary service. He has taken the trouble to come to the laboratory presumably to aid the experimental research. That he later becomes an involuntary subject does not alter the fact that, initially, he consented to participate without qualification. Thus he has in some degree incurred an obligation toward the experimenter.
4. The subject, too, has entered the experiment voluntarily, and perceives himself under obligation to aid the experimenter. He has made a commitment, and to disrupt the experiment is a repudiation of this initial promise of aid.

5. Certain features of the procedure strengthen the subject's sense of obligation to the experimenter. For one, he has been paid for coming to the laboratory. In part this is canceled out by the experimenter's statement that:

"Of course, as in all experiments, the money is yours simply for coming to the laboratory. From this point on, no matter what happens, the money is yours."³

6. From the subject's standpoint, the fact that he is the teacher and the other man the learner is purely a chance consequence (it is determined by drawing lots) and he, the subject, ran the same risk as the other man in being assigned the role of learner. Since the assignment of positions in the experiment was achieved by fair means, the learner is deprived of any basis of complaint on this count. (A similar situation obtains in Army units, in which — in the absence of volunteers — a particularly dangerous mission may be assigned by drawing lots, and the unlucky soldier is expected to bear his misfortune with sportsmanship.)
7. There is, at best, ambiguity with regard to the prerogatives of a psychologist and the corresponding rights of his subject. There is a vagueness of expectation concerning what a psychologist may require of his subject, and when he is overstepping acceptable limits. Moreover, the experiment occurs in a closed setting, and thus provides no opportunity for the subject to remove these ambiguities by discussion with others. There are few standards that seem directly applicable to the situation, which is a novel one for most subjects.
8. The subjects are assured that the shocks administered to the subject are "painful but not dangerous." Thus they assume that the discomfort caused the victim is momentary, while the scientific gains resulting from the experiment are enduring.
9. Through Shock Level 20 the victim continues to provide answers on the signal box. The subject may construe this as a sign that the victim is still willing to "play the game." It is only after Shock Level 20 that the victim repudiates the rules completely, refusing to answer further.

³Forty-three subjects, undergraduates at Yale University, were run in the experiment without payment. The results are very similar to those obtained with paid subjects.

These features help to explain the high amount of obedience obtained in this experiment. Many of the arguments raised need not remain matters of speculation, but can be reduced to testable propositions to be confirmed or disproved by further experiments.⁴

The following features of the experiment concern the nature of the conflict which the subject faces.

10. The subject is placed in a position in which he must respond to the competing demands of two persons: the experimenter and the victim. The conflict must be resolved by meeting the demands of one or the other; satisfaction of the victim and the experimenter are mutually exclusive. Moreover, the resolution must take the form of a highly visible action, that of continuing to shock the victim or breaking off the experiment. Thus the subject is forced into a public conflict that does not permit any completely satisfactory solution.

11. While the demands of the experimenter carry the weight of scientific authority, the demands of the victim spring from his personal experience of pain and suffering. The two claims need not be regarded as equally pressing and legitimate. The experimenter seeks an abstract scientific datum; the victim cries out for relief from physical suffering caused by the subject's actions.

12. The experiment gives the subject little time for reflection. The conflict comes on rapidly. It is only minutes after the subject has been seated before the shock generator that the victim begins his protests. Moreover, the subject perceives that he has gone through but two-thirds of the shock levels at the time the subject's first protests are heard. Thus he understands that the conflict will have a persistent aspect to it, and may well become more intense as increasingly more powerful shocks are required. The rapidity with which the conflict descends on the subject, and his realization that it is predictably recurrent may well be sources of tension to him.

13. At a more general level, the conflict stems

from the opposition of two deeply ingrained behavior dispositions: first, the disposition not to harm other people, and second, the tendency to obey those whom we perceive to be legitimate authorities.

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⁴A series of recently completed experiments employing the obedience paradigm is reported in Milgram (1964).

The Milgram Paradigm After 35 Years: Some Things We Now Know About Obedience to Authority¹

THOMAS BLASS²

University of Maryland Baltimore County

Guided by the belief that we cannot make broad extrapolations from the obedience studies without first firmly establishing what has and has not been found using the paradigm itself, this article draws on 35 years of accumulated research and writings on the obedience paradigm to present a status report on the following salient questions and issues surrounding obedience to authority: (a) How should we construe the nature of authority in the obedience experiment? (b) Do predictions of those unfamiliar with the obedience experiment underestimate the actual obedience rates? (c) Are there gender differences in obedience? and (d) Have obedience rates changed over time?

What have I learned from my investigations? First, that the conflict between conscience and authority is not wholly a philosophical or moral issue. Many of the subjects felt, at the philosophical level of values, that they ought not to go on, but they were unable to translate this conviction into action.

It may be that we are puppets—puppets controlled by the strings of society. But at least we are puppets with perception, with awareness. And perhaps our awareness is the first step to our liberation. (Milgram, 1974b, p. 568)

SAFER: . . . are you suggesting that—that it could happen here?

MILGRAM: I would say, on the basis of having observed a thousand people in the experiment and having my own intuition shaped and informed by these experiments, that if a system of death camps were set up in the United States of the sort we had seen in

¹Quotes from letters and most information given without citation are from the Stanley Milgram Papers, Yale University Archives. I want to express my thanks to Annamarie Krackow for her help with some of the analyses presented in this article.

²Correspondence concerning this article should be addressed to Thomas Blass, Department of Psychology, University of Maryland Baltimore County, 1000 Hilltop Circle, Baltimore, MD 21250. e-mail: blsass@umbc2.umbc.edu.

Nazi Germany, one would be able to find sufficient personnel for those camps in any medium-sized American town. (CBS News, *Sixty Minutes*, March 31, 1979)

Milgram conducted his obedience studies early in his professional career, and then went on to apply his innovative touch to a variety of other phenomena, such as the small-world method and the effects of televised antisocial behavior. Yet, clearly, the obedience work has overshadowed his other research—it remains his best-known and most widely discussed work. Of the approximately 140 invited speeches and colloquia he gave during his lifetime, more than one third dealt, directly or indirectly, with obedience. Milgram was still giving invited colloquia on the topic in 1984, the year he died—22 years after he completed them—one at LaSalle College on April 7, and the other at the University of Tennessee at Martin on April 26. In fact, it is somewhat ironic that his very last publications, both appearing posthumously in 1987, dealt with obedience. One was in the *Concise Encyclopedia of Psychology* (Milgram, 1987a), and the other in the *Oxford Companion to the Mind* (Milgram, 1987b).

Given the widespread familiarity with Milgram's obedience studies, it should not be surprising to find the obedience research discussed or referred to in publications as diverse as the *Archives of Internal Medicine* (Green, Mitchell, Stocking, Cassel, & Siegler, 1996) and the *Indian Journal of the History of Science* (Laurent, 1987), nor to see it brought into discussions of topics as wide-ranging as business ethics (Browne, Kubasek, & Giampetro-Meyer, 1995/1996; Ferrell & Gardiner, 1991; MacLellan & Dobson, 1997), military psychology (Guimond, Kwak, & Langevin, 1994; Spector, 1978), economics (Anderson & Block, 1995), Holocaust studies (e.g., Browning, 1992; Goldhagen, 1996; Katz, 1993), philosophy (Assiter, 1998; Morelli, 1983), and law (Koh, 1997). Perhaps it should not even be surprising to find it in the title of a song ("We Do What We're Told—Milgram's 37" by rock musician Peter Gabriel on his 1986 album titled *So*) or featured prominently in a French film, *I Comme I care* [I as in Icarus], starring Yves Montand. The obedience experiments were the focus of the Fall 1995 issue of the *Journal of Social Issues*, and they continue to fascinate the reading public (e.g., French, 1997; Masters, 1996).

The interest generated by the obedience research has crossed not only disciplinary boundaries but language barriers as well. Early on, Milgram's (1965b) article "Some Conditions of Obedience and Disobedience to Authority" appeared in translation in a German psychology journal in 1966 (Milgram, 1966) and in Hebrew in the Israeli journal *Megamot* in 1967 (Milgram, 1967). The book *Obedience to Authority: An Experimental View* (Milgram, 1974a) has been translated into 11 languages. During the past few years, a social psychologist at the Russian State University of the Humanities, Alexander Voronov, has been introducing Milgram's work to Russian audiences through his teaching, newspaper articles

(e.g., Voronov, 1993), and Milgram's (1965a) documentary film, *Obedience*, with a Russian voice-over added.

The obedience research is clearly among the best-known and most widely discussed work in the social sciences. Undoubtedly, an important reason for this is that it has been a source of usable insights and lessons for both self and society. As Milgram's colleague, Irwin Katz, described the obedience studies at Milgram's funeral,

After two decades of critical scrutiny and discussion, they remain one of the most singular, most penetrating, and most disturbing inquiries into human conduct that modern psychology has produced in this century. Those of us who presume to have knowledge of man are still perplexed by his findings, with their frightful implications for society. (Katz, 1984)

Purpose

The purpose of the present article is to provide a detailed examination of a number of salient questions and issues surrounding the Milgram obedience experiments which are still in need of systematic attention. (For reviews and analyses related to other aspects of the obedience paradigm and of other facets of Milgram's life and work, the reader is referred to Blass, 1991, 1992b, 1993, 1996b; see also Miller, 1986.) Specifically, I will draw on about 35 years of accumulated research and writings on the obedience paradigm to present a status report on four questions and issues. While each of the questions and issues could be addressed independently of the others, what unites them is that, in their totality, their answers should help to advance our knowledge of research using the Milgram paradigm and its implications.

First, I will address the question of how to construe the nature of authority in the obedience experiment. This is a fundamentally important question, since the kinds of authority-subordinate relationships to which the findings from the obedience experiments are generalizable hinge on the answer to that question. In pursuit of that answer, I will review the various views on this question. Then, in an attempt to provide at least an indirect resolution of the conflicting viewpoints, I will present the results of a person-perception experiment I conducted using an edited version of Milgram's (1965a) documentary film, *Obedience*.

Second, I will review the evidence regarding the apparent inability of naive respondents to predict the high degree of obedience Milgram found in his standard conditions. The tendency for those unfamiliar with the obedience experiments to vastly underestimate actual obedience rates reported by Milgram has contributed importantly to the revelatory power of the experiments. The prediction versus outcome dichotomy is also important because, as we will see, it is

closely intertwined with a controversy regarding how to interpret the obedient subjects' behavior—as one representing destructive obedience, as Milgram saw it, or as one involving a more benign view centered on subjects' trust in the experimenter, as represented in Mixon's (1976) approach.

Third, I will present a review of all of the methodological replications of Milgram's standard or baseline conditions which allowed comparisons of males and females in rates of obedience. As will be shown, the totality of the findings of my review are consistent with those of Milgram, although there are a couple of discrepant results which pose a challenge to understanding.

And finally, this article provides an empirical answer to the question of whether or not obedience rates have changed since Milgram first conducted his experiments in 1961-1962. The answer not only has practical usefulness for those of us who often have fielded this question from students when teaching about the obedience experiments, but it has theoretical importance as well: It provides data-based input regarding the validity of Gergen's (1973) *enlightenment effects* notion.

How Should We Construe the Nature of Authority in the Obedience Experiment?

How to characterize the kind of authority embodied by Milgram's experimenter is a fundamentally important question, since the kind of authority-subordinate relationships the experiments have implications for depend on the answer to that question. We will first examine Milgram's view of the authority figure in his experiments, as well as the differing perspectives. Then, I will present the findings from an experiment which provides a rapprochement between the conflicting viewpoints, at least indirectly.

Milgram saw his experimenter as representing a legitimate authority, one who is seen as having a right to issue commands, and to whom one feels an obligation to obey. As Milgram (1974a) put it, "an authority system . . . consists of a minimum of two persons sharing the expectation that one of them has the right to prescribe behavior for the other" (pp. 142-143). He also notes that a legitimate authority is one who is "perceived to be in a position of social control within a given situation" (p. 138) and that "the power of an authority stems not from personal characteristics but from his perceived position in a social structure" (p. 139). And what is it about a legitimate authority that, according to Milgram, enables him to elicit destructive obedience, the kind that bears a kinship to the behavior of a Nazi storm trooper? First is the ability of a legitimate authority to define reality for the person who accepts his or her authority. As Milgram (1974a) put it, "There is a propensity for people to accept definitions of action provided by legitimate authority. That is, although the subject performs the action, he allows authority to define its meaning" (p. 145). Earlier, Milgram (1965b), had made the point even more strongly:

With numbing regularity good people were seen to knuckle under the demands of authority and perform actions that were callous and severe. Men who are in everyday life responsible and decent were seduced by the trappings of authority, by the control of their perceptions, and by the uncritical acceptance of the experimenter's definition of the situation, into performing harsh acts. (p. 74)

The other factor that enables a legitimate authority to evoke destructive obedience, according to Milgram (1974a), is the shift of subjects into a different experiential state—the agentic state—which enables them to relinquish responsibility to the authority and, therefore, to follow his or her orders without regard to their morality. As Milgram (1974a) stated, “The most far-reaching consequence of the agentic shift is that a man feels responsible *to* the authority directing him but feels no responsibility *for* the content of the actions that the authority prescribes” (pp. 145-146).

A main differing perspective on the nature of authority in the obedience experiment is to see him as an expert authority. Morelli (1983), a critic of Milgram, succinctly captures the difference between a legitimate authority and an expert authority via the difference between saying someone is *in* authority (i.e., in charge) or *an* authority (i.e., someone with expertise on some topic).

One of several writers (Greenwood, 1982; Helm & Morelli, 1985; Morelli, 1983; Penner, Hawkins, Dertke, Spector, & Stone, 1973) who expresses the authority-as-expert point of view is Patten (1977), a philosopher, and in so doing, he argues for a distinction between the obedience of a subject in the Milgram experiment and obedience to carry out mass killings. He argues that there is a difference between the type of authority represented by Milgram's experimenter and the kind wielded by a Hitler. The former possesses what Patten calls *expert-command authority*. That is, he is able to command obedience by means of his presumed expertise regarding learning and shock machinery. The latter, more worrisome, kind of authority wields what he calls a *simple-command authority*; namely, whose power to command and exact obedience is based on legal or quasi-legal considerations, not because of any special expertise regarding the task at hand. According to Patten, knowledge about how a person might react to expert-command authority cannot tell us about that individual's behavior in relation to simple-command authority.

Milgram clearly distinguished between his conception of his experimenter as a legitimate authority and authority based on expertise. In an interview conducted by Evans (1976, p. 349), he said “When we talk about a medical authority, we're talking about someone with expertise. That's not quite the same as the kind of authority I was studying, which is someone perceived to have the right to control one's behavior.”

What is interesting about this comment is that there is evidence provided by Milgram himself—though it is anecdotal—that for some of his own subjects, the authority's expertise may have been his salient attribute. In his book, he quotes an exchange between a subject (Mr. Rensaleer) and the experimenter. The subject had just stopped at 255 V, and the experimenter tried to prod him on by saying "There is no permanent tissue damage." Mr. Rensaleer answers, "Yes, but I know what shocks do to you. I'm an electrical engineer, and I have had shocks . . . and you get real shook up by them—especially if you know the next one is coming. I'm sorry" (Milgram, 1974a, p. 51). What this subject seems to be doing is pitting his own expertise against the experimenter's expertise as a way of undermining the latter's power.

It is also worth noting that Milgram was not entirely consistent in his view about the source of his experimenter's power as an authority. Or, more precisely, he seemed to have shifted his position somewhat, later in his career. In 1983, in one of the last things Milgram wrote about obedience before his death, here is what he said in reply to a critical article by Morelli (1983):

In regard to the term *authority*, Morelli states I did not adequately distinguish between the expert knowledge of *an* authority and a person who is *in* authority (in the sense that he occupies an office or position). I fully agree with Morelli that this is an important distinction. . . . Within my own study, how would the experimenter be classified in terms of these two types of authority? As frequently happens, real life is more complex than textbooks: Both components co-exist in one person. The experimenter is both the person "in charge" and is presumed by subjects to possess expert knowledge. One could envision a series of experiments that attempt to empirically disentangle these two elements and I am all for such inquiry. (Milgram, 1983, pp. 191-192)

I recently conducted an experiment which was designed to assess the perceived roles played by expertise and legitimacy in the obedience experiment (Blass, 1992a). I studied my subjects' judgments about obedience rather than their own obedience, so it is not exactly the kind of experiment Milgram had in mind that would "empirically disentangle [the] two elements." Still, I had hoped that it would serve as useful input into the issue. (I should note that there is a study, a doctoral dissertation by Frederick Miller, 1975, that is probably closer to the kind that Milgram had in mind. It pitted the experimenter's expertise and legitimacy against each other in a factorial design, and obedient vs. defiant behavior of the subject served as the dependent variable. However, its focus was on self-inflicted pain, which probably involves different underlying dynamics than obedience to inflict pain on another person.)

The conceptual framework I worked with is French and Raven's (1959) classic formulation regarding the bases of social power. There is a natural affinity between French and Raven's schema and the obedience work, for a couple of reasons. First, many social psychology textbooks discuss them together. Second, Raven (1965; Raven & Rubin, 1983) in later publications actually cites the obedience experiment as an illustration of legitimate power, one of the types of power in French and Raven's system. (For a recent statement on the bases of social power, see Raven, 1992.) For my purposes, French and Raven's conceptualization is also useful because expert power is another one of their categories. A further potential benefit of using French and Raven's schema is that they actually distinguish among six different types of power: besides legitimate and expert power there are reward, coercive, referent, and informational power. So by using French and Raven's framework, we might also learn about the perceived role of other attributes besides expertise and legitimacy as determinants of the authority's power. They are listed, with their meanings, in the first and second columns of Table 1.

The college student participants in the experiment were shown a 12-min videotape, a shortened, edited version of Milgram's (1965a) documentary film, *Obedience*, similar to ones which I have used in other studies focusing on attributional processes in the Milgram experiment (Blass, 1990, 1995). The end of the segment they saw shows a subject, referred to in Milgram's (1974a) book by the pseudonym "Fred Prozi" going through the shock sequence, beginning with his giving 90 V. In the full version of the film, he is shown ending up completely obedient (i.e., giving the 450-V shock). In the edited version shown to my subjects, the tape was stopped immediately after Prozi administered the 180-V shock.

Participants were then asked to indicate why they thought the subject they just saw kept on following the experimenter's instructions and continued to shock the learner. To answer that question, they were provided with a set of six cards, each of which contained a different explanation which was meant to capture a specific social power category. These are listed in the third column of Table 1.³ The subjects were asked to indicate which reason they thought was the most likely one, then the next most likely one, and so on.

Subjects' choices were assigned rank scores, 1 through 6, with the most likely explanation receiving a rank score of 1. The data were analyzed by means of a one-way repeated-measures ANOVA, with social power category as the independent variable and assigned rank as the dependent variable, yielding a highly significant, $F(5, 170) = 42.77, p < .0001$. Dependent t tests, using the Bonferroni test correction, were then conducted to test for differences between pairs of mean

³I am indebted to Forsyth (1987) and Raven and Rubin (1983) for some of the ideas and wording that I used in developing the explanations.

Table 1

Mean Rankings of Bases of Social Power as Explanations for an Obedient Subject's Behavior in the Milgram Experiment

Power categories	Meanings: Subjects are influenced because . . .	Explanation	Mean ranks
Reward	they see the E as a potential source of rewards.	Because the experimenter is a figure of authority, his positive evaluations are especially rewarding, so the subject carries out the experimenter's wishes, thereby hoping to win his approval.	4.46 _b
Coercive	they see the E as a potential source of punishments.	The experimenter urges the subject to continue, using such phrases as "The experiment requires that you go on." For the subject, such phrases seem to warn of negative consequences if he does not continue.	2.71 _a
Legitimate	they believe that the E has a legitimate right to prescribe behavior for them.	Because the experimenter represents the authority of science and the subject agreed to be a participant, he believes that the experimenter has a right to control his actions, and so the subject feels obliged to comply with the experimenter's wishes.	2.40 _a
Referent	they identify with, or like, the E.	The subject has respect and admiration for the experimenter, identifies with him, and would like to be such a person.	5.86 _c
Expert	they perceive the E as having some special knowledge or expertise.	As a scientific expert, the experimenter has the faith and trust of the subject, so when the experimenter tells him that "although the shocks may be painful, they're not dangerous," the subject feels reassured and continues with the procedure.	2.31 _a
Informational	the information the E provides is intrinsically compelling or convincing.	The introductory information, provided by the experimenter, about the goal of the experiment—namely, to learn more about the effect of punishment on memory—convinces the subject that the study has value and, therefore, that his cooperation is important.	3.23 _a

Note. Means sharing a subscript do not differ significantly from each other.

rank scores. The mean rank scores are presented in the last column of Table 1. As can be seen, the expert power explanation was seen as most likely, followed very closely by legitimate power, while coercive power was seen as the third and informational power as the fourth most likely explanation. These differences, however, were not significant. Reward power comes next, and referent power is seen as the least likely reason for the subjects' compliance.

Several conclusions can be drawn from the findings, tempered by the obvious caution that they are based on data from external perceivers about 30 years after the fact, and not from actual participants in the Milgram experiments. First, it is reassuring to know that the experimental authority's two attributes seen as most salient by naive perceivers are the same ones that have been pointed to over the years by more scholarly perspectives; that is, legitimacy and expertise. Second, rather than deciding between legitimacy and expertise, the results suggest that both factors may have combined to give Milgram's experimenter the tremendous power that he had. Third, the fact that the coercive power explanation was ranked relatively high (as the third most likely explanation) is surprising, because it suggests that some subjects may have been reading things into the experimenter's words. Further, it leaves us with the gnawing possibility that many subjects may have been reading other things into the experimenter's words that we don't know about, which may have figured importantly as determinants of their behavior. And, finally, this study affirms—as do other studies (Blass, 1990, 1995, 1996a; Collins & Brief, 1993; Guimond & Kwak, 1995; Miller, Gillen, Schenker, & Radlove, 1974; Pearson, 1992) the value of using person-perception and attributional methodologies to advance our understanding of obedience to authority.

Do Predictions of Those Unfamiliar With the Experiment Underestimate the Actual Obedience Rates?

Milgram (1974a) found that they did, vastly, and much of the revelatory power of the obedience work is based on this contrast between our expectations of very little obedience and the actual result of a majority of subjects obeying in Milgram's standard or baseline conditions. Milgram considered this finding so centrally important that, according to one of his students (interview with Harold Takooshian, June 17, 1993, Fordham University at Lincoln Center), he would become furious if a student suggested that it was all common sense; that if you thought about it, you could have predicted the outcome. Incidentally, this feature of the obedience studies was dramatized very effectively in 1976 in the *Tenth Level*, a made-for-TV movie starring William Shatner, which earned its writer, George Belak, an Honorable Mention in the American Psychological Foundation's 1977 National Media Awards. Specifically, Milgram (1963) found that a group of Yale seniors predicted an obedience rate of 1.2%, while a group of psychiatrists predicted that only 0.125% of subjects would be fully obedient. Here is how he described this latter finding in a letter to E. P. Hollander (September 24, 1962):

Recently I asked a group of 40 Yale psychiatrists to predict the behavior of experimental subjects in a novel, though significant situation. The psychiatrists—although they expressed great certainty in the accuracy of their predictions—were wrong by a factor of 500. Indeed, I have little doubt that a group of charwomen would do as well.

While Milgram's powerful demonstration that normal individuals are much more willing to obey a legitimate authority's orders than one might have thought remains an enduring insight, subsequent studies suggest that it is in need of some qualification, since they show that greater accuracy in predicting the results of an obedience experiment is possible.

In studies using maximum voltages predicted on the 450-V scale as the dependent variable, mean estimates of others' obedience levels have been as high as 276.75 V (Miller et al., 1974), 225 V (Maughan, 1981), and 216 V (Maughan & Higbee, 1981) in specific conditions.

The gap between expected and obtained obedience narrows even more substantially when we consider studies which obtained predictions using obedience rates. Mixon (1971) read participants the Method section from Milgram (1963) and then asked them how "a hypothetical group of 100 American males" would behave. The percentage of subjects predicted to be fully obedient ranged from an average of 33.52% (naive females' estimates) to 44.3% (naive males' estimates). Kaufmann and Kooman (1967) gave subjects descriptions based on Milgram's (1963) procedures and found 27% of them predicting that the "teacher" would continue to the end of the 450-V shock scale. A similar finding was obtained in a more recent study by Guimond et al. (1994) involving a group of Canadian officer candidates. After learning about a baseline obedience experiment (without the outcome) from a short videotape, 23.9% of them predicted full obedience by other Canadians. Furthermore, Mixon (1971) was able to get variations in predicted obedience by systematically modifying the details about the procedure that was read to subjects. These ranged from 0% of the subjects predicting complete obedience when the description they read clearly indicated that the learner was in danger of being harmed to 90% when indications of possible harm were minimized. Taken together, these findings not only point to greater accuracy in perceivers' predictions about obedience, but also to a different way of understanding underestimations of obedience.

An influential perspective on underestimations of obedience has been that of Ross (1977). According to his view, in attempting to predict obedience, people erroneously overlook the determining influence of the situation—the power of the authority—and place too much weight on the personal dispositions of the "teacher," exemplifying a tendency he labeled the *fundamental attribution error*. Mixon's (1971) findings suggest, however, that the discrepancy between

predictions and findings takes place not because people do not give enough weight to the immediate situation, but because those who are asked to make predictions, on the one hand, and actual subjects in an obedience experiment, on the other hand, may be responding to different situations: The descriptions given in prediction tasks may convey a procedure that is potentially more harmful for the learner than the real subject in an obedience experiment typically found it to be. Thus, for example, Bierbrauer (1974) had participants learn about the obedience experiment by either watching, or serving as the "teacher" in, a reenactment of an experimental session which ends in complete obedience. Across two experiments and a number of conditions, his participants' subsequent estimates of the percentage of subjects who would give the 450-V shock averaged 11.5%.⁴ In introducing the reenactment, however, Bierbrauer (1974) told his subjects that "Professor Milgram wanted to see whether subjects would obey an experimenter's instructions to deliver painful and *potentially dangerous* electric shocks to one of their peers" (p. 78; italics added). But, as Mixon (1976) has argued, both the scientific context and the experimenter's reassurances that the shocks may be painful but not dangerous probably led the actual participants in Milgram's experiments to anticipate that the "learner" would not be harmed.

In other words, Mixon's (1989) view of subjects' behavior in the obedience experiment is a more benign one than is Milgram's. If Mixon is right, then was Milgram wrong in referring to his obedient subjects' actions as "destructive"? This is how Mixon sees it, and for a long time, I saw Milgram's and Mixon's approaches as conflicting and irreconcilable. But then recently, in a review of Mixon's (1989) book, Hamilton (1992) presented a persuasive and insightful perspective that brings the implications of Mixon's viewpoint closer to Milgram's:

I believe . . . that Milgram's work has a value beyond that accorded it in Mixon's account. True, perhaps Milgram's subjects suspended their doubts and disbeliefs in going along with experimental commands. Perhaps they did not really believe that damage and death could or should ensue from their actions. So what; they still did them. I see the actions of Milgram's subjects as more closely analogous to those of corporate employees who produce unsafe products and believe that the company could not really be endangering consumers just to make a profit, than to the actions of a military subordinate ordered to shoot civilians. The fact remains that these employees—or Milgram's subjects—perform the deeds they are asked to perform. (Hamilton, 1992, p. 1313)

⁴This number was computed by averaging across the condition means in Tables 2 and E-4 in Bierbrauer (1974).

Table 2

Studies Using the Milgram Paradigm Which Have Compared Male and Female

Author and year	Country	Gender	Num-ber of sub-jects	Author's name for or description of condition (when more than 1 in study)	Equivalent Milgram condition(s)	Percentage fully obedient
Milgram (1962)	United States	F	40	8. Women as subjects	N/A	65
Edwards et al. (1969)	South Africa	M F	10 6	—	2. Voice feed-back	87.5
Bock & Warren (1972)	United States	M F	17 13	—	5. New baseline	?
Bock (1972)	United States	M/F	25	Scientific authority	5. New baseline	40
Kilham & Mann (1974)	Australia	M F	25 25	Executant	2. Voice feed-back	28
Costanzo (1976)	United States	M F	48 48	"Retaliation" and "nonretaliation" conditions combined	1. Remote	81
Shanab & Yahya (1977)	Jordan	M F	48 48	Experimental	1 and 2. Remote and voice-feedback combination	73
Shanab & Yahya (1978)	Jordan	M F	12 12	Experimental	1 and 2. Remote and voice-feedback combination	62.5
Miranda, Caballero, Gomez, & Zamorano (1981)	Spain	M F	12 12	"Not watching" and "watching" conditions combined	2. Voice feed-back 3. Proximity	50
Schurz (1985)	Austria	M F	24 32	—	1. Remote	80

Subjects on Level of Obedience

Gender of experimenter	Subject gender differences		Remarks
	Yes/no	Percentage fully obedient	
M	No	—	Compared to Milgram's Condition 5 (same condition using 40 male subjects) in which 65% were fully obedient. The data on women first appeared in Milgram (1974a), but all conditions were completed between the summer of 1961 and May 1962. The women's condition was carried out in 1962. Thus, the 1962 in the citation reflects the completion date, not the publication date.
F	No	—	The experimenter, a 19-year-old female, as well as her two male "technician" assistants, were college students. See also the note about this experiment in the Appendix.
M	No	—	Percentage of fully obedient subjects not reported. The measure of obedience was maximum shock level given.
M	No	—	Lack of subject gender differences reported only for total subject sample, that is, across three conditions, of which the scientific authority condition was one.
M	Yes	M 40% F 16%	Subjects assumed role of executants taking orders to shock from confederate transmitters who, they thought, were also subjects. Paired male executant with male learner and female executant with female learner.
F	No	—	Subject and learner paired in four conditions: M-M, M-F, F-M, F-F.
F	No	—	Subjects were children aged 6 to 16. Subject and learner paired in two conditions: M-M, F-F.
F	No	—	Subject and learner paired in two conditions: M-M, F-F.
M/F	No	—	When subjects were male, experimenter and learner were male. When subjects were female, experimenter and learner were female. The dependent variable was highest shock given, rather than percentage fully obedient. However, a graph in the report reveals indirectly that at least 50% were fully obedient.
F	No	—	Learner was female. Stimulus: "ultrasound waves" supposedly damaging to skin at higher intensities.

Are There Gender Differences in Obedience?

Although almost all of his subjects were men, Milgram had one condition (Experiment 8 in Milgram, 1974a) in which the participants were women. The result was exactly the same rate of obedience—65%—as for men in the comparable condition (Experiment 5). I found nine methodological replications in the literature which had both male and female participants. Consistent with Milgram's own findings, eight out of nine of these studies found no gender differences (Table 2).

As can be seen in Table 2, the one exception is a study by Kilham and Mann (1974), conducted in Australia, in which they found the obedience rate in men (40%) to be significantly higher than among women (16%). (The Kilham & Mann study is also noteworthy for another reason: Its overall rate of obedience—28%—is the lowest reported in the literature for a standard obedience condition.)

It is also relevant to mention two other studies in this context because they pose a challenge to understanding, though they were not included in Table 2: the first, because it lacked a comparison group of males; the second, because it used a real victim, an animal "learner." Ring, Wallston, and Corey (1970) conducted a voice-feedback replication using 57 female subjects. While the main focus of this study was the relative effectiveness of different debriefing methods, an important finding was that 91% of their subjects were fully obedient, the highest rate for a standard condition reported in the obedience literature. Sheridan and King (1972) conducted a unique Milgram-type study using a puppy as the "learner." Even though the cute puppy was visible to the subjects and enough actual shock was delivered to cause the puppy to yelp and jump in pain, 100% of the female subjects were fully obedient, while only 54% of the males were obedient.

Milgram (1974a) had also reported that, although the level of obedience in women was the same as in men, the self-reported tension of the obedient women was higher than among 20 groups of obedient male subjects. This result finds support in a study by Shanab and Yahya (1977) involving Jordanian children and adolescents. They reported that females were more likely to show visible signs of tension than were males.

Two consistencies emerge from the studies presented in this section. First, it is quite remarkable that 9 out of 10 comparisons (Table 2) showed no gender differences in obedience, despite the existence of between-experiment differences on such factors as country where the experiment was conducted, gender of experimenter, gender of learner, and specific details of the experimental procedures. Eagly's (1978) seminal review of gender differences in influenceability showed that the widely held assumption about women being generally more influenceable than men was wrong. She found no gender differences in the majority of the studies she reviewed. A tendency for women to be more susceptible to influence than men showed up in only one domain—the Asch-type (Asch, 1956) group-

pressure conformity situation, in which 34% of the studies found women to be significantly more conforming than men. Her review, although mentioning the Milgram studies and two replications that looked at gender differences (Kilham & Mann, 1974; Sheridan & King, 1972), did not include a systematic review of studies of gender differences in the obedience paradigm. The findings reported here complement Eagly's review by identifying yet another social influence paradigm in which the majority of studies show no gender differences.

Second, the consistency of Milgram's findings on gender differences in self-reported tension is also quite noteworthy, with obedient women reporting greater tension than the obedient males in 20 conditions. These findings have wide-ranging implications beyond the question of gender differences. In particular, the fact that the same observable behaviors—identical rates of obedience (65%), in men and women in a baseline condition—were accompanied by different levels of nervousness should alert us to the importance of trying to identify the underlying processes involved in acts of obedience and defiance, be they those involving the Milgram paradigm or not.

Have Obedience Rates Changed Over Time?

One of the questions I have posed to my social psychology classes when presenting the obedience studies is what they think the results would be if the research were conducted today. I collected systematic data relating to this and several other questions from students in 11 social psychology classes from 1983 to 1990. The results were as follows: 40% predicted less obedience today, 39% predicted the same amount, and only 11% predicted an increase in obedience (Blass & Krackow, 1991).

After completing this analysis, it occurred to me that it would be even more interesting to determine whether or not a change in obedience tendencies over time could be detected in the actual outcomes of obedience studies. So I took Milgram's standard or baseline conditions (i.e., in which the learner is physically separated from and not visible to the subject: Experiments 1, 2, 5, 6, 8, and 10 in Milgram, 1974a) and all of the methodological replications of these experiments carried out by others (there were 14 of these), and correlated the rank order of the year of publication of the study with the rank order of its obedience rate. The studies spanned a period of 22 years, from 1963 to 1985, which is the year of publication of the last methodological replication that I have found (Schurz, 1985). Although levels of obedience across studies ranged from a low of 28% (Kilham & Mann, 1974) to a high of 91% (Ring et al., 1970), there was no systematic relationship between when a study was conducted and the amount of obedience obtained: The Spearman rank-order correlation coefficient (r_s) was .002. A second correlation was performed, this time adding Milgram's Proximity condition (Experiment 3) and three proximity-condition replications by other

investigators (for a total of 24 conditions or studies). These had been excluded from the first correlation because the rate of obedience in Milgram's Experiment 3 was significantly lower than those of his Experiments 1, 2, 5, and 8 (Blass, 1991), suggesting that methodologically and experientially they were distinct. However, as it turns out, the addition of the Proximity studies leaves the correlation virtually unchanged: $r_s = -.008$. (See the Appendix for a listing of studies and findings which were used in the correlational analyses.)

An important implication of the findings of these correlational analyses is that they provide evidence—at least, indirectly—against the operation of enlightenment effects, which had been proposed by Gergen (1973). Gergen had argued that “sophistication as to psychological principles liberates one from their behavioral implications” (p. 313). If Gergen is right, the later studies should have found less obedience than the earlier ones since, with the longer passage of time, the participants in the more recent studies would have had more of a chance to hear about Milgram's work and thereby become enlightened about, and liberated from, the unwanted demands of authority.

Two unpublished studies attempted to provide more direct tests regarding the operation of enlightenment effects using the Milgram paradigm—one by Brant (1978) and the other by Shelton (1982). Brant had college undergraduates, who had first been familiarized with the obedience studies, participate in a “learning” experiment, similar to Milgram's Experiment 11, in which they could choose any shock level on a 390-V “shock” generator whenever the learner made an error. Brant reports that only 4 subjects out of 44 refused to participate in the study after they heard the instructions—a finding which he interprets as “seriously call[ing] into question” (p. 53) Gergen's thesis. However, the study suffers from a serious methodological flaw, precluding any firm conclusions about enlightenment effects: It is not clear how many of the subjects, if any, actually knew about the obedience studies prior to their own participation. This is because the attempt to inform them about it took the following form:

Prior to their participation, subjects had been assigned readings in their classes concerning the obedience research as well as other psychological findings in conjunction with their coursework. In addition, these students had been lectured to on topics relevant to this investigation. (Brant, 1978, p. 19)

There was no attempt, however, to ascertain whether or not subjects had actually read the assigned readings or attended the relevant lectures.

Shelton's (1982) attempt to determine the validity of Gergen's claim that the acquisition of psychological information can change a person's behavior was not only a methodological improvement over Brant's study but also was quite clever in its conception. First, she gave all of her subjects a detailed synopsis of the

obedience experiment to read and then asked them a set of questions about what they had read. She then asked them to serve as experimenters in a similar "learning" experiment. Their job was to oversee a subject (the teacher) who was supposed to teach a verbal-learning task to another subject (the learner) by using increasing voltages of shock as punishment on each subsequent mistake. The subject (experimenter) was told that the learner was a confederate, but unbeknownst to the former, the teacher was also a confederate, who, as the shock levels and the learner's expressions of pain increased, "expressed uneasiness, then became quite anxious, angry, on the verge of tears; cursed, complained of stomach pains, asked for a glass of water, and pleaded with the experimenter to stop the session . . ." (p. 31). In spite of this, 22 out of 24 subjects continued to the end, commanding the teacher to keep increasing the shock to the maximum 450-V level. Apparently, subjects could not draw a parallel between their obedience to Shelton and the teacher's obedience to them.

How do we reconcile a finding like Shelton's with the life-changing testimonials of individuals who found the strength to resist the unwanted demands of authority after participating in, or otherwise learning about, the obedience experiments (e.g., Appendix I in Milgram, 1974a)? One possibility is suggested in an insightful letter written to Milgram in April 1982, by a former participant in a Milgram-type experiment at the University of Minnesota in 1967. He wrote: "I'm writing to thank you for making a major contribution to my understanding of myself and of the meaning of the values I have." He wrote that he learned a number of things from his participation in the experiment, one of which was "that it is easier for me (although hardly simple) to recognize and avoid situations in which authority and obedience play significant roles (e.g., the military, many government and business organizations) than it is to defy authority within such situations." That is, contrary to what is implied by Gergen's enlightenment-effects notion, knowledge does not or cannot always lead to action. Being enlightened about the unexpected power of authority may help a person to stay away from an authority-dominated situation, but once he or she is already in such a situation, knowledge of the drastic degree of obedience that authorities are capable of eliciting does not necessarily help to free the individual from the grip of the forces operating in that concrete situation; that is, to defy the authority in charge.

Summary and Conclusions

In this article I set out to present a status report on four important questions and issues surrounding the obedience paradigm, grounded in systematic analysis—something which had heretofore not been done with these questions and issues. My analyses involved a variety of methods: literature reviews, a person-perception experiment, and correlational analyses. On the basis of these analyses, I believe that the following conclusions are called for. First, in all likelihood,

Milgram's experimental authority was perceived by subjects as embodying a combination of a legitimate authority and a scientific expert. Second, a review of prediction studies found that while naive subjects generally underestimate actual obedience rates, the gap between estimated and actual obedience rates is often quite a bit smaller than what Milgram found. Third, with one exception, in all studies permitting a comparison between male and female subjects, no gender differences in obedience have been found. And fourth, rates of obedience show no systematic change over time: Two correlational analyses between year of publication and obedience outcome showed no relationship whatsoever between when a study was conducted and how much obedience occurred. In each case, the wider implications of each of these findings were also discussed.

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Appendix

List of Obedience Studies and Their Findings (in Obedience Rates) Used in the Correlational Analyses Reported in the Article

Study	Country	Obedience rate (%)
Milgram (1963) ^a	United States	
Exp. 1		65
Exp. 2		62.5
*Exp. 3		40
Exp. 5		65
Exp. 6		50
Exp. 8		65
Exp. 10		47.5
Holland (1967)	United States	75
*Ancona & Pareyson (1968)	Italy	85
Rosenhan (1969)	United States	85
*Podd (1970) ^b	United States	31
Edwards, Franks, Friedgood, Lobban, & Mackay (1969) ^c	South Africa	87.5
Ring, Wallston, & Corey (1970)	United States	91
Mantell (1971)	West Germany	85
Bock (1972)	United States	40
Powers & Geen (1972)	United States	83
Rogers	United States	37
Kilham & Mann (1974)	Australia	28
Shalala (1974)	United States	30
Costanzo (1976)	United States	81
Shanab & Yahya (1977)	Jordan	73
Shanab & Yahya (1978)	Jordan	62.5
*Miranda, Caballero, Gomez, & Zamorano (1980)	Spain	50
Schurz (1985)	Austria	80

Note. Studies preceded by an asterisk were included in the second, but not the first, correlation. (See the body of the article for an explanation.) Some studies listed consist of more than one condition. In such cases, the obedience rate reported is for the condition that represents the methodological replication of Milgram's standard or proximity conditions (i.e., Experiments 1, 2, 3, 5, 6, 8, or 10 in Milgram, 1974a).

^aAlthough the numbers designating Milgram's experiments are the ones he used in his book (Milgram, 1974a), all of his obedience experiments (other than pilot work) were conducted between the summer of 1961 and the end of May 1962. In the correlational analyses, they were all designated by the year 1963, the year of the first publication of his obedience findings. ^bThe obedience rate found by Podd (1970) does not appear in his dissertation, but was provided by him in a personal communication. ^cThe study by Edwards et al. (1969) was conducted by third-year psychology majors for a course in Experimental Social Psychology at the University of Witwatersrand in Johannesburg, South Africa. Their instructor, L. Melamed, sent a copy of the report to Milgram on October 23, 1969. In his book, Milgram (1974a) mentions South Africa as one of the foreign countries where replications of the obedience experiments had been conducted, but gave no reference for it. Since in searching the literature I have not found any other South African obedience study, this is the one that, in all likelihood, Milgram had in mind.

Communication in Context: Effects of Speaker Status on the Comprehension of Indirect Requests

Thomas Holtgraves

Four experiments were conducted to examine how a speaker's status can affect the comprehension of conventional and nonconventional indirect requests. The processing of conventional forms was not affected by the speaker's relative status, and consistent with past research (R. W. Gibbs, 1983), these forms were recognized quickly and without the hearer recognizing and then rejecting the literal meaning of the remark. In contrast, processing of nonconventional forms was affected by speaker status. When the interactants were equal in status, the comprehension of nonconventional forms was time-consuming and involved activation of the remark's literal meaning. This did not occur when the speaker was higher in status than the hearer. Results illustrate the role played by the interpersonal context in the comprehension of certain indirect requests.

Communication is often indirect. Sometimes we hint for what we want rather than directly asking for it. At times it is possible to criticize another with a (faint) compliment, and sometimes our criticisms may be compliments. Indirectness of this sort quite often occurs in the service of face management (P. Brown & Levinson, 1987; Goffman, 1967). Thus, we are able to impose, criticize, refuse, and so on, in a manner that allows for the mutual preservation of face, or a desirable public image. There is now, in fact, considerable research delineating the manner in which the production of indirectness (as a form of politeness) is motivated by face management concerns and the variables (e.g., power) that affect it (P. Brown & Levinson, 1987; R. Brown & Gilman, 1989; Holtgraves, 1992; Holtgraves & Yang, 1990, 1992).

The fact that people are often indirect raises an interesting question regarding language use. How is it that hearers are able to understand what a speaker means with an indirect request? For example, how do hearers decide that the utterance "It's cold in here" is a request to shut the window rather than a comment on the room temperature? In contrast to the production of indirectness, there has been much less theoretical and empirical research on the interpersonal factors involved in the comprehension of indirectness. Language use, however, is a social activity (Clark, 1985), and the manner in which hearers interpret some indirect requests cannot be understood without reference to the interpersonal context in which the communication occurs.

Conversational Implicatures and Indirect Speech Acts

Probably the most influential proposal for explaining the comprehension of indirectness is Grice's (1975) theory of

conversational implicature. The essence of this model is that interactants mutually assume adherence to the cooperative principle, or expectation that a speaker's utterances will be appropriate for the conversation of which it is a part. The cooperative principle involves four maxims: quantity (be as informative as required), quality (say what is true), manner (be clear), and relevance (make your utterances relevant to the exchange). Rather than being a normative prescription for how speakers should converse, the cooperative principle is a generalized expectation that guides both the production of remarks and the interpretation of what a speaker means with a remark. Specifically, if a speaker violates the cooperative principle and the hearer assumes the speaker is being cooperative, then the hearer will assume the speaker means more than what is said and so forgo a literal reading of the remark and instead generate a conversational implicature.

Imagine, for example, that while sitting in a room that is very cold because of an open window, one person says to another: "It's very cold in here." The remark, given the context, can be regarded as stating the obvious, a violation of the quantity maxim. An assumption by the hearer that the speaker is being cooperative should then result in a conversational implicature, namely that the speaker is requesting the hearer to shut the window. In this model, the recognition of any indirect request is assumed to follow a similar process. Thus, in this context the remark "Could you shut the window?" is also a violation of the quantity maxim, and so the hearer should recognize that the speaker is not inquiring about her ability to shut the window, but is instead requesting her to shut the window.¹

The making of a conversational implicature is similar to the process involved in the recognition of the primary illocutionary act (the speaker's intention) performed with an indirect speech act (Searle, 1975). For example, a literal reading of "Could you shut the window?" results in the illocutionary act

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Correspondence concerning this article should be addressed to Thomas Holtgraves, Department of Psychological Science, Ball State University, Muncie, Indiana 47306. Electronic mail may be sent to 00t0holtgrav@bsuvc.bsu.edu.

¹ There is a certain similarity between this model and the interpretive process articulated by conversation analytic researchers (Drew, 1989; Heritage, 1984; Schegloff, 1968). That is, sequencing rules are assumed to constrain the production of turns (e.g., answers are expected to follow questions) and deviations from these rules are grounds for making an inference.

of asking a question, although the primary illocutionary act is that of making a request. The process by which a hearer recognizes that an ulterior act is performed is assumed to be based on the cooperative principle; the literal point is not relevant.²

Comprehension in the Grice (1975) and Searle (1975) models thus involves multiple stages (Searle outlines 10 distinct steps). At the least, a hearer must first recognize the literal reading of the remark, decide that the literal reading is not appropriate for the context, and then generate a reasonable interpretation that makes sense in context.

Empirical research on both models has produced mixed results. On the one hand, some early research provided support for certain aspects of each model. For example, Clark and Lucy (1975) found that subjects took longer to comprehend sentences with a negative literal reading (e.g., "Shouldn't you open the door?") than sentences with a positive literal reading (e.g., "You should open the door"), even though the sentences had the same conveyed or indirect reading (i.e., a request to open the door). Consistent with both models, this finding suggests that subjects were recognizing the literal readings of the sentences before their recognition of the conveyed meaning. Similar results with young children (ages 4 to 7) have also been obtained (Carrell, 1981).

Additional support for the models comes from research on perceptions of politeness (Clark & Schunk, 1980) and responses to polite requests (Clark, 1979). The politeness of a request is based primarily on the literal meaning rather than the conveyed meaning. For example, "Could you shut the door?" is more polite than "I want you to shut the door" even though both have the same conveyed meaning. The fact that politeness judgments vary as a function of literal wording (Clark & Schunk, 1980; Holtgraves & Yang, 1990) indicates that hearers are attending to the literal wording.³

On the other hand, both psycholinguists (Ervin-Tripp, Strage, Lampert, & Bell, 1987) and artificial intelligence researchers (Cohen & Levesque, 1990) have recently argued that hearers will frequently recognize a speaker's meaning without engaging in this type of inference process. More important, Gibbs (1983) has demonstrated that some indirect requests are in fact idiomatic (e.g., "Can you pass the salt?") and comprehended quickly without the relatively time-consuming process suggested by the models of Grice (1975) and Searle (1975). In two priming studies, Gibbs found that indirect requests facilitated subsequent sentence verification judgments of indirect readings of the request, but they did not facilitate judgments of the literal readings. This indicates that (for these requests) hearers do not need to compute a literal reading of a remark before determining the indirect reading (the heart of the Grice and Searle models). Rather, speaker meaning (i.e., the indirect reading) is computed first and represented independent of the literal reading.

Similar results have been found for the comprehension of idiomatic expressions (Gibbs, 1980; Ortony, Schallert, Reynolds, & Antos, 1978; Schweigert & Moates, 1988). That is, for idiomatic expressions such as "He's singing a different tune," people take longer to understand the literal meaning (e.g., He's not singing the same song) than they do to comprehend the indirect meaning (e.g., He's changed his mind) (Gibbs, 1980).

Overall, then, there has been only mixed support for the models proposed by Grice (1975) and Searle (1975) regarding the comprehension of indirect requests, and so it is not clear when (if ever) a Gricean inference process is involved in the comprehension of these speech acts.⁴ This is due, in part, to a failure to investigate fully the role played by the interpersonal context in the processing of indirect requests.

Importantly, some researchers have recently demonstrated how features of the communication context that play a role in the production of indirect requests will also play a role in the comprehension of indirect requests. Specifically, Gibbs (1981, 1986) and Francik and Clark (1985) have provided evidence that speakers, in formulating their requests, attempt to specify for the hearer the greatest potential obstacle to compliance with the request. For example, in requesting the time from a stranger, the greatest potential obstacle often will be whether the stranger knows the time. As a result, speakers will tend to specify this feature of the context in their requests (e.g., "Do you know the time?" or "Do you have a watch?"). If speakers routinely phrase their requests so that the greatest potential obstacle is specified, then hearers should be similarly attuned to this aspect of the context. That is, hearers and speakers should coordinate to understand one another (Clark, 1985). Consistent with this reasoning, Gibbs (1986) found that requests that specified an appropriate obstacle were comprehended more quickly than requests that specified an inappropriate obstacle.

The purpose of the present research was to extend this line of inquiry by examining the role of one feature of the interpersonal context, speaker status, in the processing of indirect requests. It seems likely that hearer-speaker coordination will extend to the interpersonal domain, and that those interpersonal variables affecting the production of indirect requests will also play a role in the comprehension of indirect requests. Speaker status was chosen for investigation because previous research has demonstrated its impact on the production of indirectness (R. Brown & Gilman, 1989; Holtgraves & Yang, 1990, 1992).

Experiment 1

The purpose of this experiment was to examine when and how knowledge of a speaker's status would affect the process-

² Note, however, that for Searle (1975), indirect speech acts always involve different illocutionary acts, such as performing a request by asking a question. Conversational implicatures, however, can involve the same illocutionary act (that has different interpretations).

³ Note, however, that this may occur simultaneously with the recognition of the conveyed meaning, and not necessarily before the recognition of the conveyed meaning (as suggested by the Grice, 1975, and Searle, 1975, models). Moreover, it is possible that request forms have conventional politeness values that will affect politeness judgments without a recognition of the literal meaning of the remark.

⁴ In this article I use the term *Gricean inference process* in a restricted sense to refer to the comprehension of a conveyed meaning by first recognizing and then rejecting the literal meaning in favor of the conveyed meaning. Nothing is implied with this phrase regarding other inference processes that may be necessary for the recognition of speaker meaning (e.g., Grice, 1957).

ing of indirect requests. In general, when an utterance has multiple interpretations in context, knowledge that a speaker is high status should serve as a cue for the disambiguation of the speaker's intent (Holtgraves, Srull, & Socall, 1989). This is because the high-status person in the dyad has the right to direct the actions of the low-status person; other things being equal, no such right exists in an equal status dyad. Thus, because high-status people usually direct the actions of others, hearers should tend to recognize fairly quickly that the utterance is being used as a directive.

Speaker status, however, should not always play a role in the comprehension process; its impact should be mediated by the conventionality of the utterance. Although there is some disagreement regarding this issue, conventional indirect requests are assumed to have the following features: (a) They can be performed by asserting or questioning the felicity conditions associated with requesting, such as the hearer's ability to perform the requested act (e.g., "Could you shut the door?") (Gordon & Lakoff, 1975; Searle, 1975); (b) the imperative (e.g., shut the door) is part of the utterance (Ervin-Tripp, 1977); and (c) the word *please* can be inserted within the utterance (e.g., "Could you please shut the door?").⁵ Conventional indirect requests (as defined here) are relatively unambiguous, and it is probable that the conveyed meaning of these forms will be recognized quickly and without the processing of the literal meaning. If this is the case, then speaker status should not play a role in the comprehension process. In other words, regardless of who says it and where it is said, people should almost always recognize quickly that "Could you shut the door?" is a request.

In contrast, nonconventional forms are more ambiguous and, as a result, speaker status should affect the manner in which they are comprehended. There are probably an unlimited number of nonconventional forms that can be used for performing a request indirectly, but there are few principled accounts of the forms they can take. One form that appears to be relatively common involves the following principle: A speaker can perform a request by asserting (or questioning) the existence of a negative state (or state that the hearer can infer is negative) if there is some action that the hearer can perform to remedy the negative state. For example, in the appropriate context "It's noisy in here" or "Isn't it noisy in here?" can be used as a request to shut a door or window. This form, which I refer to as a *negative state remark*, differs from conventional indirect requests on each of the above three criteria (they do not contain the imperative, the word *please* cannot be inserted, and they are not related (at least directly) to the felicity conditions for requesting).

To recognize the conveyed request of a negative state remark, it seems likely that the hearer must first recognize the literal reading of the remark and then decide that this reading is not appropriate in this context. Rejection of the literal meaning is not automatic (it may not occur, in fact) and will depend, in part, on whether there is a more reasonable reading available (cf. Sperber & Wilson, 1986). For example, to interpret a negative state remark (e.g., "It's noisy in here") as a request, a hearer will need to know that a negative state (e.g., a noisy room) is undesirable (and therefore that the speaker

does not want a noisy room) and that performing some action (e.g., shutting the door) will eliminate the negative state.

Speaker status should affect both the rejection of the literal reading and the ultimate recognition of the request interpretation. High-status people frequently direct the actions (and hence make requests) of others. As a result, knowledge that a speaker is high status should provide contextual information that nonliteral readings of the remark are possible. Awareness of an alternative interpretation should then facilitate rejection of the literal reading and guide the hearer to a directive interpretation.

Subjects in Experiment 1 read descriptions of situations in which a speaker (who was either higher than or equal to the status of the interlocutor) made a request with either a conventional indirect request or a negative state remark. Subjects' perceptions of the meaning of the remarks, the speed with which they comprehended the remarks, and their subsequent memory for the wording of the remarks were assessed. There were two general predictions. First, because of their greater ambiguity, negative state remarks should be more difficult to comprehend than conventional indirect requests. Specifically, negative state remarks should be comprehended more slowly, and recognized as requests less frequently, than conventional indirect requests. This differential processing should have a corresponding effect on subsequent memory. Evidence indicates that more effortful processing (assuming successful comprehension) is associated with better memory (Cairns, Cowart, & Jablon, 1981; Gibbs, 1987; O'Brien & Myers, 1985; Stern, Marrs, Millar, & Cole, 1984). Because of the extra processing required to comprehend negative state remarks, memory should be better for them (when they were recognized as requests) than for the conventional indirect requests.

Second, and most important, because negative state remarks are more ambiguous than conventional indirect requests, speaker status should play a role in the processing of the former but not the latter. Specifically, negative state remarks should be comprehended more quickly when the speaker is high status rather than equal status; no such difference should occur for conventional indirect requests.

Method

Subjects. Subjects were students enrolled in introductory psychology courses at Ball State University who participated to fulfill partial course requirements. All the subjects were native speakers of English. A total of 63 subjects provided usable data. The data from 4 subjects was replaced because they judged the paraphrase to be false for all six remarks for at least one of the four status-remark conditions. Hence, for these subjects it would not be possible to test the Remark \times Status interaction.

Materials and design. The design and method for this study was patterned after Gibbs (1980; 1981). There were 24 relevant scenarios

⁵ It should be noted that Gibbs (1981, 1986) has argued that conventionality is context dependent. However, for Gibbs conventionality is equated with likelihood of use rather than the pragmatic and linguistic criteria outlined here. In general, Gibbs's argument would be correct for differences between conventional forms as defined here, but not for the present distinction between conventional forms and negative state remarks.

Table 1
Materials for All Versions of One Scenario in Experiment 1

Scenario	
The Carter construction company was repairing rural roads during the summer. It was hot work and everyone drank a lot of water. One particularly hot day, the crew's water jug was almost empty by mid-morning. During a break, the foreman (one worker) said to a (another) worker . . .	
Remark	
Conventional indirect request	Would you fill the water jug?
Negative state remark	The water jug is almost empty.
Paraphrase	Go fill the water jug.
Memory test items	
Prompt	They were repairing rural roads.
Conventional correct	Would you fill the water jug?
Negative state correct	The water jug is almost empty.
Conventional incorrect	I'd like you to go fill the water jug.
Negative state incorrect	The water jug hasn't been filled for a while.
Direct	Go fill the water jug.

Note. The words in parentheses were used for the equal status version of the scenario.

and 18 filler scenarios. Each scenario consisted of a short description of an interaction involving two people, a remark said by the speaker addressed to the hearer, and a paraphrase of the speaker's remark (see Table 1 and Appendix A).

Two versions of each relevant scenario were created to manipulate speaker status. One half of the time the speaker was higher status than the hearer (e.g., a professor addressing student); the remainder of the time the speaker and hearer were equal in status (e.g., a student addressing another student).

Two different remarks were used to manipulate remark type. Half of the time the remark was a conventional indirect request (e.g., Could you shut the door?) and half of the time the remark was a negative state remark (e.g., It's noisy in here). All conventional indirect requests questioned either the hearer's ability (i.e., Could you *x*?) or the hearer's willingness (i.e., Would you *x*?) to perform *x* (e.g., shut the door). All negative state remarks were constructed by asserting a negative state (e.g., it is noisy) that the hearer could remedy by performing *x* (e.g., shutting the door). For each scenario, the conventional indirect request and negative state remark contained the same number of words.

The paraphrase that followed a remark was the same for the conventional indirect request and the negative state remark and was always a direct interpretation of the preceding remark (e.g., shut the door).

Speaker status and remark type were completely crossed resulting in four different versions of each scenario. Each subject saw all 24 scenarios, 6 each of the resulting four status-remark type combinations. Status and remark type were thus within-subjects variables. Across the experiment, an approximately equal number of subjects saw each of the four versions of the 24 scenarios.

There were also 18 filler scenarios (some adopted from Gibbs, 1981). These scenarios involved a short description of a situation, a remark, and a paraphrase (see Appendix A). The paraphrases, however, were always clearly false. These filler items were included to prevent subjects from responding automatically on the basis of an expectation that the paraphrase was always correct. The order of presentation of the 42 scenarios was completely randomized for each subject.

Memory test. A five-item forced-choice recognition memory test was constructed. There were 29 items on this test, of which 24

corresponded to the 24 relevant scenarios and 5 were fillers. Each item consisted of a brief one-sentence description of the situation, followed by five remarks (see Table 1). Subjects were instructed to read the sentence and five remarks and then indicate which of the remarks they had previously seen. The five remark alternatives were as follows: the conventional indirect request associated with that scenario (correct if presented), the negative state remark associated with that scenario (correct if presented), a conventional indirect request that had not been presented, a negative state remark that had not been presented, and a direct request that had not been presented.⁶ The forms used for the direct, negative state, and conventional indirect request lures were used in many of the filler trials. Thus, during the experiment subjects were exposed to these forms. The five alternatives were randomized for each item, and the 29 items were presented in a random order.

Procedure. The reaction time portion of the experiment was conducted on an IBM personal computer using the Micro Experimental Laboratory Software (MEL; Schneider, 1988) package. Subjects first read detailed instructions regarding the task and then engaged in four practice trials. The experimenter provided feedback during these practice trials.

To begin a trial subjects would push the enter key. The situation description then appeared on the screen. When they had read and understood the description they again pushed the enter key. The screen then went blank for 1.5 s and a 500-Hz tone sounded indicating that the remark was about to appear. The remark was presented in the center of the screen 1 s after the tone ended. Subjects were instructed to read the remark and push the space bar, which was labeled *Understand Remark*, as soon as they understood what the speaker meant with the remark. They were instructed to do this as quickly as possible, making sure that they understood what the speaker meant with the remark. Subjects then pushed the enter key to continue. The screen went blank for 1.5 s and a 500-Hz tone sounded indicating that the paraphrase was about to be presented. Subjects were instructed to position their fingers over the keys marked *Yes* (/ key) and *No* (z key). The paraphrase appeared on the screen 1 s after the tone ended. Subjects were instructed to indicate, as quickly as possible, whether or not the paraphrase was a reasonable interpretation of the preceding remark. They pushed the *Yes* key if it was a correct interpretation and the *No* key if it was not a correct interpretation. Subjects then pushed the enter key to see the next scenario. Paraphrase judgments (yes or no) and reaction times for the remarks and paraphrase judgments were automatically recorded.

Immediately after completing the reaction time portion of the study, subjects engaged in a 5-min distractor task (recall the names of the states in the United States). Subjects then completed the recognition memory test. Subjects were instructed to read the five alternatives for a scenario and indicate which of the five had been previously presented on the computer screen. The entire procedure lasted between 30 and 40 min.

Results

Results for comprehension speed, memory, and paraphrase judgment were analyzed separately with a 2×2 (Status \times Remark Type) within-subjects analysis of variance (ANOVA). All ANOVAs were conducted with both subjects (F_1) and stimuli (F_2) as random variables. All reported means, however, were calculated averaging over subjects. Only trials on which subjects indicated that the paraphrase was correct were used in

⁶ The direct lure was the same as the paraphrase that subjects had judged earlier. However, subjects were explicitly instructed to base their judgments on the remarks and not the paraphrases.

the analysis of the reaction time and memory data. Also, response times longer than 4,500 ms were not included; these trials were clearly outliers and constituted 1.19% and 2% of the relevant trials for the remarks and paraphrases respectively. The results are presented in Table 2.

Remark comprehension speed. The time it took subjects to comprehend the remarks was analyzed first. The predicted Status \times Remark Type interaction was significant in the subject analysis, $F_1(1, 62) = 5.07, p < .05, MS_e = 168,679$, but not in the item analysis, $F_2(1, 23) = 1.26, p > .10, MS_e = 897,675$. When the remark was a conventional indirect request, response times were roughly equal for the high status ($M = 1,798$) and equal status speaker ($M = 1,794$), $F_1(1, 62) < 1, MS_e = 252,316$; $F_2(1, 23) < 1, MS_e = 632,785$. As expected, however, it took subjects longer to comprehend a negative state remark when the speaker was equal status ($M = 1,905$) than when the speaker was high status ($M = 1,791$), $F_1(1, 62) = 4.08, p < .05, MS_e = 268,397$; $F_2(1, 23) = 2.1, p < .2, MS_e = 677,486$.

There was also a marginally significant effect for remark type in the subject analysis, $F_1(1, 62) = 3.73, p = .058, MS_e = 412,807$. As expected, response time was longer for negative state remarks ($M = 1,848$) than for conventional remarks ($M = 1,796$). This effect, however, was not reliable in the item analysis, $F_2(1, 23) < 1, MS_e = 1,040,577$.

Paraphrase judgment speed. It is possible that subjects indicated they understood a remark even though they were not exactly sure of the meaning. If this was the case, then the speed with which subjects made judgments regarding a paraphrase of the remarks also should be affected by speaker status and remark type. Thus, I analyzed the speed with which subjects made judgments regarding the paraphrase.

There was again a significant Status \times Remark Type interaction, $F_1(1, 62) = 4.95, p < .05, MS_e = 483,223$; $F_2(1, 23) = 5.64, p < .03, MS_e = 279,161$. Like response times for the remarks, there was no difference between the high-status speaker ($M = 1,416$) and equal status speaker ($M = 1,386$) for conventional indirect requests, $F_1(1, 62) < 1, MS_e = 357,717$; $F_2(1, 23) < 1, MS_e = 545,969$. When the remark was a negative state remark, however, it took subjects longer to make the paraphrase judgment when the speaker was equal status ($M = 1,720$) than when the speaker was high status ($M = 1,578$), $F_1(1, 62) = 4.77, p < .04, MS_e = 514,561$; $F_2(1, 23) = 2.89, p = .10, MS_e = 576,008$.

There was also a significant main effect for remark type, $F_1(1, 62) = 34.28, p < .001, MS_e = 511,459$; $F_2(1, 23) = 33.45, p < .001, MS_e = 565,918$. As expected, it took subjects longer to verify that the paraphrase was correct for a negative state remark ($M = 1,620$) than for the conventional indirect request ($M = 1,401$). Despite the significant Status \times Remark interaction, simple effects analyses indicated that this effect was significant for both the high-status speaker, $F_1(1, 62) = 9.25, p < .01, MS_e = 398,468$; $F_2(1, 23) = 12.77, p < .01, MS_e = 389,419$, and the equal status speaker, $F_1(1, 62) = 26.99, p < .001, MS_e = 581,502$; $F_2(1, 23) = 31.16, p < .001, MS_e = 479,964$.

Paraphrase judgments. The only significant results were main effects for status, $F_1(1, 62) = 7.6, p < .01, MS_e = .13$; $F_2(1, 23) = 12.59, p < .01, MS_e = .05$, and remark type, $F_1(1,$

Table 2

Remark and Paraphrase Comprehension Speed (in Milliseconds), Percentage of Correct Paraphrase Judgments, and Memory for Wording as a Function of Speaker Status and Remark Type in Experiment 1

Remark type	Speaker status		
	High	Equal	<i>M</i>
Conventional indirect request			
Remark comprehension speed	1,798	1,794	1,796
Paraphrase comprehension speed	1,416	1,386	1,401
Correct paraphrase ^a	97.3	93.9	95.6
Correct recognition ^b	56.9	58.4	57.6
Negative state remark			
Remark comprehension speed	1,791	1,905	1,848
Paraphrase comprehension speed	1,578	1,720	1,620
Correct paraphrase ^a	87.3	80.7	84.0
Correct recognition ^b	76.0	77.9	77.0

^aRepresents the percentage of trials for which subjects correctly indicated that the paraphrase was correct. ^bRepresents the percentage of the target remarks that were correctly recognized by subjects.

$62) = 27.31, p < .001, MS_e = .19$; $F_2(1, 23) = 38.56, p < .001, MS_e = .18$. Subjects were more likely to endorse the paraphrase when the speaker was high status ($M = 92.3\%$) rather than equal status ($M = 87.3\%$), and when the remark was a conventional indirect request ($M = 95.6\%$) rather than a negative state remark ($M = 84\%$).⁷

Memory. Only trials for which subjects made correct paraphrase judgments were included in this analysis.⁸ As expected, correct recognition was higher for negative state remarks (77.6%) than for conventional indirect requests (57.6%), $F_1(1, 62) = 19.2, p < .001, MS_e = .50$; $F_2(1, 23) = 25.6, p < .001, MS_e = .46$. This memory difference corresponds to the longer comprehension speed for negative state remarks. No other effects were significant (all $F_s < 1$).

Discussion

There were two hypotheses in this study. First, it was expected that indirect requests would vary in their ambiguity such that conventional forms would be easier to process than nonconventional forms. The results were quite consistent with this hypothesis. Overall, conventional indirect requests were processed more quickly, and remembered less well, than nonconventional forms (i.e., negative state remarks). Second, the more ambiguous a remark, the more the hearer should rely on the context (e.g., speaker status) to comprehend the speaker's meaning. The results were also consistent with this hypothesis. The ambiguous negative state remarks were recognized more quickly if the speaker was high status rather than equal status, but speaker status did not affect how quickly the less ambiguous conventional indirect requests were recognized. Note that these effects appear to be reliable; I obtained

⁷ These analyses are based on all trials. When the trials with outliers are excluded the results remain the same.

⁸ When all trials are analyzed (i.e., including trials on which subjects judged the paraphrase to be incorrect) the results remain the same.

essentially the same results in a preliminary study using the same methodology (but with only 16 rather than 24 scenarios).

The results of this experiment, however, did not specify clearly the nature of the process involved in the comprehension of indirect requests. It is not clear, for example, the extent to which the conveyed meanings of the negative state remarks were activated at comprehension or whether subjects only recognized the request interpretation when they read the paraphrase. Also, although negative state remarks took longer to comprehend, it is not clear whether their comprehension involved the recognition and then rejection of the literal meaning (i.e., a Gricean inference process). Finally, although speaker status had an effect on comprehension speed, it is not clear how it affected the comprehension process. These and other questions were examined in Experiments 2, 3, and 4.

Experiment 2

It was not clear from the results of Experiment 1 whether the conveyed (request) meaning of negative state remarks was activated at comprehension. Recall that remark type and speaker status (for negative state remarks) had a substantial effect on the speed of the paraphrase judgments. If subjects had recognized the conveyed meaning when they indicated their understanding of the remark, then paraphrase judgment speed should not have varied across experimental conditions as it did. Previous research has demonstrated that the conveyed meanings of conventional indirect requests are accessed at comprehension (Gibbs, 1983). It is not clear, however, whether this also occurs for nonconventional forms such as negative state remarks.

Even if a conveyed meaning is recognized, it is also possible that this recognition is probabilistic rather than absolute. Harris and Monaco (1978) have argued that much human communication is probabilistic in the sense that hearers only form hypotheses about what a speaker means, and that these hypotheses are open to further testing and subsequent refinement. This would seem to be particularly true for the recognition of the conveyed meanings of negative state remarks. Thus, one interpretation of the relatively large effects for the paraphrase judgments is that subjects sometimes made only a tentative interpretation when they indicated their comprehension of the remark, and their degree of confidence in this interpretation was affected by speaker status and remark type.

I examined these possibilities by using a sentence verification procedure developed by Gibbs (1983). Subjects read the same scenarios and remarks that had been used in Experiment 1. However, after each remark, subjects engaged in a sentence verification task. A string of words appeared on the screen, and subjects were asked to indicate as quickly as possible whether or not the word string formed a sentence. Sometimes these target strings were sentences that were indirect interpretations of the preceding remark (i.e., the conveyed meaning); other times the strings were sentences that were not related to the preceding remark (control sentences). If there is any activation of the conveyed meaning when a remark is comprehended, then sentence verification judgments for the conveyed meaning targets should be facilitated (relative to control sentences). Using this procedure, Gibbs (1983) demonstrated that the

conveyed meaning of conventional indirect requests are activated at comprehension. If the conveyed meaning of negative state remarks is also activated at comprehension, then they also should facilitate subsequent sentence verification judgments for the conveyed meaning targets. Finally, if the degree to which the conveyed request meaning is activated varies as a function of speaker status (as Experiment 1 suggests), then facilitation should be greater for a high-status speaker than for an equal status speaker.

Method

Subjects. Subjects were students enrolled in introductory psychology courses at Ball State University who participated to fulfill course requirements. All subjects were native speakers of English. A total of 74 subjects provided usable data. The data from 5 subjects were discarded; 4 subjects reported that they had not read the scenario descriptions and 1 subject provided incorrect verification judgments for all of the trials in one condition.

Stimulus materials and design. The same 42 (24 relevant and 18 fillers) scenarios (including the remarks) used in Experiment 1 were retained. As before, speaker status and remark type were crossed resulting in four versions of each scenario. Orthogonal to the remark and status manipulation was the type of target sentence presented in the sentence verification task. The target string for the 24 relevant scenarios was always a meaningful, grammatical sentence. However, half of the time this sentence was the conveyed (request) meaning of the immediately preceding remark (related sentence condition); the remainder of the time this sentence was not related to the previously presented remark (unrelated sentence condition). For example, for the scenario presented in Table 1, the related target sentence was "Go fill the water jug" and the unrelated target sentence was "I heard that new song" (see Appendix A for other examples).

Subjects saw all 24 relevant scenarios, three each of the resulting eight Status \times Remark Type \times Target Type conditions, which were within-subject variables. Across the experiment, an approximately equal number of subjects saw each of the eight versions of each scenario.

The related and unrelated target sentences for each scenario contained the same number of words, and a pretest was conducted to ensure that the related targets were not easier to verify than the unrelated targets. Pretest subjects ($n = 29$) performed the sentence verification task but with no accompanying context. For none of the 24 scenarios was the speed for verifying the unrelated target significantly longer than for the related targets ($p > .05$).

Because the target strings (both related and unrelated) for the 24 relevant scenarios were grammatical sentences, the correct response for the subjects was to indicate yes. To keep subjects from responding yes automatically, all of the target strings presented on the 18 filler trials were clearly not sentences (see Appendix A for examples). The presentation order of the scenarios was randomized for each subject.

Procedure. The general procedure was identical to Experiment 1 with one major difference. After subjects indicated they understood what the speaker meant with the remark, the screen went blank for 1 s and a 500-Hz tone sounded indicating that the target string was about to be presented. The target string appeared on the screen 1 s after this tone ended. Subjects were instructed to indicate, as quickly as possible, whether or not the presented word string formed a meaningful, grammatical sentence. The instructions emphasized that subjects should respond as quickly as possible while trying to be as accurate as possible, and to push the Yes (/) key if the string was a meaningful, grammatical sentence and the No (z) key if it was not a meaningful, grammatical sentence. Sentence verification judgments (yes or no) and judgment speed were automatically recorded.

Results

Only trials on which subjects made correct sentence verification judgments were included in the analyses. The overall error rate was 3.4%, and this rate did not vary significantly ($p > .10$) as a function of any of the independent variables (see Table 3). Response times greater than 4,500 ms were treated as errors and not included in the analyses. These trials were clearly outliers and constituted 1.57% of the relevant trials. The time it took subjects to perform the sentence verification task was analyzed with a $2 \times 2 \times 2$ (Status \times Remark Type \times Target Type) within-subjects ANOVA. All ANOVAs were conducted twice using subjects (F_1) and items (F_2) as random variables. However, all reported means were computed averaging over subjects. The means are reported in Table 3.

Overall, subjects were 191 ms faster at verifying the related sentences ($M = 1,546$) than the unrelated sentences ($M = 1,737$), $F_1(1, 73) = 70.07$, $p < .0001$, $MS_e = 227,247$; $F_2(1, 23) = 24.17$, $p < .0001$, $MS_e = 624,731$. This indicates that there was some activation of the conveyed meaning of the requests at comprehension. Despite significant interactions (see below), this effect was substantial and significant across items and subjects (all $ps < .01$) for each of the four status-remark type combinations.

The degree of facilitation was not constant, however, as indicated by a significant three-way Status \times Remark Type \times Target Type interaction, $F_1(1, 73) = 5.59$, $p < .05$, $MS_e = 310,484$; $F_2(1, 23) = 5.78$, $p < .05$, $MS_e = 204,201$. For the negative state remark, the difference between the related and unrelated targets was significantly greater for the high-status speaker ($M = 279$ ms) than for the equal status speaker ($M = 136$ ms), $F_1(1, 73) = 8.33$, $p < .01$, $MS_e = 217,987$; $F_2(1, 23) = 8.08$, $p < .01$, $MS_e = 211,428$. For the conventional indirect requests, however, the difference between the related and unrelated targets was roughly equal for the high-status ($M = 146$ ms) and equal status speaker ($M = 203$ ms), $F_1(1, 73) < 1$, $MS_e = 353,260$; $F_2(1, 23) < 1$, $MS_e = 698,052$.

There was also a significant Status \times Target Type interaction over items, $F_2(1, 23) = 4.38$, $p < .05$, $MS_e = 120,867$, but not over subjects, $F_1(1, 73) = 2.48$, $p = .12$, $MS_e = 176,902$. The difference between the related and unrelated target sentences was greater for the high-status speaker ($M = 212$ ms) than for the equal status speaker ($M = 170$ ms).

Discussion

Previous research (Gibbs, 1983) has demonstrated that the conveyed meaning of conventional indirect requests are accessed at comprehension. The present results suggest that this also may be the case for nonconventional negative state remarks. Specifically, subjects' responses to the related target sentences were facilitated (relative to judgments made to unrelated target sentences) when they read the scenarios followed by the negative state remarks. This effect was roughly equal in size to that obtained when subjects read scenarios followed by the conventional indirect requests.

At the same time, there also was evidence that the degree to which the conveyed meaning is accessed can vary as a function of the speaker's status. For negative state remarks, subjects'

Table 3
Target Verification Speed (in Milliseconds) as a Function of Speaker Status, Remark Type, and Target Type in Experiment 2

Speaker status	Target type				Difference ^c
	Related ^a		Unrelated		
	<i>M</i>	Error rates ^b	<i>M</i>	Error rates ^b	
Conventional indirect request					
High	1,565	2.7	1,711	5.4	-146
Equal	1,537	3.4	1,740	2.3	-203
<i>M</i>	1,551	3.0	1,726	3.8	-174
Negative state remark					
High	1,515	2.7	1,794	2.3	-279
Equal	1,566	2.3	1,702	2.0	-136
<i>M</i>	1,541	2.5	1,748	2.1	-208

^aThe related targets were the indirect (request) interpretation of the sentence. ^bPercentage of trials for which subjects made errors on the verification task. ^cThis indicates the difference between sentence verification times for the related and unrelated targets.

responses to the related target sentences were facilitated to a greater degree when the speaker was high (rather than equal) status. Thus, the conveyed meanings of negative state remarks may not be activated with absolute certainty. Instead, hearers may develop an expectation of how a remark is to be interpreted, and this expectation will be influenced by features of the context such as speaker status.

The status difference for negative state remarks was a result of both relative facilitation (verification of the related sentences was 51 ms faster for the high-status speaker than for the equal status speaker) and relative inhibition (verification of the unrelated sentences was 92 ms slower for the high-status speaker than for the equal status speaker).⁹ This is consistent with the depiction of comprehension as involving variable expectations about how a remark is to be interpreted. In these situations, knowledge that a speaker is high status creates a fairly strong expectation of a directive interpretation of any remark that may occur. Thus, when an unrelated target appears, subjects must shift attention from the expected (directive) interpretation of the remark to the unrelated (and unexpected) target. Relative inhibition occurs because the expectation of a directive is not as strong when the speaker is equal status. Facilitation occurs when the speaker is high status because of the confirmation of the expectation.

Note that in this research there was a relatively long delay between the remark and target string. Research (e.g., Neely, 1977) suggests that with delays of this length, any facilitation, inhibition, or both will be due to conscious and deliberate (rather than automatic) processing. Thus, these results are not diagnostic with regard to whether the conveyed meaning of indirect requests is (ever) automatically activated. What these results do demonstrate, though, is that certain contexts can activate an expectation of a request interpretation and that this expectation is greater when the speaker is high rather than equal in status.

⁹ The effects are referred to as relative rather than absolute because there is no neutral comparison.

Table 4
*Target Verification Speed (in Milliseconds) as a Function of
 Speaker Status, Remark Type, and Target Type in Experiment 3*

Speaker status	Target type				Difference ^c
	Related ^a		Unrelated		
	<i>M</i>	Error rates ^b	<i>M</i>	Error rates ^b	
Conventional indirect request					
High	2,268	3.3	2,254	2.8	14
Equal	2,293	3.1	2,281	2.6	12
<i>M</i>	2,281	3.2	2,267	2.7	13
Negative state remark					
High	2,260	1.4	2,230	4.7	30
Equal	2,127	3.8	2,372	4.9	-245
<i>M</i>	2,194	2.6	2,301	4.8	-107

^aThe related targets were literal interpretations of the sentences.

^bPercentage of trials for which subjects made errors on the verification task. ^cThis indicates the difference between sentence verification times for the related and unrelated targets.

Finally, these results do not provide direct evidence regarding whether the literal meaning of these indirect remarks are ever accessed. Similarly, it is not clear whether the expectation of a directive occurred only after a remark was presented or whether the context alone may activate an expectation regarding the interpretation of potential remarks. These possibilities were examined in Experiments 3 and 4, respectively.

Experiment 3

The results of Experiments 1 and 2 suggest that the processing of negative state remarks when the speaker is high status is similar to the processing of conventional forms; the conveyed meaning is recognized relatively quickly, and possibly without the hearer seriously entertaining the literal meaning of the remark. In contrast, when the speaker is not high status, the processing of negative state remarks is more time consuming, and possibly does involve a serious consideration (and eventual rejection) of the literal meaning of the remark. The purpose of Experiment 3 was to test these possibilities.

Subjects in this experiment saw the same situations and remarks as in Experiment 2. However, the target strings on the relevant trials were either literal readings of the remark (rather than the indirect request readings as in Experiment 2) or sentences unrelated to the context. If the literal meaning is seriously considered during processing, then sentence verification judgments of the targets that are literal readings should be facilitated. Based on the results of Experiments 1 and 2, it was expected that this would occur only for negative state remarks uttered by an equal status speaker. In contrast, judgments of the literal targets were not expected to be facilitated when the speaker was high status or the form was conventional.

Method

Subjects. Subjects (all native speakers of English) were students enrolled in introductory psychology classes at Ball State University who participated to fulfill course requirements. A total of 71 subjects

provided usable data. The data from 4 subjects (who did not read the scenarios and remarks) was deleted.

Stimulus materials and design. The materials used in this experiment were identical to those used in Experiment 2 with one important exception. On the trials for which a related target was to be judged, the target was a literal reading of the remark rather than the indirect (request) reading that had been used in Experiment 2. For example, for the scenario presented in Table 1, the literal readings were "We're almost out of water" and "Are you willing to get water?" for the negative state remark and conventional indirect request, respectively. The unrelated target for this scenario was "He forgot to go to class" (see Appendix A). This procedure creates a new difficulty because the literal targets are different for the negative state and conventional remarks (in Experiment 2 the indirect reading was identical for the two different remark types). Moreover, new unrelated targets were required that were not any easier to comprehend than these literal interpretations. All three target sentences for each scenario (i.e., the two literal targets and one unrelated target) had the same number of words. In addition, several pretests were conducted to select a set of literal interpretations for the two remark types that were equal in processing difficulty when there was no context. For the final set of targets, sentence verification speeds (when there was no context) were not significantly different ($p > .05$) for any of the sets of three targets. The filler trials used in Experiment 2 were retained. Thus, the target strings on these trials were clearly ungrammatical. The presentation order was randomized for each subject.

Procedure. The procedure for this experiment was identical to the procedure for Experiment 2.

Results and Discussion

Only trials for which subjects made correct sentence verification judgments were included in the analyses. The overall error rate was 3.3%, and this rate did not vary significantly ($p > .10$) as a function of any of the independent variables. Response times greater than 6,000 ms (less than 2% of the trials) were treated as errors and not included.¹⁰ The time it took subjects to perform the sentence verification task was analyzed with a $2 \times 2 \times 2$ (Status \times Remark Type \times Target Type) within-subjects ANOVA. All analyses were conducted twice using subjects (F_1) and items (F_2) as random variables. All means (reported in Table 4) are averaged over subjects.

The only significant effect occurred for the three-way Status \times Remark Type \times Target Type interaction, $F_1(1, 70) = 4.16, p < .05, MS_e = 507,476$; $F_2(1, 23) = 10.79, p < .01, MS_e = 190,033$. Simple effects analyses indicated that subjects were significantly faster at verifying the literal target than the unrelated target when an equal status speaker used a negative state remark, $F_1(1, 70) = 15.38, p < .001, MS_e = 368,330$; $F_2(1, 23) = 4.53, p < .05, MS_e = 880,096$. In contrast, verification of the literal targets was not facilitated (all $F_s < 1$) for any of the other three status-remark type conditions. Thus, only when the form was not conventional (e.g., a negative state remark) and the speaker was equal status did there appear to be any activation of the literal meaning. This is consistent with the results of Experiments 1 and 2 and suggests that the literal meaning of an indirect request may not be activated when the

¹⁰ Using 4,500 ms as the cutoff point for outliers (as in Experiments 1 and 2) would have resulted in excluding more than 2% of the trials. This is not recommended (Shoben, 1982).

speaker is high status or when the form is conventional. As in Experiment 2, both facilitation and inhibition occurred, although (unlike Experiment 2) the relative size of each was roughly equal.

Experiment 4

There are at least two ways in which speaker status (and other features of the interpersonal context) can affect the comprehension of indirect requests. First, knowledge that a speaker is high status could simply facilitate a process whereby the hearer recognizes and then rejects the literal meaning of a request interpretation. A second possibility is that knowledge that a speaker is high status could create an expectation that directive interpretations are likely, and this may result in hearers recognizing the request meaning of a remark without first recognizing (and then rejecting) the literal meaning of the remark (i.e., a Gricean inference process). In this case, the situational context (i.e., high-status speaker) would predispose hearers to recognize a speaker's conveyed meaning (Cohen & Levesque, 1990; Ervin-Tripp, et al., 1987).

The results of Experiment 3 are consistent with the second possibility. That is, the fact that there was no activation of the literal meaning when the speaker was high status suggests that a Gricean inference process was circumvented. The purpose of Experiment 4 was to examine this possibility further.

If high speaker status is a contextual feature that predisposes hearers to interpret ambiguous remarks as directives, then knowledge that an interactant is high status should be sufficient for facilitating the comprehension of requests. Subjects in this experiment read scenarios in which either a high- or low-status interactant notices a negative state in a situation and hence for which a request might be expected. Unlike Experiments 1, 2, and 3, however, a request did not follow the scenario. Instead, subjects engaged in the sentence verification task after reading the scenario. The target sentences in this task were either requests to fix the negative state (the related target sentences used in Experiment 2) or remarks unrelated to the context. It was expected that verification of the related sentences would be faster than verification of the unrelated sentences when the speaker was high in status but not when the speaker was low in status.

Method

Subjects. Subjects (all native speakers of English) were students enrolled in introductory psychology courses at Ball State University who participated to fulfill course requirements. A total of 78 subjects provided usable data. The data from 3 subjects were discarded. These subjects reported that they had not read the scenario descriptions and remarks before performing the sentence verification task.

Stimulus materials and design. The 42 scenarios (24 relevant and 18 fillers) from Experiments 1, 2, and 3 were adopted for use in this study. Several changes were made in these materials. First, the indirect requests that followed the scenario descriptions were deleted. Second, the scenarios were rewritten so that the last sentence of each one described one of the interactants as noticing or realizing the existence of a negative state (the same negative state that prompted the request in Experiments 1-3). Third, the interactant who notices the negative state was either high or low (rather than equal) status. Thus, the status

Table 5

Target Verification Speed (in Milliseconds) as a Function of Speaker Status, and Target Type in Experiment 4

Speaker status	Target type				
	Related ^a		Unrelated		Difference ^c
	<i>M</i>	Error rates ^b	<i>M</i>	Error rates ^b	
High	1,625	3.1	1,856	2.8	-231
Low	1,648	3.2	1,745	6.5	-97
<i>M</i>	1,636	3.1	1,801	7.5	-165

^aThe related targets were requests that would lessen or eliminate the negative state noticed in the situation. ^bPercentage of trials for which subjects made errors on the verification task. ^cThis represents the difference between sentence verification times for the related and unrelated targets.

manipulation was stronger in this experiment than in Experiments 1-3. Sample scenarios are presented in Appendix B.

There were two levels of status (high or low) for each scenario. Orthogonal to the status manipulation was the type of target string presented in the sentence verification task. For the 24 relevant scenarios, the string to be judged was either a direct request (the conveyed request interpretations used in Experiments 1 and 2) that would eliminate or lessen the negative state (related sentence condition) or a sentence that was not related to the previously presented remark (unrelated sentence condition). These target sentences were the same ones used in Experiment 2 (thus, pretesting had indicated no difference in comprehension speed between these remarks in the absence of a context). Speaker status and target type were crossed resulting in four different versions for each scenario. Subjects saw all 24 relevant scenarios, 6 each of the resulting four status-target type conditions. Status and target type were thus within-subject variables. Across the experiment, an approximately equal number of subjects saw each of the four versions of the 24 scenarios. As in Experiments 2 and 3, all of the target strings presented on the 18 filler trials were ungrammatical strings. The presentation order was randomized for each subject.

Procedure. The general experimental procedure was identical to that used in Experiments 2 and 3. However, a remark was not presented after the situation description. Instead, after reading the scenario, the target string to be judged appeared in the center of the screen. Subjects were instructed to push the Yes key if the string formed a meaningful, grammatical sentence, and to push the No key if the string did not form a meaningful, grammatical sentence. As before, subjects were instructed to make this judgment as quickly as possible while striving to be as accurate as possible.

Results

Only the trials on which subjects made correct sentence verification judgments were included in the analyses. Responses greater than 5,000 ms were treated as errors and not included in the analyses. This constituted 1.4% of all relevant trials.¹¹ The time it took subjects to judge the target sentences was analyzed with a 2×2 (Status \times Target Type) within-subjects ANOVA. All analyses were conducted twice using subjects (F_1) and items (F_2) as random variables. However, all means (reported in Table 5) were computed averaging over subjects.

¹¹ The cutoff for outliers was chosen so that less than 2% of the trials were excluded from analysis.

Subjects were 165 ms faster at verifying the related sentences ($M = 1,636$) than the unrelated sentences ($M = 1,801$), $F_1(1, 77) = 19.06, p < .0001, MS_e = 551,375$; $F_2(1, 23) = 9.32, p < .01, MS_e = 1,215,484$. This was qualified, however, by a significant Status \times Target Type interaction, $F_1(1, 77) = 5.12, p < .03, MS_e = 394,028$, $F_2(1, 23) = 4.18, p = .053, MS_e = 371,731$. Simple effects analyses indicated that when the speaker was high status, subjects were significantly faster at verifying the related sentences than the unrelated sentences, $F_1(1, 77) = 28.51, p < .0001, MS_e = 382,100$; $F_2(1, 23) = 15.28, p < .001, MS_e = 702,206$. In contrast, when the speaker was low status, the speed with which subjects verified the related and unrelated sentences was not significantly different, $F_1(1, 77) = 2.86, p > .05, MS_e = 552,882$; $F_2(1, 23) = 2.52, p > .10, MS_e = 921,105$.

An analysis of the error rate indicated that subjects made more errors for the unrelated targets (7.5%) than for the related targets (3.1%), $F_1(1, 77) = 9.59, p < .01, MS_e = .08$; $F_2(1, 23) = 7.61, p < .02, MS_e = .10$. No other effects were significant for the error rates.

Discussion

Subjects in this study read short descriptions of situations in which either a high- or low-status speaker noticed something negative (e.g., the room was cold) in the setting. Participants then made sentence verification judgments of word strings that were either direct requests to fix the negative state (e.g., shut the door) or were unrelated to the setting. Subjects were significantly faster at verifying the request targets than the unrelated targets when the speaker was high status; this did not occur when the speaker was low status.

Note that in this experiment (unlike Experiments 2 and 3), the significant difference in verification speed as a function of speaker status was due almost exclusively to inhibition (i.e., slower response times to the unrelated target when the speaker was high status) rather than to facilitation (faster response times for the related target when the speaker was high status). This is consistent with semantic priming research demonstrating that inhibition effects become large when the prime allows subjects to form only a general expectation about possible targets (Becker, 1980). A general (rather than specific) expectation produces inhibition because there is a relatively large set of expected targets that must be rejected when encountering an unexpected target. In this study, the remark was not presented and so subjects could form only a general expectation about the targets (in contrast to Experiments 2 and 3 when the presentation of the remark produced more specific expectations). In general, the set of expected targets was probably larger for the high-status interactant than for the low-status interactant because the former set included directives and the latter did not. Substantial inhibition effects occurred because when the interactant was high status, the expected directives had to be rejected whenever an unrelated target was presented; this was not necessary when the interactant was low status.

In summary, the results provide some support for the idea that speaker status (and other contextual features) may play a role in the comprehension of indirect requests before the

occurrence of a remark (through the activation of expectancies about likely speech acts), rather than being a contextual feature that is referenced only after a remark has been encountered.

General Discussion

The results of these experiments fill in some of the gaps in our knowledge about the processes involved in the comprehension of nonliteral meanings. Indirect speech acts, and indirect requests in particular, have presented a problem for theories of language comprehension. How is it that hearers are able to infer a speaker's intended, nonliteral meaning? The present results suggest that this question is too simple. Rather, there appears to be different means for performing indirect requests, and these various request types will be processed differently depending on the interpersonal context within which they occur.

Many indirect requests are conventional and hence relatively unambiguous. As a result, these forms almost always are recognized as requests and are comprehended quickly (Experiment 1). Moreover, the recognition and rejection of the literal reading of these remarks does not appear to be necessary to understand the conveyed meaning (Experiment 3). Most important, the ease with which these forms are processed does not appear to be affected by features of the interpersonal context such as speaker status (Experiments 1–3). Regardless of speaker status, conventional indirect requests will be quickly recognized as requests.

There are, however, many (if not an infinite number of) nonconventional ways of performing indirect requests, and there have been few attempts to systematically describe these forms. One fairly common form, termed *negative state remark*, was introduced in this research. Negative state remarks can be performed by asserting (or questioning) the existence of some negative state if the negative state can be eliminated or lessened by the hearer. These forms should be more ambiguous than conventional forms (they are off record in P. Brown and Levinson's 1987 politeness theory), and the present results indicate that they are. Relative to conventional forms, negative state remarks were less frequently judged to be requests, subjects took longer to comprehend them, memory for them was quite good (Experiment 1), and unlike conventional forms, speaker status had an impact on how they were comprehended (Experiments 1–3).

This difference in the processing of conventional and nonconventional forms is mediated, however, by the status of the speaker. When the speaker is high status, the processing of negative state remarks is quite similar to the processing of conventional forms. The remark is understood quickly (Experiment 1) and without the literal meaning being accessed (Experiment 3), and this quick and direct recognition appears to be due to an expectation that directive interpretations are likely when the speaker is high status (Experiment 4). It is only when the speaker is not high status that a processing difference occurs. In this case, directives are not expected (Experiment 4). As a result, comprehension of a nonconventional form requires first the comprehension of the literal meaning of the remark (Experiment 3) and then the rejection of that meaning

in favor of the conveyed meaning (Experiment 1). The results of Experiment 1 demonstrate that this is a relatively time-consuming process.

These results, then, have implications for theories regarding the comprehension of indirect speech acts. Specifically, the process articulated by Grice (1975) and Searle (1975) whereby the hearer must first recognize and then reject the literal meaning of a remark need not always occur. Conventional forms that are idiomatic can circumvent this type of processing, as Gibbs (1983) has also demonstrated. High speaker status may also circumvent the need for this type of processing through the activation of expectations regarding the most likely reading of a speaker's remarks. Higher status people frequently direct the actions of others, and hence others expect the remarks of higher status speakers (in the appropriate contexts) to act as directives. The effects of this expectation probably extend to nonverbal communication. For example, a nonverbal action (e.g., shivering to indicate that one is cold) may be comprehended more quickly as a request (e.g., to shut a window) when the speaker is high rather than low in status.

The sensitivity of negative state remarks to the social context also provides support for the idea that there is variability in terms of the strength with which a hearer will endorse the indirect interpretation of a remark. If the conveyed meanings were always recognized with absolute certainty, then neither paraphrase judgments (Experiment 1) nor sentence verification speeds (Experiment 2) should have varied as a function of speaker status. That they did is consistent with the probabilistic model of communication articulated by Harris and Monaco (1978). Note, in this regard, that when the recognition of the conveyed meaning is less than certain, there are options open to the hearer. For example, instead of immediately acting, the recipient of a negative state remark (e.g., "It's cold in here") may respond to the literal reading of the remark (e.g., "Yes, it is. . ."), and then offer to perform the action that will eliminate or lessen the negative state (e.g., "Should I close the window?"). Whether or not a request was intended can be clarified in this manner.

There are obvious similarities between the present research and research on the obstacle hypothesis (Francik & Clark, 1985; Gibbs, 1981, 1986). Both approaches are based on the assumption that interlocutors must coordinate (Clark, 1985), and therefore that variables affecting the production of indirect requests will play a role in the comprehension of indirect requests. Also, people use indirect requests (and hence are polite) primarily because of interpersonal considerations (P. Brown & Levinson, 1987; Holtgraves, 1992), and pointing out a potential obstacle to the hearer is one way in which a speaker can be polite.¹²

The present results provide only a rough sketch of the reasoning processes involved in the comprehension of indirect requests, particularly negative state remarks. For example, when are inferences about the speaker's interpersonal motives made, and what role do those inferences play in the recognition of the speaker's intention? It is possible that the comprehension of a negative state remark involves not only a recognition of the conveyed meaning (i.e., the request) but also inferences about why the request is being phrased in this way (e.g., to avoid imposing on the hearer, or conversely, to be

manipulative). In fact, it may be the case that the recognition of the conveyed meaning of certain remarks depends on the hearer's recognition of the speaker's reason for being indirect (see Holtgraves, 1991).

There are several additional broad avenues for future investigation. Most of the past psycholinguistic research on indirect speech acts has examined requests, and much less is known about the role of interpersonal variables in the production and comprehension of other speech acts. For example, to what extent are criticisms and self-disclosures performed indirectly as a function of the interpersonal context, and if so, what role is played by these variables in a hearer's recognition of the speaker's intent with these remarks? There is an additional need to examine the role played by interpersonal variables other than status in the comprehension of indirect speech acts. There is preliminary evidence that other interpersonal variables that affect the production of certain indirect speech acts will play a role in the comprehension of those remarks. Holtgraves (1991), for example, found that the degree of face-threat in a situation influenced the extent to which the indirect reading of a reply was judged to be what the speaker meant; the greater the face-threat, the more likely the indirect reading was perceived as being intended. Also, Sluski and Turnbull (1988) found that the nature of the relationship between interactants (in terms of familiarity and affect) influenced the extent to which compliments and insults were interpreted indirectly. Future research should examine in depth these and other interpersonal variables that have been demonstrated to play a role in the production of different speech acts.

That the social context is important in language processing is obvious and perhaps trivial. How it affects processing is considerably less obvious and definitely nontrivial. In this article, I have argued that people are generally indirect because of interpersonal concerns, and that the comprehension of indirectness must therefore involve many of the same interpersonal considerations. Language use is a social activity, and it seems likely that the same social variables will play a role in both the production and the interpretation of language.

¹² Note, however, that even when an obstacle does not exist, people will still generally use indirect constructions so as to be polite (Holtgraves & Yang, 1992), and this is especially so when the speaker is lower in status than the hearer.

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Appendix A

Sample Scenarios, Remarks, Paraphrases, and Target Strings Used in Experiments 1–3

The words in parentheses were used for the equal status versions. The conventional indirect request (CIR) and negative state remark (NSR) were the remarks used in all three experiments. The conveyed meaning was the paraphrase in Experiment 1 and the related target string in Experiment 2. The literal targets were the related target strings in Experiment 3.

Relevant Trials

1. Robert is the owner of a company that manufacturers computer software, and Michael is one of his employees. (Robert and Michael work together for a company that manufactures computer software.) This morning they are working together on a project. However, they cannot finish the project until they receive a report that is due to come in today's mail. Robert pauses and then says to Michael:

CIR: Could you go get the mail now?

NSR: We can't finish this without the mail.

Conveyed Meaning: Go get the mail now.

Unrelated (Experiment 2): The kitchen is on fire.

Literal for CIR: Are you able to get the mail now?

Literal for NSR: We will need the mail to finish this.

Unrelated (Experiment 3): The kitchen is too small for me.

2. Mary and Jane work in Bracken library. Mary is the head reference librarian and Jane is one of her student assistants. (They are both cataloging assistants.) This afternoon they are to catalog some books and they plan to do this in one of the workrooms. This room hasn't been used for a while and the thermostat is set for low. As a result it is very cold in the room. Soon after they start working Mary says to Jane:

CIR: Would you turn up the thermostat?

NSR: It seems very cold in here.

Conveyed Meaning: Turn up the thermostat.

Unrelated (Experiment 2): I know it happened.

Literal for CIR: Are you willing to turn it up?

Literal for NSR: The room seems very cold to me.

Unrelated (Experiment 3): We will finish it by tomorrow.

3. Dr. White was making his rounds in the hospital one morning with his nurse. (Nurse White and Nurse Jones were making the rounds in the hospital one morning.) He (They) spent some time examining one patient who seemed not to be recovering. After talking with the patient and examining his chart, he turned and said to the nurse (Nurse White turned and said to Nurse Jones):

CIR: Would you give this patient his medicine?

NSR: This patient hasn't had his medicine yet.

Conveyed Meaning: Give this patient his medicine.

Unrelated (Experiment 2): I will drive there tomorrow.

Literal for CIR: Are you willing to give him the medicine?

Literal for NSR: The patient has not yet taken his medicine.

Unrelated (Experiment 3): I will drive to the store tomorrow afternoon.

4. Janet was a secretary at the Acme employment agency. (Marsha and Janet were secretaries at the Acme employment agency.) She (Janet) recently started smoking cigarettes again after having quit for several months. Today Janet was smoking a lot and the room was getting very smoky. Mr. Smith, the manager of the agency, (Marsha) turned to Janet and said:

CIR: Would you stop smoking in here?

NSR: It's getting very smoky in here.

Conveyed Meaning: Stop smoking in here.

Unrelated (Experiment 2): He helped me yesterday.

Literal for CIR: Are you willing to stop smoking?

Literal for NSR: The smoke is thick in here.

Unrelated (Experiment 3): I took out the garbage yesterday.

5. Jack and Sam work for the post office in the mailroom. Jack is the mailroom supervisor and Sam is a new worker. (They are both mail sorters.) Today they are working together sorting mail. It's dark outside and the lighting is low and it's difficult to read the addresses. Jack turns to Sam and says:

CIR: Could you turn up the lights?

NSR: The lighting seems low in here.

Conveyed Meaning: Turn up the lights.

Unrelated (Experiment 2): He heard the news.

Literal for CIR: Are you able to turn up the lights?

Literal for NSR: There is not enough light in this room.

Unrelated (Experiment 3): I will take a cab to the airport.

6. The board of directors at the Acme motor company was about to convene a meeting in the company conference room. Harry Smith, president of the company, and Jan his secretary, were in the conference room checking to see if it was ready. (Harry and Frank, two of the president's assistants, were in the conference room checking to see if it was ready) Mr. Smith then said to Jan (Harry then turned to Frank) and said:

CIR: Would you go fill the water glasses?

NSR: The water glasses seem to be empty.

Conveyed Meaning: Go fill the water glasses.

Unrelated (Experiment 2): Summer is my favorite season.

Literal for CIR: Are you willing to fill the water glasses?

Literal for NSR: There is no water in the water glasses.

Unrelated (Experiment 3): I am going to Florida for spring break.

Filler Trials

The paraphrase was used in Experiment 1 and the ungrammatical target in Experiments 2 and 3.

1. Mick and Steve were walking in the Village one day. They decided that they wanted to eat some lunch. They walked into the Chug and looked at the menu. After Nick decided what he wanted, he went to the counter and said:

Remark: I'd like a hamburger.

Paraphrase: Do you have bratwurst?

Ungrammatical Target: Why did go for now?

2. After a long hard day, Sarah was ready to drive home. When she got into her car and tried to start it nothing happened. She knew the battery was dead. She spotted a campus police car. She waved him down and said to him:

Remark: My battery seems to be dead.

Paraphrase: Do you know what time it is?

Ungrammatical Target: Said it forgot then.

3. John and Mary decided to see the new movie at the Westwood theatre. The movie was not very good and John was very bored. He really wanted to leave. He turned to Mary and said:

Remark: This movie is boring me to death.

Paraphrase: Go get us some popcorn.

Ungrammatical Target: Can we tree the forgotten?

4. Beth went to Radio Shack to buy a telephone answering machine. She saw one she liked, but the price wasn't marked and she wasn't sure she could afford it. She called over to the salesman and said:

Remark: There is no price on this machine.

(Appendixes continue on next page)

Paraphrase: Is there a warranty on this machine?

Ungrammatical Target: Go figure the weeds are.

5. Tony was a heavy cigarette smoker. One afternoon he was walking with his friend Mark when he discovered he was out of smokes. They walked over to the student center and Tony went into the bookstore to

get change for the cigarette machine. He walked up to the counter and said to the person there:

Remark: I'd like change for the cigarette machine.

Paraphrase: Give me a pack of Marlboros.

Ungrammatical Target: Close now for it.

Appendix B

Sample Scenarios Used in Experiment 4

The words in parentheses were used for the low speaker status versions of the scenarios. The target strings and filler scenarios were identical to those used in Experiment 2.

1. Robert is the owner of a company that manufactures computer software, and Michael is one of his employees. This morning they are working together on a project. Robert (Michael) realizes that they cannot finish the project until they receive a report that is due to come in today's mail.

2. Mrs. Meeker is the head reference librarian at Bracken library and Jane is one of her student assistants. This afternoon they are cataloging some books in one of the workrooms. This room hasn't been used for a while and the thermostat is set for low. Mrs. Meeker (Jane) notices how cold it is in the room.

3. Dr. White was making his rounds in the hospital one morning with his nurse. They spent some time examining one patient who seemed not to be recovering. After talking with the patient and examining his chart, Dr. White (the Nurse) realizes this patient has not received his medicine.

4. Janet is a secretary (Mr. Smith is the manager) at the Acme employment agency. She (He) recently started smoking cigarettes again after having quit for several months. Today Janet (Mr. Smith) was smoking a lot and the room was getting very smoky. Mr. Smith, Janet's boss, (Janet, a secretary at the office,) notices that the smoke is bothering the customers and staff.

5. Jack is a mailroom supervisor for the post office, and Jeff is one of the new workers. Today they are working together sorting mail. It's

dark outside and the lighting is low and it's difficult for Jack (Jeff) to read the addresses.

6. The board of directors at the Acme motor company was about to convene a meeting in the company conference room. Harry Smith, president of the company, and Jan his secretary, were in the conference room checking to see if it was ready. Mr. Smith (Jan) noticed that the water glasses had not been filled.

7. Mrs. Brown runs a consulting firm and Beth is one of her employees. Today they're giving a presentation to a group of clients. They are in a conference room getting things ready for the presentation when Mrs. Brown (Beth) notices that there are not enough chairs.

8. The Westside basketball team was playing the final game of the season and they were down by five points at half-time. Mark, the team's star forward, had been playing very aggressively and already had three fouls. The coach (Sam, one of the substitutes), thought that if Mark continued to play aggressively and fouled out, they would lose the game.

9. Business had been booming at the Bridle insurance company. Mr. Rate, the manager of the company, (one of the secretaries) realized they had been so busy they had run out of coffee, and coffee was a must for the customers and staff.

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Police Behavior during Traffic and Street Stops, 2011

Lynn Langton, Ph.D., and Matthew Durose, *BJS Statisticians*

In 2011, over 62.9 million U.S. residents age 16 or older, or 26% of the population, had one or more contacts with police during the prior 12 months (**figure 1**). For about half (49%) of persons experiencing contact with police, the most recent contact was involuntary or police-initiated. In 2011, 86% of persons involved in traffic stops during their most recent contact with police and 66% of persons involved in street stops (i.e., stopped in public but not in a moving vehicle) believed that the police both behaved properly and treated them with respect during the contact. A greater percentage of persons involved in street stops (25%) than those pulled over in traffic stops (10%) believed the police had not behaved properly. Regardless of the reason for the stop, less than 5% of persons who believed the police had not behaved properly filed a complaint.

The data in this report were drawn from the Bureau of Justice Statistics' (BJS) 2011 Police-Public Contact Survey (PPCS), a supplement to the National Crime

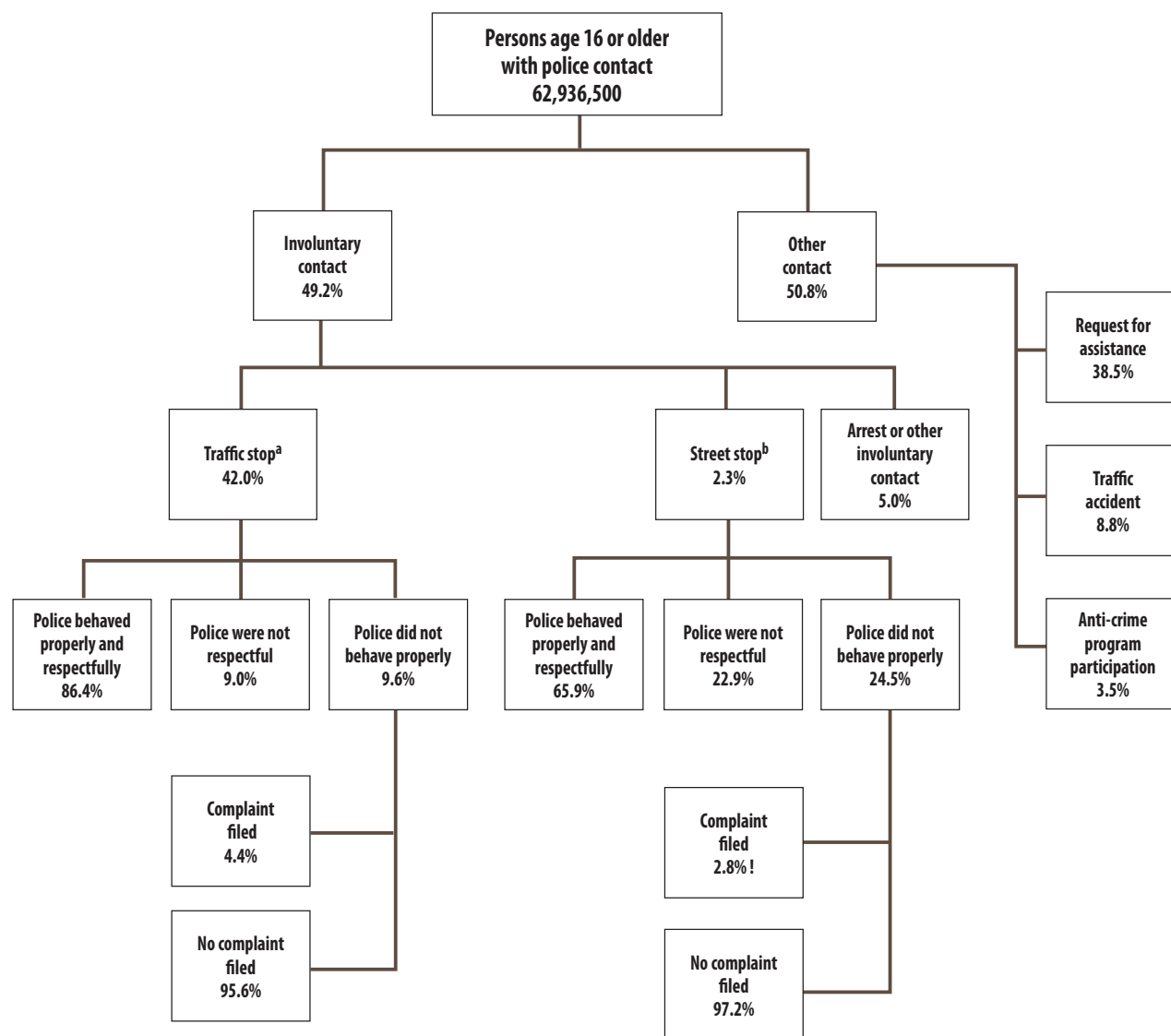
Victimization Survey (NCVS), which collects information from a nationally representative sample of persons in U.S. households. The PPCS collects information on contact with police during a 12-month period. This report examines involuntary contacts with police, specifically those that occurred when the person was the driver of a motor vehicle (i.e., traffic stops) or when the person was stopped by the police while in a public place but not in a moving vehicle (i.e., street stops). It describes variations in perceptions of police behavior and police legitimacy during traffic and street stops. (For more information on how perceptions of police behavior and legitimacy were measured in this report, see survey questions on page 12.) All findings in this report are based on persons for whom the most recent contact in 2011 was in a street stop or as the driver in a traffic stop. For information on voluntary contacts with police, see *Requests for Police Assistance, 2011*, NCJ 242938, BJS website, September 2013.

HIGHLIGHTS

- Relatively more black drivers (13%) than white (10%) and Hispanic (10%) drivers were pulled over in a traffic stop during their most recent contact with police. There were no statistical differences in the race or Hispanic origin of persons involved in street stops.
- Persons involved in street stops were less likely (71%) than drivers in traffic stops (88%) to believe that the police behaved properly.
- Of those involved in traffic and street stops, a smaller percentage of blacks than whites believed the police behaved properly during the stop.
- Drivers pulled over by an officer of the same race or ethnicity were more likely (83%) than drivers pulled over by an officer of a different race or ethnicity (74%) to believe that the reason for the traffic stop was legitimate.
- White drivers were both ticketed and searched at lower rates than black and Hispanic drivers.
- Across race and Hispanic origin, persons who were searched during traffic stops were less likely than persons who were not searched to believe the police behaved properly during the stop.
- About 1% of drivers pulled over in traffic stops had physical force used against them by police. Of these drivers, 55% believed the police behaved properly during the stop.
- About 6 in 10 persons age 16 or older involved in street stops believed they were stopped for a legitimate reason.
- About 19% of persons involved in street stops were searched or frisked by police. The majority of persons who were searched or frisked did not believe the police had a legitimate reason for the search.

FIGURE 1

Perceptions that police behaved properly and respectfully during most recent contact with persons age 16 or older, by type of contact, 2011



Note: Based on the most recent contact with police during the past 12 months. Detail may not sum to 100% due to missing data and multiple responses. See appendix table 1 for estimates and standard errors.

^aIncludes being stopped by police as either a driver or a passenger in a motor vehicle. All other tables focus on the driver of the motor vehicle in a traffic stop.

^bIncludes being stopped by police in a public place, not a moving vehicle.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

About 71% of persons involved in streets stops thought the police behaved properly, compared to 88% of drivers pulled over in traffic stops

In 2011, less than 1% of the 241.4 million U.S. residents age 16 or older were involved in a street stop during their most recent contact with police (table 1; appendix table 2). A greater percentage of males (1%) than females (less than 1%) were involved in street stops during 2011. Persons ages 16 to 24 were more likely than persons age 35 or older to be involved in street stops. While no differences were observed in the percentage of non-Hispanic white, non-Hispanic black, and Hispanic populations age 16 or older involved in a street stop, among those who were stopped, a smaller percentage of blacks (38%) than Hispanics (63%) or whites (78%) felt the police behaved properly during the stop.

Traffic stops were a more common form of police contact than street stops in 2011. About 10% of the 212.3 million U.S. drivers age 16 or older were stopped while operating a

motor vehicle during their most recent contact with police.¹ As with street stops, a greater percentage of male drivers (12%) than female drivers (8%) were pulled over in traffic stops. Across age groups, the highest percentage of stopped drivers was among drivers ages 18 to 24 (18%). A higher percentage of black drivers (13%) than white (10%) and Hispanic (10%) drivers age 16 or older were pulled over in a traffic stop during their most recent contact with police.

A higher percentage of drivers in traffic stops (88%) than persons involved in street stops (71%) believed the police behaved properly during the stop. White drivers pulled over by police (89%) were more likely than black drivers (83%) to think that the police behaved properly, while no difference was observed between the percentages of stopped white drivers and Hispanic drivers who thought that the police behaved properly. There was also no statistical difference in the percentages of black and Hispanic stopped drivers who believed the police behaved properly.

¹The driving population includes persons age 16 or older who reported driving a few or more times during the year or who were stopped as the driver in a traffic stop during 2011.

TABLE 1
Involuntary contact with police among persons age 16 or older, by demographic characteristics and type of contact, 2011

Demographic characteristics	Street stops ^a			Traffic stops ^b		
	Percent of all persons	Percent of stopped persons		Percent of all drivers ^c	Percent of stopped drivers	
		Total	Police behaved properly ^d		Total	Police behaved properly ^d
Total	0.6%	100%	70.7%	10.2%	100%	88.2%
Sex						
Male	0.8%	67.5%	69.8%	11.9%	58.8%	86.9%
Female	0.4	32.5	72.7	8.4	41.2	89.9
Race/Hispanic origin						
White ^e	0.6%	65.2%	77.6%	9.8%	69.3%	89.4%
Black/African American ^e	0.6	12.4	37.7!	12.8	12.6	82.7
Hispanic/Latino	0.7	15.3	62.9	10.4	12.2	86.5
American Indian/Alaska Native ^e	0.5!	0.6!	100!	15.0	0.6	74.2
Asian/Native Hawaiian/other Pacific Islander ^e	0.4!	3.6!	85.0!	9.4	4.0	89.5
Two or more races ^e	1.8!	3.1!	76.6!	13.4	1.3	94.8
Age						
16–17	1.5%	8.5%	67.4%	9.0%	1.8%	92.3%
18–24	1.6	31.7	72.1	17.8	19.5	85.1
25–34	0.9	27.1	64.4	12.7	22.4	88.1
35–44	0.4	10.6	81.6	11.3	19.8	87.9
45–54	0.4	10.9	79.7	9.4	17.9	88.7
55–64	0.2	5.5	62.2!	7.1	11.4	89.7
65 or older	0.2	5.7	68.8!	4.8	7.2	92.3

Note: See appendix table 2 for estimates of the U.S. population and driving population age 16 or older and appendix table 3 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

^aIncludes persons stopped by police during the past 12 months for whom the most recent contact involved being stopped by police on the street or in public, but not in a moving motor vehicle.

^bIncludes persons stopped by police during the past 12 months for whom the most recent contact was as a driver in a traffic stop.

^cPercents based on the driving population age 16 or older, which includes PPCS respondents who reported driving a few times a year or more or were the driver in a traffic stop.

^dDenominator includes approximately 2% of respondents who did not know or did not report whether police behaved properly.

^eExcludes persons of Hispanic or Latino origin.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Traffic stops

Regardless of the reason for the traffic stop, black (67%) and Hispanic (74%) drivers were less likely than white drivers (84%) to believe the reason for the stop was legitimate

In 2011, a greater percentage of white drivers (84%) than Hispanic (74%) or black drivers (67%) who were stopped by police believed they were pulled over for a legitimate reason (table 2). Across all races and Hispanic origin, drivers stopped for speeding were among the most likely to perceive that the reason for the traffic stop was legitimate (90% of white, 83% of Hispanic, and 73% of black drivers). In general, drivers who were pulled over and not given a reason for the traffic stop were the least likely to think the traffic

stop was legitimate. For example, 51% of white drivers who were stopped without the police giving a reason believed the stop was legitimate, whereas 84% who were given a reason believed that the stop was legitimate.

Among other reasons for traffic stops that were associated with comparatively lower perceptions that the stop was legitimate, less than 70% of white (69%), black (69%), and Hispanic (64%) drivers who were pulled over for a stop light or stop sign violation believed the police had a legitimate reason for stopping them. Less than 70% of black drivers stopped due to a vehicle defect (69%), a seatbelt or cell phone violation (64%), or an illegal turn or lane change violation (65%) thought the police had a legitimate reason for stopping them.

TABLE 2

Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race or Hispanic origin of driver and reason for stop, 2011

Reason for traffic stop	Percent of stopped drivers				
	All	White ^a	Black/African American ^a	Hispanic/Latino	Other ^{a,b}
Any reasons	100%	100%	100%	100%	100%
Police gave reason for the stop					
Speeding	46.5	50.1	37.7	39.2	37.3
Vehicle defect	14.1	12.7	19.0	16.5	14.6
Record check	9.7	9.0	14.0	9.7	9.9
Roadside sobriety check	1.3	1.6	0.4!	1.0!	1.0!
Seatbelt or cell phone violation	6.6	6.6	6.5	6.5	7.4
Illegal turn or lane change	7.0	6.6	7.0	7.1	10.8
Stop sign/light violation	6.7	6.1	5.5	9.9	9.4
Other reason ^c	5.1	4.7	5.3	6.8	5.2
Police did not give reason for the stop	3.1	2.6	4.7	3.3	4.2!
Reason for traffic stop	Percent reporting reason for stop was legitimate ^d				
	All	White ^a	Black/African American ^a	Hispanic/Latino	Other ^{a,b}
Any reasons	80.0%	83.6%	67.5%	73.6%	78.4%
Police gave reason for the stop					
Speeding	87.1	89.6	72.8	83.1	87.3
Vehicle defect	81.2	86.4	69.0	74.4	79.3
Record check	80.0	80.9	83.0	70.7	81.2
Roadside sobriety check	79.4	86.0	--!	56.6!	68.1!
Seatbelt or cell phone violation	79.7	84.0	63.8	77.3	69.0!
Illegal turn or lane change	73.0	75.4	65.0	72.6	67.1
Stop sign/light violation	68.4	68.8	69.2	63.6	74.6
Other reason ^c	59.1	65.2	21.6!	61.9	67.8!
Police did not give reason for the stop	44.6	51.0	36.6!	18.3!	59.8!

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 4 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer cases or the coefficient of variation is greater than 50%.

-- Less than 0.05%.

^aExcludes persons of Hispanic or Latino origin.

^bIncludes persons identifying as American Indian, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

^cDenominator includes approximately 3% of white, 6% of black, 3% of Hispanic, and 4% of other race drivers who did not know or did not report whether the reason for the stop was legitimate.

^dIncludes reasons such as reckless driving, littering, failure to yield, following too closely, obstructed license plate, and noise violations.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police–Public Contact Survey, 2011.

A greater percentage of drivers pulled over by an officer of the same race or ethnicity (83%) than drivers stopped by an officer of a different race or ethnicity (74%) believed the reason for the traffic stop was legitimate

About 83% of drivers pulled over by an officer of the same race or Hispanic origin thought the reason for the traffic stop was legitimate, compared to 74% of drivers pulled over by an officer of a different race or Hispanic origin (table 3).² However, drivers' perceptions of traffic stop legitimacy varied somewhat by the reason for the stop and whether the driver and officer were the same race or Hispanic origin. When the reason for the stop was speeding, a vehicle

defect, a roadside sobriety check, or a seatbelt or cell phone violation, drivers pulled over by an officer of a different race or ethnicity were less likely than drivers pulled over by an officer of the same race or ethnicity to perceive the reason for the traffic stop to be legitimate. In comparison, a similar percentage of drivers stopped for a record check, an illegal turn or lane change, or a stop light or stop sign violation perceived the stop to be legitimate, regardless of whether the officer was the same race or ethnicity as the driver or a different race or ethnicity. Whether the driver and officer were intraracial (41%) or interracial (42%), the officer's failure to give a reason for the stop resulted in less than half of stopped drivers believing the stop was legitimate.

²Data on officer race or Hispanic origin are based on respondent's perception.

TABLE 3
Perception that reason for traffic stop was legitimate among drivers age 16 or older, by reason for stop and whether driver and officer were intra- or interracial, 2011

Reason for traffic stop	Intraracial driver and officer		Interracial driver and officer	
	Total stopped drivers	Reason for stop was legitimate ^a	Total stopped drivers	Reason for stop was legitimate ^a
Any reasons	100%	83.3%	100%	74.4%
Police gave reason for the stop				
Speeding	51.1	89.4	42.3	83.6
Vehicle defect	13.0	84.4	16.7	74.6
Record check	8.7	79.3	9.6	80.8
Roadside sobriety check	1.4	83.4	0.4!	38.5!
Seatbelt or cell phone violation	6.1	86.2	7.2	70.4
Illegal turn or lane change	6.6	75.2	7.3	67.7
Stop sign/light violation	6.0	70.9	7.5	62.3
Other reason ^b	4.6	63.6	5.8	45.6
Police did not give reason for the stop	2.4	46.8	3.1	41.8

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Information on the race or Hispanic origin of the officer is based on respondent's perception. Excludes drivers who were stopped by two or more officers of different races or Hispanic origin and officers whose race or Hispanic origin were unknown to the driver. See appendix table 5 for standard errors.

^aDenominator includes approximately 3% of respondents who did not know or did not report whether the reason for the stop was legitimate.

^bIncludes reasons such as reckless driving, littering, failure to yield, following too closely, obstructed license plate, and noise violations.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

While the majority of drivers pulled over in a traffic stop were stopped by white officers, a larger percentage of black drivers (14%) than white (4%) or Hispanic (3%) drivers were stopped by black officers (table 4). Similarly, a greater percentage of Hispanic drivers was stopped by Hispanic officers (17%) than were white (3%) or black (6%) drivers.

A similar percentage of white drivers believed the reason for the stop was legitimate, regardless of whether they were stopped by white, Hispanic, or black officers. While black drivers had similar perceptions of police legitimacy when pulled over by white (70%) or black (71%) officers, a lower percentage of black drivers stopped by Hispanic officers perceived the stop to be legitimate (47%). Among Hispanic drivers, no differences were observed in perceptions of traffic stop legitimacy, regardless of the race or Hispanic origin of the officer.

Among drivers who thought the reason for the stop was not legitimate, 65% believed the police behaved properly, compared to 94% among drivers who thought the stop was legitimate

When the reason for the traffic stop was not seen as legitimate, a smaller percentage of white, black, and Hispanic drivers believed the police behaved properly during the stop than when the reason for the stop was legitimate. Whether the driver and officer were intra- or inter- racial, relatively fewer whites, blacks, and Hispanics thought the police behaved properly when the reason for the stop was perceived to be illegitimate. Regardless of the race or Hispanic of the officer, over 90% of white, black, and Hispanic drivers who believed the stop was legitimate also thought that the police behaved properly. Among white and Hispanic drivers who believed the police had no legitimate reason for the stop, the percentage who also believed that the police behaved properly did not vary, regardless of whether the officer was white, black, or Hispanic. Among black drivers who believed the officer had no legitimate reason for the traffic stop, a higher percentage thought the police behaved properly when the officer was black (87%) than when the officer was white (58%) or Hispanic (55%).

TABLE 4

Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race and ethnicity of driver and officer and driver's perception that police behaved properly, 2011

Race and ethnicity of driver and officer	Percent of all drivers	Total ^a	Reason for stop was legitimate ^b	Percent of stopped drivers	
				Police behaved properly ^c	
				Reason for stop was legitimate	Reason for stop was not legitimate
Total	10.2%	100%	80.0%	93.9%	65.0%
White driver^{d,e}	9.8%	100%	83.6%	93.9%	64.5%
White officer ^d	7.9	81.0	84.0	93.8	67.2
Black/African American officer ^d	0.4	4.3	82.3	96.6	60.3
Hispanic/Latino officer	0.3	3.3	76.5	98.0	63.5
Black/African American driver^{d,e}	12.8%	100%	67.5%	94.2%	58.7%
White officer ^d	8.3	65.3	70.2	93.6	58.3
Black/African American officer ^d	1.8	13.8	70.7	91.6	87.1
Hispanic/Latino officer	0.7	5.7	46.8	100	55.4!
Hispanic/Latino driver^e	10.4%	100%	73.6%	94.7%	60.1%
White officer ^d	6.7	64.9	74.3	94.2	64.1
Black/African American officer ^d	0.3	3.2	74.1	94.2!	64.3!
Hispanic/Latino officer	1.7	16.7	77.4	93.8	54.4!
Other driver^{d,e}	10.5%	100%	78.4%	91.9%	78.7%
White officer ^d	7.6	72.3	80.3	93.1	75.0
Black/African American officer ^d	0.3!	3.1!	52.7!	51.2!	79.4!
Hispanic/Latino officer	0.6!	5.8!	76.9!	100!	100!

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Information on the race or Hispanic origin of the officer is based on driver's perception. See appendix table 6 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation was greater than 50%.

-- Less than 0.05%.

^aPercentages do not sum to 100% due to 11% of white drivers, 15% of black drivers, 15% of Hispanic drivers, and 19% of other race drivers who were stopped by officers identified as American Indian, Alaska Native, Asian, Native Hawaiian, other Pacific Islander, or two or more races; groups of officers of different races and Hispanic origin; and officers whose race or Hispanic origin was unknown to the driver.

^bDenominator includes approximately 3% of respondents who did not know or did not report whether the reason for the stop was legitimate.

^cDenominator includes approximately 1% of drivers who thought the stop was legitimate and 6% of drivers who did not think it was legitimate who did not know or did not report whether the police behaved properly.

^dExcludes persons of Hispanic or Latino origin.

^eIncludes officers identified as American Indian, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

In 2011, there were small racial differences in the percentage of drivers who were ticketed

About 5% of the 212.3 million drivers age 16 or older were pulled over and ticketed in the most recent contact with police (table 5). Another 3% of all drivers were pulled over and given a verbal or written warning and 1% were allowed to proceed with no enforcement action after being stopped.

A greater percentage of male drivers (6%) were ticketed than female drivers (4%), and a greater percentage of black (7%) and Hispanic (6%) drivers were ticketed than white drivers (5%). A greater percentage of black drivers (2%) were stopped and allowed to proceed with a no enforcement action than white (1%) and Hispanic (1%) drivers. A greater percentage of drivers ages 18 to 24 (10%) were ticketed than drivers in any other age group.

The majority (93%) of stopped drivers who were issued a warning believed that the police behaved properly during the stop. Regardless of the demographic characteristics of the driver, 87% to 95% of drivers who were issued a warning

after being stopped believed the police behaved properly. Among drivers of all sexes, ages, races and Hispanic origin ticketed by police, the percentage who believed the police behaved properly ranged from 81% to 90%.

Across most demographic characteristics examined, stopped drivers who were allowed to proceed without any enforcement action were less likely than drivers who were issued a warning to believe the police behaved properly. Since previous findings in this report suggest an association between perceptions of traffic stop legitimacy and perceptions that police behaved properly, this may suggest that drivers were less likely to believe the reason for the stop was legitimate when no enforcement action occurred as a result of the stop.

In 2011, 1% of stopped drivers were arrested during the stop. The majority of arrested drivers also received a ticket or a warning during the stop. Among stopped drivers who were arrested, 76% believed the police behaved properly (not shown in table).

TABLE 5

Enforcement actions taken by police against drivers age 16 or older, by driver's demographic characteristics and perception that police behaved properly, 2011

Race of driver	Percent of all drivers	Ticketed		Percent of all drivers	Warned		Percent of all drivers	Allowed to proceed with no enforcement action	
		Percent of stopped drivers	Police behaved properly ^a		Percent of stopped drivers	Police behaved properly ^a		Percent of stopped drivers	Police behaved properly ^a
All drivers	5.3%	100%	86.6%	3.4%	100%	93.3%	1.4%	100%	82.5%
Sex									
Male	6.2%	58.5%	86.2%	3.9%	57.8%	92.4%	1.7%	61.7%	78.6%
Female	4.4%	41.5%	87.2%	2.9%	42.2%	94.5%	1.1%	38.3%	88.9%
Race/Hispanic origin									
White ^b	4.8%	65.5%	87.5%	3.6%	75.5%	94.2%	1.4%	69.2%	84.5%
Black/African American ^b	7.0%	13.2%	81.1%	3.5%	10.5%	87.4%	2.1%	14.7%	78.9%
Hispanic/Latino	6.2%	14.0%	86.6%	2.8%	9.9%	91.7%	1.3%	11.0%	74.7%
Other ^{b,c}	6.7%	7.2%	89.1%	2.5%	4.1%	94.6%	1.3%	5.1%	83.9%
Age									
16-17	3.9%	1.5%	88.2%	4.2%	2.5%	94.5%	0.9% !	1.3%	100 % !
18-24	9.8%	20.5%	84.4%	5.2%	17.1%	91.5%	2.7%	21.2%	76.9%
25-34	7.2%	24.1%	85.4%	4.1%	21.6%	95.0%	1.4%	17.5%	81.5%
35-44	6.1%	20.3%	86.6%	3.9%	20.2%	92.7%	1.3%	16.9%	79.8%
45-54	4.7%	17.2%	88.3%	3.1%	17.9%	91.8%	1.5%	20.9%	83.5%
55-64	3.4%	10.6%	89.1%	2.4%	11.7%	94.2%	1.2%	13.4%	87.6%
65 or older	2.0%	5.7%	89.9%	2.0%	9.0%	95.4%	0.8%	8.8%	90.8%

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Excludes a small percentage of drivers who were arrested without any other enforcement action (0.4%). See appendix table 7 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

^aDenominator includes approximately 3% of respondents who did not know or did not report whether police behaved properly.

^bExcludes persons of Hispanic or Latino origin.

^cIncludes persons identifying as American Indian, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Among black and Hispanic stopped drivers, a similar percentage of ticketed and not ticketed drivers believed the police behaved properly during the traffic stop

Among drivers stopped in traffic stops, there was no statistical difference in the percentage of white (50%) and black (55%) stopped drivers were ticketed in 2011. Hispanic stopped drivers (60%) were more likely than white stopped drivers to receive a ticket. A greater percentage of white drivers were ticketed when stopped by black officers (64%) than white officers (49%) (table 6). However, for black and Hispanic drivers stopped by police, the percentage issued

a ticket did not vary by the race or Hispanic origin of the officer. These differences and similarities in enforcement practices by race or Hispanic origin of the driver and officer may be related to the reason for the traffic stop or other factors and do not necessarily reflect biased or unbiased treatment.

Among white, black, and Hispanic drivers who were stopped and ticketed, the percentage who believed the police behaved properly did not vary regardless of whether the officer was white, black, or Hispanic. Overall, for most racial and ethnic groups, the majority of stopped drivers believed the police behaved properly whether a ticket was issued or not.

TABLE 6

Stopped drivers age 16 or older who were ticketed, by race of officer and driver and driver's perception that police behaved properly, 2011

Race of driver and officer	Percent of all drivers issued a ticket	Ticketed	Percent of stopped drivers ^a	
			Police behaved properly	
			Ticketed drivers	Drivers not ticketed ^b
White driver^{c,d}	4.8%	49.5%	87.5%	91.2%
White officer ^c	3.9	49.2	87.5	91.6
Black/African American officer ^c	0.3	63.6	86.5	96.7
Hispanic/Latino officer	0.2	50.2	88.3	91.5
Black/African American driver^{c,d}	7.0%	55.1%	81.1%	84.6%
White officer ^c	4.7	56.5	80.5	86.3
Black/African American officer ^c	1.1	63.2	88.9	92.6
Hispanic/Latino officer	0.4	53.4	74.4!	78.4!
Hispanic/Latino driver^d	6.2%	60.2%	86.6%	86.4%
White officer ^c	3.9	58.7	85.0	88.3
Black/African American officer ^c	0.2!	52.9!	91.9!	80.4!
Hispanic/Latino officer	1.0	59.1	88.7	79.4
Other driver^{c,d}	6.7%	64.0%	89.1%	89.3%
White officer ^c	4.8	63.5	89.5	89.5
Black/African American officer ^c	0.3	89.1	60.2!	100!
Hispanic/Latino officer	0.6!	100!	100!	—!

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Information on the race or Hispanic origin of the officer is based on driver's perception. Excludes drivers who were stopped by officers identified as American Indian, Alaska Native, Asian, Native Hawaiian, other Pacific Islander, or two or more races; drivers stopped by groups of officers of different races and Hispanic origin; and officers whose race or Hispanic origin was unknown to the driver. See appendix table 8 for standard errors.

-- Less than 0.05%.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

^aDenominator includes about 2% of respondents who did not know or did not report whether police behaved properly.

^bIncludes drivers who were given a verbal or written warning or allowed to proceed without any enforcement action.

^cExcludes persons of Hispanic or Latino origin.

^dIncludes officers identified as American Indians, Alaska Natives, Asians, Native Hawaiians, or other Pacific Islanders, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Stopped drivers who were searched were less likely than drivers who were not searched to believe that the police behaved properly

In 2011, 3% of drivers pulled over by police in a traffic stop had their person or vehicle searched (table 7). A greater percentage of male drivers (4%) than female drivers (2%) were searched during traffic stops. Male drivers accounted for 76% of searches conducted among stopped drivers. A lower percentage of white drivers stopped by police were searched (2%) than black (6%) or Hispanic (7%) drivers. A greater percentage of stopped drivers ages 18 to 34 (5%) than those age 55 or older (1%) was searched during traffic stops.

Across all demographic groups examined, a smaller percentage of drivers who had their person or vehicle searched by police during a traffic stop than drivers who were not searched believed the police behaved properly. Overall, 61% of searched drivers believed the police behaved properly, compared to 89% of drivers who were stopped but not searched. The percentage of searched drivers who believed the police behaved properly did not vary by sex, race or Hispanic origin, or age.

TABLE 7
Stopped drivers age 16 or older who were searched by police, by driver's demographic characteristics and perception that police behaved properly, 2011

Demographic characteristics	Percent of all stopped drivers searched by police	Percent of stopped drivers ^a		
		Searched	Police behaved properly	
			Searched drivers	Drivers not searched
Total	3.5%	100%	61.3%	89.1%
Sex				
Male	4.5%	75.7%	61.0%	88.1%
Female	2.1	24.3	62.2	90.5
Race/Hispanic origin				
White ^b	2.3%	46.6%	62.4%	90.0%
Black/African American ^b	6.3	22.8	61.6	84.1
Hispanic/Latino	6.6	23.1	64.8	88.1
Other ^{b,c}	4.4!	7.4!	42.5!	91.3
Age				
16-17	1.4%!	0.7%!	--%!	93.5%
18-34	4.8	58.0	58.7	88.1
35-54	3.1	33.8	69.8	88.9
55 or older	1.4!	7.5!	49.0!	91.3

Note: Includes respondents for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 9 for standard errors.

-- Less than 0.05%.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

^aDenominator includes about 6% of searched drivers and 2% of other stopped drivers who did not know or did not report whether the police behaved properly.

^bExcludes persons of Hispanic or Latino origin.

^cIncludes persons identifying as Native American, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

When the police did not ask permission to conduct a search, less than half of searched drivers thought the officers behaved properly during the traffic stop

Less than half (46%) of drivers believed the police behaved properly when a person or vehicle search was conducted without the police first asking permission to conduct the search or without the police having a perceived legitimate reason to conduct the search (table 8). When the police asked permission before conducting a search during a traffic stop, a greater percentage of drivers believed the police behaved properly (72%). Similarly, a greater percentage of drivers thought the police behaved properly when they believed the police had a legitimate reason for conducting the search (86%) than when the reason for the search was not seen as legitimate (46%). About 6% of searched drivers reported that the police uncovered illegal items during the search (not shown in table).

More than half of drivers who experienced police use of physical force or verbal threats thought police behaved properly

In 2011, 6% of drivers pulled over in traffic stops experienced some type of force used against them, from shouting and cursing, to verbal threats of force or other action, to physical force, including hitting, handcuffing, and pointing a gun (table 9). Of the 1% of stopped drivers who experienced physical force during the traffic stop, more than half (55%) believed the police behaved properly during the contact. A similar percentage of drivers who experienced verbal threats of force believed the police behaved properly (56%).

Three in 4 (75%) stopped drivers who experienced any type of force believed the police actions were unnecessary. About two-thirds (65%) of drivers who experienced police use of force did not think the force was excessive. Among stopped drivers who experienced any type of verbal or physical force, 83% who believed the force used or threatened against them was necessary also thought police behaved properly, compared to 38% of those who did not believe the use of force was necessary.

TABLE 8
Stopped drivers age 16 or older who had their person or vehicle searched by police, by perception that police behaved properly, 2011

Stop characteristics	Percent of stopped drivers	
	Total	Police behaved properly ^a
All stops	100%	88.2%
Police searched driver or vehicle		
No	96.4%	89.3%
Yes	3.5	61.4
Percent of searched drivers		
Police asked permission to search ^b		
No	40.2%	46.0%
Yes	59.8	71.8
Driver thought search was legitimate ^b		
No	61.4%	46.1%
Yes	38.6	85.8

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 10 for standard errors.

^aDenominator includes about 6% of searched and 2% of other stopped drivers not searched who did not know or did not report whether the police behaved properly.

^bBased on the number of drivers who experienced a personal or vehicular search.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

TABLE 9
Type of force used or threatened by police against stopped drivers age 16 or older, by perception that police behaved properly, 2011

Type of force	Percent of stopped drivers	
	Total	Police behaved properly ^a
All stops	100%	88.2%
Force used		
Shouting or cursing ^b	1.2%	22.0% !
Verbal threats ^c	3.4	56.4
Physical force ^d	1.5	55.0
Percent of drivers who experienced force		
Driver thought use of force was necessary ^e		
No	74.7%	38.4%
Yes	19.1	83.3
Driver thought use of force was excessive ^e		
No	64.6%	68.3%
Yes	33.3	12.4 !

Note: Includes persons for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 11 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

^aDenominator includes about 3% of respondents who did not know or did not report whether the police behaved properly.

^bExcludes stopped drivers who experienced verbal threats or physical force.

^cIncludes threats of arrest, ticketing, or use of force. Excludes stopped drivers who experienced physical force used against them.

^dIncludes pushing, grabbing, hitting, kicking, handcuffing, using chemical or pepper spray, using an electroshock weapon, or pointing a gun.

^eBased on stopped drivers who had force (shouting or cursing, verbal threats, or physical force) used against them by police. Percent of stopped drivers does not sum to 100% due to persons who did not know whether the use of force was necessary or excessive.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Street stops

About 6 in 10 persons involved in street stops believed they were stopped for a legitimate reason

In 2011, less than 1% of persons age 16 or older were stopped by the police while in a public place (**table 10**). Among persons stopped by the police in a street stop during their most recent police contact, at least 41% were stopped because the police suspected them of something or they matched the description of someone for whom the police were looking. At least 16% of persons involved in street stops said the police did not provide a reason for the stop or the police were seeking information about another person or investigating a crime, and at least 7% were stopped because the police were providing a service. About 20% of persons involved in street stops did not report a reason for the stop.

Overall, 64% of persons involved in street stops believed the police stopped them for a legitimate reason. Among persons who were stopped because the police suspected them of something, 61% thought the reason for the stop was legitimate. The percentage of persons who thought the reason for the stop was legitimate was higher among those who were stopped because the police were providing a service (91%) or seeking information or investigating a crime (92%). Similarly, compared to those who were stopped because the police were investigating a crime (90%) or were providing assistance (96%), a lower percentage of persons stopped because they were suspected of something believed the police behaved properly (68%).

TABLE 10

Reason for street stops involving persons age 16 or older, by perceptions that stop was legitimate and police behaved properly, 2011

Reason for street stop	Percent of all persons	Percent of stopped persons		
		Total	Reason for stop was legitimate ^a	Police behaved properly ^b
Any reasons	0.6%	100%	64.1%	70.7%
Suspected of something or matched description of someone police were looking for ^c	0.2	40.7	60.8	68.5
Police were seeking information about another person or investigating a crime	0.1	15.5	92.1	89.8
Police were providing a service	--	6.9	90.8	95.9
No reason given by police	0.1	16.5	29.7	49.0
Unknown ^d	0.1	20.4	68.1	69.8

Note: Based on persons for whom the most recent contact with police involved being stopped by police in public or on the street, not in a moving vehicle. See appendix table 12 for standard errors.

-- Less than 0.05%.

^aDenominator includes less than 1% of respondents who did know or did not report whether police had a legitimate reason for the stop.

^bDenominator includes about 3% of respondents who did know or did not report whether police behaved properly.

^cIncludes street stops in which the respondent was with someone who the police suspected of something or who matched the description of someone for whom they were looking.

^dNo reason reported.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Regardless of the reason for the stop, a slightly higher percentage of persons involved in a street stop with an officer of the same race or ethnicity believed that the police behaved properly (79%) than persons stopped by an officer of a different race or ethnicity (62%) (table 11). Persons least likely to believe the police behaved properly during street stops were those stopped for reasons they did not believe were legitimate (38%), persons who were searched without a perceived legitimate reason (29%), and persons who had force used against them (30%).³

³Due to small sample sizes, the percentage of persons who had illegal items uncovered in the search could not be calculated.

TABLE 11

Characteristics of persons age 16 or older involved in street stops and outcomes of the stop, by perceptions that police behaved properly, 2011

Stop characteristics	Percent of stopped persons	
	Total	Police behaved properly ^a
All stops	100%	70.7%
Officer and respondent were the same race or Hispanic origin ^b		
No	22.4%	62.3%
Yes	54.5	78.8
Unknown ^c	23.1	59.8
Reason for stop was legitimate		
No	35.6%	37.5%
Yes	64.1	89.5
Searched or frisked		
No	78.9%	76.6%
Yes	19.1	53.8
Person thought search was legitimate		
No	11.1%	29.5%
Yes	8.0	87.9
Force used		
No	74.6%	84.5%
Yes	25.4	30.3
Person thought force was excessive		
No	17.3%	44.5%
Yes	7.0	-- !

Note: Based on persons for whom the most recent contact with police involved being stopped by police in public or on the street, not in a moving vehicle. See appendix table 13 for standard errors.

-- Less than 0.05%.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

^aDenominator includes about 4% of respondents who did know or did not report whether police behaved properly.

^bInformation on the race or Hispanic origin of the officer is based on the person's perception.

^cIncludes person who were stopped by two or more officers of different races or Hispanic origin and officers whose race or Hispanic origin was unknown to the person.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011

Police-Public Contact Survey questions pertaining to perceptions of police behavior and legitimacy of police actions

Perceptions of police behavior

Q. Looking back in this contact, do you feel the police behaved properly?

Perceptions of legitimacy of stop

Q. Would you say that the police officer(s) had a legitimate reason for stopping you?

Perceptions of legitimacy of search

Q. Do you think the police officers had a legitimate reason to search the vehicle (asked of drivers in traffic stops only)?

Q. Do you think that police officers had a legitimate reason to search you, frisk you, or pat you down?

Perceptions of police use of force

Q. Did you feel that this/these action(s) [used by police against you] was/were necessary?

Q. Did you feel any of the force used or force threatened against you was excessive?

Methodology

Data collection

The Police-Public Contact Survey (PPCS) is a supplement to the National Crime Victimization Survey (NCVS). The NCVS annually collects data on crime reported and not reported to the police against persons age 12 or older from a nationally representative sample of U.S. households. The sample includes persons living in group quarters (such as dormitories, rooming houses, and religious group dwellings) and excludes persons living in military barracks and institutional settings (such as correctional or hospital facilities) and the homeless. (For more information, see the *Survey Methodology in Criminal Victimization in the United States, 2008*, NCJ 231173, BJS website, May 2011.)

Since 1999, the PPCS has been administered every 3 years at the end of the NCVS interview to persons age 16 or older within households sampled for the NCVS. Proxy responders and those who complete the NCVS interview in a language other than English were not eligible to receive the PPCS.

The U.S. Census Bureau administered the 2011 PPCS questionnaire between July 1, 2011, and December 31, 2011, and processed the survey data. Respondents were provided a list of specific reasons for having contact with police and were asked if they had experienced any of those types of contacts during the prior 12 months. For example, persons interviewed in July 2011 were asked about contacts that occurred between August 2010 and July 2011. Persons who said they had a contact during 2011 were asked to describe the nature of the contact, and those who had more than one contact were asked about only their most recent contact during the period. To simplify the discussion of the findings, this report describes all contacts reported during the 12 months prior to the interviews as 2011 contacts.

PPCS nonrespondents consisted of persons whose household did not respond to the NCVS (NCVS household nonresponse), persons within an interviewed NCVS household who did not respond to the NCVS (NCVS person nonresponse), and persons who responded to the NCVS but did not complete the PPCS (PPCS person nonresponse). The NCVS household response rate was 89% and the person response rate was 88%. In 2011, PPCS interviews were obtained from 49,246 of the 62,280 individuals age 16 or older in the NCVS sample (79%). A total of 13,034 nonrespondents were excluded from the 2011 PPCS as noninterviews or as proxy interviews. Noninterviews (10,907) included respondents who were not available for the interview, those who refused to participate, and non-English-speaking respondents. (Unlike the NCVS interviews, PPCS interviews were conducted only in English.) The remaining 2,127 were proxy interviews representing household members who were unable to participate for physical, mental, or other reasons.

To produce national estimates on police-public contacts, sample weights were applied to the survey data so that the respondents represented the entire population, including the nonrespondents. After adjustment for nonresponse, the sample cases in 2011 were weighted to produce a national population estimate of 241,404,142 persons age 16 or older.

Despite the nonresponse adjustments, low overall response rates and response rates to particular survey items can still increase variance in these estimates and produce bias when the nonrespondents have characteristics that differ from the respondents. The Office of Management and Budget guidelines require a nonresponse bias study when the overall response rate is below 80%. The Bureau of Justice Statistics (BJS) and the Census Bureau compared the distributions of respondents as well as nonrespondents and nonresponse estimates for various household and demographic characteristics, and examined their impact on the national estimates produced for the 2011 PPCS. The study looked at household-level and person-level response rates and found some evidence of bias in the rates among blacks and persons of Hispanic origin. Blacks accounted for 12% of the U.S. population in 2011 but about 11% of PPCS respondents after weighting adjustments. Hispanics accounted for 14% of the U.S. population but about 12% of the PPCS respondents after weighting adjustments. Because the largest bias in person nonresponse was observed in the Hispanic origin characteristics, future iterations of the PPCS will address this issue by administering the survey in languages other than English and including Hispanic origin as a factor in the noninterview adjustment. Item nonresponse statistics were also computed for key survey questions from the PPCS, and no evidence of bias was found during the analysis.

Changes to the 2011 PPCS

Since its inception in 1996, the PPCS has captured information about in-person (i.e., face-to-face) contacts between police and the public. Telephone contacts were previously not included. The survey also excluded face-to-face interactions in which persons approached an officer or an officer initiated contact with them in a social setting or because their work brought them into regular contact. In March 2010, BJS hosted a series of meetings with subject-matter experts in the area of policing and police legitimacy to initiate discussion and work on substantive changes to the PPCS questionnaire. In 2011, based in part on these meetings, the PPCS was revised to expand the scope of the survey and to better capture contacts with police.

First, to determine if contact occurred and to enhance individuals' recollections about their interactions with police over a 12-month period, BJS implemented new screening procedures in the 2011 PPCS that describe a broad range of situations known to bring people in contact with police. Second, the scope of the PPCS was expanded to collect information about interactions that people had with the police that did not result in a face-to-face contact

(e.g., reporting a crime to the police by phone or email). Additionally, a new set of questions was added to the instrument to collect detailed information about requests for police assistance (e.g., reporting a crime or noncrime emergency) and contacts in which the police stopped someone in a public place or on the street but not in a motor vehicle (known as street stops).

These revisions, which included adding new questions and reordering existing questions, were significant when compared to the 2008 version of the questionnaire. To assess the impact of the survey redesign on trends in rates and types of contact, BJS administered a split-sample design in which a subset of the sample was interviewed using the 2008 version of the questionnaire, and the remaining sample was interviewed using the 2011 version. Based on the evaluation, it was determined that a 15/85 split would provide sufficient power to measure a 15% change in contact rate. In other words, about 85% of the 2011 sample was randomly assigned the revised questionnaire and the other 15% received the questionnaire designed for the 2008 survey. The Census Bureau completed interviews for 41,408 (79%) of the 52,529 residents who received the revised questionnaire.

The findings in this report are based on data collected from the revised questionnaire. An evaluation of the impact of the changes to the 2011 PPCS instrument on trends in contacts between the police and the public is underway, and the results of that assessment will be made available through the BJS website.

Standard error computations

When national estimates are derived from a sample, as is the case with the PPCS, caution must be taken when comparing one estimate to another estimate. Although one estimate may be larger than another, estimates based on a sample have some degree of sampling error. The sampling error of an estimate depends on several factors, including the amount of variation in the responses, the size of the sample, and the size of the subgroup for which the estimate is computed. When the sampling error around the estimates is taken into consideration, the estimates that appear different may, in fact, not be statistically different.

One measure of the sampling error associated with an estimate is the standard error. The standard error can vary from one estimate to the next. In general, for a given metric, an estimate with a smaller standard error provides a more reliable approximation of the true value than an estimate with a larger standard error. Estimates with relatively large standard errors are associated with less precision and reliability and should be interpreted with caution.

In order to generate standard errors around estimates from the PPCS, the Census Bureau produces generalized variance function (GVF) parameters for BJS. The GVFs take into account aspects of the NCVS complex sample design

and represent the curve fitted to a selection of individual standard errors based on the Jackknife Repeated Replication technique. The GVF parameters were used to generate standard errors for each point estimate (i.e., numbers or percentages) in the report.

In this report, BJS conducted tests to determine whether differences in estimated numbers and percentages were statistically significant once sampling error was taken into account. Using statistical programs developed specifically for the NCVS, all comparisons in the text were tested for significance. The primary test procedure used was Student's t-statistic, which tests the difference between two sample estimates. To ensure that the observed differences between estimates were larger than might be expected due to sampling variation, the significance level was set at the 95% confidence level.

Data users can use the estimates and the standard errors of the estimates provided in this report to generate a confidence interval around the estimate as a measure of the margin of error. The following example illustrates how standard errors can be used to generate confidence intervals:

According to the NCVS, in 2011, an estimated 88.2% of drivers stopped by police in traffic stops believed that the police behaved properly during the contact (see table 1). Using the GVFs, BJS determined that the estimate has a standard error of 1.13 (see appendix table 3). A confidence interval around the estimate was generated by multiplying the standard errors by ± 1.96 (the t-score of a normal, two-tailed distribution that excludes 2.5% at either end of the distribution). Thus, the confidence interval around the estimate is $88.2 \pm (1.13 \times 1.96)$ or 86.1 to 90.4. In other words, if different samples using the same procedures were taken from the U.S. population in 2011, 95% of the time the percentage of stopped drivers who believed the police behaved properly would be between 86% and 90%.

In this report, BJS also calculated a coefficient of variation (CV) for all estimates, representing the ratio of the standard error to the estimate. CVs provide a measure of reliability and a means to compare the precision of estimates across measures with differing levels or metrics. In cases where the CV was greater than 50%, or the unweighted sample had 10 or fewer cases, the estimate was noted with a "!" symbol (interpret data with caution; estimate is based on 10 or fewer sample cases, or the coefficient of variation exceeds 50%).

Many of the variables examined in this report may be related to one another and to other variables not included in the analyses. Complex relationships among variables were not fully explored in this report and warrant more extensive analysis. Readers are cautioned not to draw causal inferences based on the results presented.

APPENDIX TABLE 1

Standard errors and estimates for figure 1: Perceptions that police behaved properly and respectfully during most recent contact with persons age 16 or older, by type of contact, 2011

Stop characteristic	Number of persons 16 or older	Standard error	
		Number	Percent
Any contact	62,936,500	1,581,523	~%
Involuntary contact	30,954,800	1,079,805	1.2%
Traffic stop	26,404,200	982,622	1.1
Driver thought police did not behave properly	2,547,600	218,913	0.7
Compliant filed			
Yes	110,900	31,184	1.2
No	2,436,700	212,649	1.5
Driver thought police were not respectful	2,371,700	208,937	0.7
Driver thought police were proper and respectful	22,808,700	899,273	1.1
Street stop	1,433,300	150,720	0.5
Person thought police did not behave properly	351,800	62,186	3.5
Compliant filed			
Yes	10,000	8,183	2.3
No	341,800	61,097	2.6
Person thought police were not respectful	327,700	59,542	3.4
Person thought police were proper and respectful	944,600	115,368	4.1
Arrest or other involuntary contact	3,117,300	249,752	0.7
Other contact	31,981,800	1,100,593	1.2%
Voluntary	24,227,400	932,916	1.3
Traffic accident	5,533,100	363,573	1.0
Anti-crime program participation	2,221,300	200,205	0.6

Note: Detail based on the most recent contact during the past 12 months. Detail may not sum to total due to missing data or categories that are not mutually exclusive. Estimates rounded to the nearest hundred.

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 2

Population of persons age 16 or older and driving population age 16 or older, by demographic characteristics, 2011

Demographic characteristics	Population age 16 or older	Driving population age 16 or older
Total	241,404,142	212,298,850
Sex		
Male	118,267,679	106,632,822
Female	123,136,463	105,666,027
Race/Hispanic origin		
White*	167,364,010	153,358,921
Black/African American*	27,763,474	21,322,976
Hispanic/Latino	31,240,097	25,495,436
American Indian/Alaska Native*	1,058,592	845,043
Asian/Native Hawaiian/other Pacific Islander*	11,447,990	9,168,427
Two or more races*	2,529,979	2,108,046
Age		
16-17	8,060,403	4,323,648
18-24	28,743,383	23,714,718
25-34	41,829,412	38,016,545
35-44	40,680,390	37,756,084
45-54	44,353,446	41,172,146
55-64	37,837,219	34,884,444
65 or older	39,899,889	32,431,265

Note: See appendix table 3 for standard errors.

*Excludes persons of Hispanic or Latino origin.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 3

Standard errors for table 1: Involuntary contact with police among persons age 16 or older, by demographic characteristics and type of contact, 2011

Demographic characteristics	Street stops				Traffic stops			
	Population age 16 or older	Percent of all persons	Percent of stopped persons		Driving population age 16 or older	Percent of all drivers	Percent of stopped drivers	
			Total	Police behaved properly			Total	Police behaved properly
Total	22,320	0.1%	~%	3.9%	1,380,721	0.4%	~%	1.1%
Sex								
Male	1,968,304	0.1%	4.1%	4.6%	1,928,128	0.5%	1.6%	1.4%
Female	1,978,921	0.1	3.9	6.1	1,923,848	0.4	1.6	1.4
Race/Hispanic origin								
White	1,899,202	0.1%	4.1%	4.3%	1,960,731	0.4%	1.6%	1.2%
Black/African American	1,012,560	0.1	2.6	9.8!	862,900	0.9	0.9	2.6
Hispanic/Latino	1,085,620	0.1	2.8	9.0	962,142	0.8	0.9	2.4
American Indian/Alaska Native	124,069	0.6!	0.5	~!	107,481	3.5	0.2	10.5
Asian/Native Hawaiian/other								
Pacific Islander	582,997	0.2!	1.3	13.0!	505,194	1.1	0.5	3.3
Two or more races	217,923	0.7!	1.3	16.4!	193,506	2.3	0.2	3.8
Age								
16–17	464,692	0.4%	2.1%	11.4%	309,377	1.4%	0.3%	4.0%
18–24	1,033,650	0.2	3.8	6.2	920,877	1.1	1.2	2.1
25–34	1,281,640	0.2	3.6	7.0	1,215,222	0.8	1.2	1.8
35–44	1,262,079	0.1	2.4	8.6	1,210,524	0.7	1.2	1.9
45–54	1,323,296	0.1	2.4	8.8	1,270,498	0.6	1.1	1.9
55–64	1,211,990	0.1	1.7	14.4!	1,157,274	0.6	0.9	2.2
65 or older	1,248,572	0.1	1.7	13.6!	1,109,568	0.5	0.7	2.3

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 4

Standard errors for table 2: Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race or Hispanic origin of driver and reason for stop, 2011

Reason for traffic stop	Percent of stopped drivers				
	All	White	Black/African American	Hispanic/Latino	Other
Any reasons	~%	~%	~%	~%	~%
Police gave reason for the stop					
Speeding	1.6	1.8	3.2	0.5	4.2
Vehicle defect	1.0	1.1	2.4	0.3	2.9
Record check	0.8	0.9	2.1	0.2	2.4
Roadside sobriety check	0.2	0.3	0.3!	0.1!	0.7!
Seatbelt or cell phone violation	0.6	0.7	1.4	0.2	2.1
Illegal turn or lane change	0.7	0.7	1.5	0.2	2.5
Stop sign/light violation	0.6	0.7	1.3	0.2	2.3
Other reason	0.6	0.6	1.3	0.2	1.7
Police did not give reason for the stop	0.4	0.4	1.2	0.1	1.6!
Percent reporting reason for stop was legitimate					
Reason for traffic stop	All	White	Black/African American	Hispanic/Latino	Other
Any reasons	1.4%	1.4%	3.2%	3.1%	3.8%
Police gave reason for the stop					
Speeding	1.5	1.5	4.4	3.7	4.5
Vehicle defect	2.6	2.7	6.0	6.1	8.2
Record check	3.0	3.5	5.6	8.0	9.4
Roadside sobriety check	6.8	6.4	~!	24.6!	32.5!
Seatbelt or cell phone violation	3.5	3.7	9.9	8.8	12.7!
Illegal turn or lane change	3.8	4.3	9.5	9.0	10.8
Stop sign/light violation	4.0	4.8	10.3	8.4	10.7
Other reason	4.7	5.4	9.0!	9.9	15.0!
Police did not give reason for the stop	5.7	7.2	11.3!	10.6!	17.3!

! Interpret with caution. Estimate based on 10 or fewer cases or the coefficient of variation is greater than 50%.

~ Not applicable.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 5

Standard errors for table 3: Perception that reason for traffic stop was legitimate among drivers age 16 or older, by reason for stop and whether driver and officer were intra- or interracial, 2011

Reason for traffic stop	Intraracial driver and officer		Interracial driver and officer	
	Total stopped drivers	Reason for stop was legitimate	Total stopped drivers	Reason for stop was legitimate
All reasons	~%	1.5%	~%	2.3%
Police gave reason for the stop				
Speeding	1.9	1.6	2.5	2.6
Vehicle defect	1.1	3.0	1.7	4.4
Record check	0.9	3.9	1.3	5.0
Roadside sobriety check	0.3	7.7	0.2 !	25.8 !
Seatbelt or cell phone violation	0.7	3.8	1.1	6.5
Illegal turn or lane change	0.8	4.6	1.1	6.6
Stop sign/light violation	0.7	5.0	1.1	6.7
Other reason	0.6	5.9	1.0	7.6
Police did not give reason for the stop	0.4	7.9	0.7	9.9

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 6

Standard errors for table 4: Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race and ethnicity of driver and officer and driver's perception that police behaved properly, 2011

Race and ethnicity of driver and officer	Percent of all drivers	Percent of stopped drivers			
		Total	Reason for stop was legitimate	Police behaved properly	
				Reason for stop was legitimate	Reason for stop was not legitimate
Total	0.4%	~%	1.4%	0.9%	2.7%
White driver	0.4%	~%	1.4%	1.0%	3.4%
White officer	0.4	1.5	1.5	1.1	3.6
Black/African American officer	0.1	0.6	4.6	2.4	12.2
Hispanic/Latino officer	0.1	0.5	5.6	2.1	11.9
Black/African American driver	0.9%	~%	3.2%	1.9%	5.1%
White officer	0.7	3.2	3.7	2.3	6.3
Black/African American officer	0.3	2.1	6.8	4.8	8.6
Hispanic/Latino officer	0.2	1.3	10.8	~	14.4 !
Hispanic/Latino driver	0.8%	~%	3.0%	1.8%	5.6%
White officer	0.6	3.3	3.6	2.2	6.7
Black/African American officer	0.1	1.0	12.7	7.8 !	26.0 !
Hispanic/Latino officer	0.3	2.3	5.8	3.8	13.2
Other driver	1.0%	~%	3.8%	2.8%	7.0%
White officer	0.9	4.1	4.1	2.9	8.9 !
Black/African American officer	0.1 !	1.3 !	20.3 !	27.5 !	23.6 !
Hispanic/Latino officer	0.2 !	1.8 !	12.9 !	~ !	~ !

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation was greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 7

Standard errors for table 5: Enforcement actions taken by police against drivers age 16 or older, by driver's demographic characteristics and perception that police behaved properly, 2011

Race of driver	Ticketed			Warned			Allowed to proceed with no enforcement action		
	Percent of all drivers	Percent of stopped drivers		Percent of all drivers	Percent of stopped drivers		Percent of all drivers	Percent of stopped drivers	
		Ticketed drivers	Police behaved properly		Warned drivers	Police behaved properly		Drivers with no enforcement	Police behaved properly
All drivers	0.3%	~%	1.5%	0.2%	~%	1.2%	0.1%	~%	2.5%
Sex									
Male	0.4%	2.0%	1.8%	0.3%	2.4%	1.6%	0.2%	3.2%	3.2%
Female	0.3	2.0	1.9	0.2	2.3	1.5	0.1	3.1	3.0
Race/Hispanic origin									
White	0.3%	2.0%	1.6%	0.2%	2.1%	1.3%	0.1%	3.0%	2.8%
Black/African American	0.7	1.2	3.4	0.4	1.2	3.7	0.3	2.1	5.7
Hispanic/Latino	0.6	1.2	2.9	0.4	1.2	3.2	0.2	1.8	6.8
Other	0.8	0.9	3.4	0.4	0.7	3.7	0.3	1.2	8.2
Age									
16-17	0.9%	0.3%	6.8%	0.9%	0.6%	4.7%	0.4% !	0.6%	~% !
18-24	0.8	1.5	2.7	0.5	1.6	2.6	0.4	2.5	5.1
25-34	0.6	1.6	2.4	0.4	1.8	1.9	0.2	2.2	5.1
35-44	0.5	1.5	2.5	0.4	1.7	2.3	0.2	2.2	5.3
45-54	0.4	1.4	2.5	0.3	1.6	2.5	0.2	2.4	4.5
55-64	0.4	1.1	2.9	0.3	1.3	2.5	0.2	2.0	4.8
65 or older	0.3	0.7	3.6	0.3	1.1	2.5	0.2	1.6	5.1

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 8

Standard errors for table 6: Stopped drivers age 16 or older who were ticketed, by race of officer and driver and driver's perception that police behaved properly, 2011

Race of driver and officer	Percent of all drivers issued a ticket	Percent of stopped drivers		
		Ticketed	Police behaved properly	
			Ticketed drivers	Drivers not ticketed
White driver	0.3%	1.8%	1.6%	1.4%
White officer	0.2	2.0	1.7	1.5
Black/African American officer	0.0	5.7	4.9	3.3
Hispanic/Latino officer	0.0	6.5	5.7	5.0
Black/African American driver	0.7%	3.3%	3.4%	3.4%
White officer	0.5	3.9	4.0	3.8
Black/African American officer	0.2	7.1	5.7	6.1
Hispanic/Latino officer	0.1	10.8	12.7 !	12.7 !
Hispanic/Latino driver	0.6%	3.3%	2.9%	3.4%
White officer	0.4	3.9	3.6	3.7
Black/African American officer	0.1 !	14.3 !	10.6 !	16.3 !
Hispanic/Latino officer	0.2	6.8	5.6	8.3
Other driver	0.8%	4.3%	3.4%	4.3%
White officer	0.7	4.9	3.8	4.8
Black/African American officer	0.1	12.8	21.1 !	~ !
Hispanic/Latino officer	0.2 !	~ !	~ !	~ !

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 9

Standard errors for table 7: Stopped drivers who were searched by police, by driver's demographic characteristics and perception that police behaved properly, 2011

Demographic characteristics	Percent of all drivers searched by police	Percent of stopped drivers		
		Searched	Police behaved properly	
			Searched drivers	Drivers not searched
Total	0.4%	~%	5.8%	1.2%
Sex				
Male	0.6%	4.8%	6.5%	1.4%
Female	0.5	4.5	10.6	1.4
Race/Hispanic origin				
White	0.4%	5.5%	7.9%	1.2%
Black/African American	1.4	4.4	10.9	2.8
Hispanic/Latino	1.4	4.4	10.7	2.5
Other	1.6	2.6	18.4!	2.8
Age				
16-17	1.5%!	0.8%!	~%!	3.9%
18-34	0.7	5.5	7.3	1.6
35-54	0.6	5.1	8.7	1.6
55 or older	0.5	2.6	18.6!	1.8

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 10

Standard errors for table 8: Stopped drivers age 16 or older who had their person or vehicle searched by police, by driver's perception that police behaved properly, 2011

Stop characteristics	Percent of stopped drivers	
	Total	Police behaved properly
All stops	~%	1.1%
Police searched driver or vehicle		
No	0.7%	1.1%
Yes	0.4	5.4
	Percent of searched drivers	
Police asked permission to search		
No	5.3%	8.0%
Yes	5.4	6.2
Driver thought search was legitimate		
No	5.4%	6.7%
Yes	5.3	5.8

~ Not applicable.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 11

Standard errors for table 9: Type of force used or threatened by police against stopped drivers, by driver's perception that police behaved properly, 2011

Type of force	Percent of stopped drivers	
	Total	Police behaved properly
All stops	~%	1.1%
Force used		
Shouting or cursing	0.2%	6.8%!
Verbal threats	0.4	5.6
Physical force	0.3	7.7
	Percent of drivers who experienced force	
Driver thought use of force was necessary		
No	3.9%	4.7%
Yes	3.2	6.6
Driver thought use of force was excessive		
No	4.2%	4.9%
Yes	4.0	4.2!

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 12

Standard errors for table 10: Reason for street stops involving persons age 16 or older, by perceptions that stop was legitimate and police behaved properly, 2011

Reason for street stop	Percent of all persons in a street stop	Percent of stopped persons		
		Total	Reason for stop was legitimate	Police behaved properly
Any reasons	0.1%	~%	4.1%	3.9%
Suspected of something or matched description of someone police were looking for	0.0	4.1	6.0	5.8
Police were seeking information about another person or investigating a crime	0.0	2.8	5.1	5.7
Police were providing a service	~	1.9	7.8	5.3
No reason given by police	0.0	2.9	8.1 !	9.0
Unknown	0.0	3.2	7.7	7.6

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 13

Standard errors for table 11: Characteristics of persons age 16 or older involved in street stops and outcomes of the stop, by perceptions that police behaved properly, 2011

Stop characteristics	Percent of stopped persons	
	Total	Police behaved properly
All stops	~%	3.9%
Person thought officer and respondent were the same race or Hispanic origin		
No	3.4%	7.7%
Yes	4.2	4.5
Unknown	3.4	7.7
Person thought reason for stop was legitimate		
No	4.0%	6.2%
Yes	4.1	3.2
Searched or frisked		
No	3.6%	4.0%
Yes	3.1	8.4
Person thought search was legitimate		
No	5.0%	9.6%
Yes	4.3	8.2
Force used		
No	3.8%	3.6%
Yes	3.5	6.7
Person thought force was excessive		
No	5.4%	8.7%
Yes	3.5	0.0 !

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.



The Bureau of Justice Statistics, located in the Office of Justice Programs, U.S. Department of Justice, collects, analyses, and disseminates statistical information on crime, criminal offenders, victims of crime, and the operation of justice systems at all levels of government. William J. Sabol is acting director.

This report was written by Lynn Langton and Matthew Durose. Matthew Durose verified the report.

Morgan Young edited the report. Barbara Quinn and Morgan Young produced the report.

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Portland Police Bureau_____

STOPS DATA COLLECTION

The Portland Police Bureau's response to the Criminal Justice Policy and Research Institute's recommendations

Presented to the Community Police Relations Committee

February 13, 2014

Sgt. Greg Stewart
Emily Covelli M.S.



Charlie Hales Mayor
Michael Reese Chief of Police

2011 Data analyses

Data

The following analyses on stops and searches utilize the PPB's stop and search data from the date of August 5 to December 31, 2011. The initial dataset consisted of 31,143 records. 5,531 records were unusable and removed from the data set for the following reasons:

- 3,432 were duplicate records or the stop was cancelled (Table 2 below)
- 1,879 occurred prior to noon on August 5, 2011¹
- 7 records were actually Gresham or Troutdale Police calls
- 213 records focused on the passenger of the vehicle (these can be used for future, separate analyses if desired)

PPB officers cancelled 11% of their SDC forms. Table 2 displays the reason officers cancelled these SDC forms (this table includes both pedestrian and traffic stops):

Table 2. Reasons for Cancelling Stop		
Reason	Count	Percent
Duplicate Stop	510	1.6%
Flagged Down (no stop)	83	0.3%
Mere Conversation (no stop)	2092	6.7%
Welfare Check (no stop)	336	1.1%
Other	411	1.3%
Not Cancelled	27711	89.0%
Total	31143	100.0%

The final analysis consisted of 24,998 records involving the driver of a vehicle on traffic stops and 614 records involving pedestrian stops.

Unresolved data issues

Several issues were identified through this analysis that will need to be resolved. These issues include:

Duplicate entries for what appears to be the same stop.

For instance, 1.5 % of patrol stops had duplicate entries where the race of the driver was the same on both entries. This may be accurate (i.e. officers stopped multiple persons on the same incident), but this needs to be confirmed. Initial analysis indicates that some portion of these duplicates are legitimate (for instance duplicate entries with different race and gender information on the same incident), others may be the result the same data being entered multiple times (for instance several stops on the

¹ The change to the new stop and search data collection system occurred on August 5, 2011. 1,299 of the 1,879 cases prior to noon on August 5, 2011, were at exactly 10:03:25, suggesting an initial error in the collection system. The cases after noon reflected reasonable activity and were likely accurate, so the data used for this report began on August 5, 2011 at noon.

same incident each logged 18 to 24 seconds apart).

The impact of these issues on the quality of the analysis appears to be minimal. For instance, 1.2% of stops of African-American/Black drivers by patrol had duplicate entries (this amounts to 24 stops) while 1.4% of stops of White drivers by patrol had duplicate entries (this amounts to 101 stops). Traffic Division had a higher percentage of duplicate entries (approximately 3.5% of stops of drivers), but this may be due to higher number of legitimate entries when an officer stops multiple drivers at the same time.

Issues surrounding the recovery of property when no search was conducted

Officers can recover property and list it in the SDC form when no search has been conducted. This was very uncommon, but creates confusion in the data analysis. Some portion of these may be the results of officers recovering property on a stop unrelated to the incident. However, given the SDC form's current configuration it is impossible to determine what portion of this is the result of human error (incorrectly indicating that no search had been conducted) and which portion is legitimate. A solution for this has been identified and the PPB is working on implementing it. This change should resolve this issue in future analyses.

Analysis

Benchmarks – Who is driving?

One of the most frequently used benchmarks for stops data is census data (Engel & Calnon, 2004). Census reporting can be informative, but is generally not a sufficient benchmark when used alone. As pointed out by Renauer et al., 2009, a variety of benchmarks is ideal. However, census and survey data can act as one potential source of benchmarking. Table 3 is taken from the American Community Survey (ACS) 5-year estimates for 2007 to 2011:

Race/Ethnicity	Percent
One race	95.7%
White	77.4%
Black or African American	6.3%
American Indian and Alaska Native	1.0%
Asian	7.2%
Native Hawaiian and Other Pacific Islander	0.5%
Some other race	3.2%
Two or more races	4.3%
Hispanic or Latino origin (of any race)	9.2%
White alone, not Hispanic or Latino	72.4%

The structure of Census and American Community Survey data is not consistent with the PPB data (Withrow, 2008). The Portland Police Bureau has consistently collected data based on the following categories: African American/Black, Asian, Hispanic, Native American, White, and Unknown/Other. While the Census and American Community Survey data are more descriptive, officers are coding based on their perceptions so it would be difficult to match this level of specificity. However, because one of the

concerns is that people are being treated unfairly based on racial perceptions and that it may be harmful and invasive to be asking community members for their racial and ethnic identity when stopped, this coding practice appears to be reasonable at this time.

This limitation may make comparisons between PPB data and ACS or Census data less accurate. For instance, some community members have justifiably pointed out that many Native American persons may be mistakenly coded as Hispanic. Other issues may arise when an officer attempts to code Hispanic individuals who are White. This limitation may be insurmountable without officers asking invasive questions not related to the stop. The PPB's position is that the damage caused by asking such questions would outweigh any potential benefits from capturing more accurate data. Given these limitations, direct comparisons to census data may be misleading.

An alternate for assessing who is using roadways is the use of the demographic data of non-responsible drivers in two vehicle accidents (Alpert, Smith, & Dunham, 2004). Unfortunately, their exact methodology could not be replicated because the PPB data does not differentiate between single and multiple vehicle accidents. Despite this limitation, the PPB crash data does have several attractive characteristics for a potential benchmark.

One benefit of the data is that PPB policy (Portland Police Bureau, 2009) requires investigations for serious injury accidents. These accident investigations are conducted by trained traffic officers, if they are available, utilizing a standardized methodology which limits discretion. Additionally, the demographic characteristics collected for this data set is in a format consistent with other PPB data. As such, these accidents represent a possible benchmark for road usage. Table 4 examines the demographic characteristics of drivers involved in injury accidents as captured by PPDS between August 5, 2011 and December 31, 2011:

Table 4. Drivers in Injury Accidents in Portland		
Race/Ethnicity	All Drivers	
	Count	Percent
African American/Black	23	6.6%
Asian	25	7.2%
Hispanic	32	9.2%
Native American	1	0.3%
White	262	75.5%
Unknown/Other	4	1.2%
Total	347	100.0%

The accident data for the dates of August 5 to December 31, 2011, was used in order to match the dates of the stop data. Future analyses could also consider using an average for 1-3 years in order to make these percentages more robust. Despite the low counts, this data is consistent with other benchmarks (ACS 5-Year and Census 18 and over). Having multiple benchmarks that provide similar benchmarks for who is driving should increase our confidence in the accuracy of these benchmarks.

Who is stopped and searched?

The next several pages provide the stops and searches analyses with the following breakdowns:

- Traffic and Patrol Stops of Drivers Combined
- Traffic and Patrol Pedestrian Stops Combined
- Traffic Stops of Drivers Only
- Patrol Stops of Drivers Only
- Patrol Pedestrian Stops Only

The data for traffic and patrol officers are broken down because officers focusing on traffic enforcement have different criteria for stops, operate in different areas and at different times of the day than patrol officers, who tend to be more focused on crime reduction as opposed to traffic law enforcement.

Stops of drivers for Traffic and patrol

Table 5 displays the demographic breakdown of all stops of the drivers of motor vehicles occurring between August 5, 2011, and December 31, 2011 in the city of Portland (both patrol and Traffic Division officers):

Race/Ethnicity	Count	Percent
African American/Black	2946	11.8%
Asian	1121	4.5%
Hispanic	1539	6.2%
Native American	66	0.3%
White	17943	71.8%
Unknown/Other	1378	5.5%
Total*	24993	100.1%

* Note: Five stops missing the race of driver. Total does not equal 100% due to rounding.

The main findings:

- African Americans/Blacks were more likely to be stopped compared to both their Census and accident data estimates. This is the only racial/ethnic group in this analysis that is consistently stopped in greater proportion than their driving population would indicate. There were 1,296 more stops of African Americans/Blacks than we would expect given their approximate percentage of the driving population (using the higher estimate for their driving population).
- Asians were less likely to be stopped compared to both their Census and accident data estimates.
- Hispanics were less likely to be stopped compared to both their Census and accident data estimates.
- Native Americans were less likely to be stopped compared to the Census estimates but are stopped at an equivalent rate compared to the accident data.

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- Whites were less likely to be stopped compared to both their Census and accident data estimates.
- The Unknown/Other category is difficult to compare to the Census estimates. This group was more likely to be stopped compared to their accident data estimate.

Reasons for the Stop

Table 6 displays the reasons citywide for stops. This information is collected to provide greater clarity on the reasons for stops. One goal of this is to identify “pre-text” stops (stops in which the traffic violation is used to initiate an investigative contact) which may be more susceptible to bias (Fridell, 2004; Renauer et al., 2009). A potential cause for the disproportionate use of pre-text stops against differing groups would be large differences in the use of more subjective or lower level offenses as a reason for stopping people of color. For instance, the use of equipment violations as a reason for stopping drivers of color may signify the use of such violations as a “pre-text” for stopping (although it may also be the result of other disparities such as socio-economic differences). Another important consideration would be the magnitude (or relative number) of such stops.

Table 6. Citywide Reasons for Stops of Drivers

RACE/ETHNICITY	City Code		Equipment		License		Major ¹		Minor ²		Other		Total	
	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent
African American/Black	6	0.2%	365	12.4%	118	4.0%	866	29.4%	1560	53.0%	31	1.1%	2946	100.0%
Asian	1	0.1%	95	8.5%	16	1.4%	388	34.6%	613	54.7%	8	0.7%	1121	100.0%
Hispanic	5	0.3%	168	10.9%	39	2.5%	502	32.6%	799	51.9%	26	1.7%	1539	100.0%
Native American	0	0.0%	9	13.6%	2	3.0%	21	31.8%	34	51.5%	0	0.0%	66	100.0%
White	15	0.1%	1504	8.4%	395	2.2%	5940	33.1%	9993	55.7%	96	0.5%	17943	100.0%
Unknown/Other	4	0.3%	118	8.6%	11	0.8%	364	26.4%	846	61.4%	35	2.5%	1378	100.0%
Grand Total*	31	0.1%	2259	9.0%	581	2.3%	8081	32.3%	13845	55.4%	196	0.8%	24998	100.0%

¹ Major Moving Violation (Traffic crime, A or B Infraction)

² Minor Moving Violation (Class C or D Infraction)

*Note five stops missing race of driver

The main findings:

- The distribution of reasons for why drivers were stopped was fairly similar among the six racial/ethnic groups.
- African Americans/Blacks and those in the Unknown/Other category were the least likely to be pulled over for a major traffic violation (African American/Blacks = 29.4%, Unknown/Other = 26.4%, and Whites = 33.1%).
- Native Americans, African Americans/Blacks, and Hispanics were more likely than Whites to be pulled over for an equipment violation (Native American = 13.6%, African Americans/Blacks = 12.4%, Hispanics = 10.9%, and Whites = 8.4%).
- African Americans/Blacks and Native Americans were more likely to be stopped for a license violation than Whites (African Americans = 4.0%, Native Americans = 3.0%, and Whites = 2.2%).
- African Americans/Blacks, Hispanics, and those in the Unknown/Other category were more likely than Whites to be stopped for an “other” violation. (African American/Blacks = 1.1%, Hispanics = 1.7%, Unknown/Other = 2.5%, and Whites = .5%).
- One suggestion for future analysis would be to add an indicator to the SDC form so

that officers could identify which stops were the results of an emphasis on traffic enforcement and which stops were conducted for investigative purposes (i.e. “pre-text” stops).

Searches of drivers

Table 7 examines searches of drivers citywide within race (i.e. when a white person is stopped a consent search is conducted 1.9% of the time):

Table 7. Reasons for Searches of Drivers Citywide (% by Ethnicity/Race)*

RACE/ETHNICITY	Consent		No Search		Plain View		Probable Cause		Weapons Pat Down		Total	
	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent
African American/Black	245	8.3%	2572	87.3%	12	0.4%	78	2.6%	39	1.3%	2946	100.0%
Asian	8	0.7%	1101	98.2%	2	0.2%	9	0.8%	1	0.1%	1121	100.0%
Hispanic	70	4.5%	1415	91.9%	5	0.3%	32	2.1%	17	1.1%	1539	100.0%
Native American	3	4.5%	60	90.9%	1	1.5%	2	3.0%	0	0.0%	66	100.0%
White	349	1.9%	17318	96.5%	32	0.2%	184	1.0%	60	0.3%	17943	100.0%
Unknown/Other	21	1.5%	1335	96.9%	2	0.1%	9	0.7%	11	0.8%	1378	100.0%
Total*	696	2.8%	23806	95.2%	54	0.2%	314	1.3%	128	0.5%	24998	100.0%

* Note % stops missing race of driver, however none of these stops resulted in a search of a driver. These stops were added to the No Search and Total columns.

The main findings:

- African-American/Black, Hispanic, and Native American drivers that were stopped were more likely than Whites to have a consent search while Asian drivers were less likely to be searched when stopped.
- 8.3 percent of the African-American/Black drivers that were stopped had a consent search.
- 4.5 percent of the Hispanic drivers that were stopped had a consent search.
- 4.5 percent of the Native American drivers that were stopped had a consent search.
- 1.9 percent of White drivers that were stopped had a consent search.
- 0.7 percent of Asian drivers that were stopped had a consent search.
- Approximately 95% of drivers that were stopped were not searched. In the roughly five month period examined, police searched³:
 - 374 African-American/Black Drivers
 - 20 Asian Drivers
 - 124 Hispanic Drivers
 - 6 Native American Drivers
 - 625 White Drivers
 - 43 Drivers whose race was unknown or not captured in the above categories.

Hit Rates on Searches

Fridell (2004) highlights issues that surround the use of hit rates (a hit rate is the percentage of searches which result in finding contraband)⁴ in general, and the

³ This number was calculated by subtracting the “No Search” value from the Total number of stops.

⁴ The PPB collects data on the following types of contraband: alcohol, drugs, nothing found, other, stolen property and weapons.

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problem related to the analysis of consent searches in particular⁵. While not addressed by Fridell, there is an additional issue regarding searches with the PPB. Officers are trained to ask for consent even when other legal reasons for a search exist (this is due to the fact that consent searches are less likely to be lost in a motion to suppress). Thus, the relatively high number of consent searches may be deceptive because other legitimate search reasons may have existed, but not been captured. This problem illustrates the difficulty of collecting and analyzing data of this complexity. Despite these issues, the use of hit rates is a viable method to examine the relative productivity of searches. Table 8 examines the hit rates of stopped drivers citywide for various kinds of contraband:

Table 8. Hit Rates of Stopped Drivers Citywide (Traffic and Patrol) for all contraband, Alcohol, Drugs and Weapons¹

Race/Ethnicity	Total Searches	All Contraband		Alcohol		Drugs		Weapons		Contraband excluding Alcohol	
		Hits	Percent	Hits	Percent	Hits	Percent	Hits	Percent	Hits	Percent
African American/Black	374	114	30.5%	21	5.6%	65	17.4%	18	4.8%	93	24.9%
Asian	20	7	35.0%	1	5.0%	6	30.0%	1	5.0%	6	30.0%
Hispanic	124	37	29.8%	11	8.9%	19	15.3%	4	3.2%	31	25.0%
Native American	6	5	83.3%	3	50.0%	2	33.3%	0	0.0%	2	33.3%
White	625	267	42.7%	96	15.4%	138	22.1%	23	3.7%	172	27.5%
Unknown/Other	43	19	44.2%	1	2.3%	7	16.3%	1	2.3%	18	41.9%
All Non-White ²	567	182	32.1%	37	6.5%	99	17.5%	24	4.2%	150	26.5%
Total³	1192	449	37.7%	133	11.2%	237	19.9%	47	3.9%	322	27.0%

¹ Officers can choose between the following results: Alcohol, Drugs, Other, Nothing Found, Stolen Property and Weapons. Stolen property and weapons are not included individually but are included in all contraband.

² This category is created by subtracting white driver results from the total and is not unique in the original data set. It includes unknown/other race individuals.

³ Total Searches may not equal search results because multiple items can be recovered in the same search.

Main Findings:

- African Americans/Blacks, Asians, and Hispanics that were searched were less likely than Whites to be found with contraband.
- 29.8 percent of Hispanics that were searched had some form of contraband.
- 30.5 percent of African Americans/Blacks that were searched had some form of contraband.
- 35.0 percent of Asians that were searched had some form of contraband.
- 42.7 percent of Whites that were searched had some form of contraband.
- 83.3 percent of Native Americans that were searched had some form of contraband (the percentages for Native Americans can be misleading due to the low search counts for this group).
- Some of the disparity appears to be related to alcohol. The hit rates when excluding alcohol are more similar than when alcohol is being accounted for.

Stops of pedestrians for Traffic and patrol

Table 9 displays the demographic breakdown of all pedestrians stopped by PPB officers in the city of Portland between August 5 and December 31, 2011. The comparison here is more difficult since we do not have an additional measure to verify the racial/ethnic breakdown of pedestrians like we do with who is driving.

⁵ Please refer to Fridell (2004) for a comprehensive review of the controversy surrounding hit rates. As mentioned earlier in this report, without an indicator of who is asked for consent versus who grants consent any analysis is of limited utility. This is especially relevant given that over half of actual searches by PPB officers is a consent search.

Table 9. Citywide Race at Stops of Pedestrians (Traffic and Patrol)

RACE/ETHNICITY	Total	
	Count	Percent
African American/Black	120	19.5%
Asian	12	2.0%
Hispanic	37	6.0%
Native American	10	1.6%
White	410	66.8%
Unknown/Other	24	3.9%
Grand Total*	614	100.0%

*Note includes one stop without race of pedestrian.

The main findings:

- African Americans/Blacks were more likely to have a pedestrian stop compared to their Census estimates. The difference in the pedestrian stops was greater than the difference for drivers.
- Asians were less likely to have a pedestrian stop compared to their Census estimates. The difference in the pedestrian stops was greater than the difference for drivers.
- Hispanics were less likely to have a pedestrian stop compared to their Census estimates. The difference in the pedestrian stops was similar to their difference for drivers.
- Native Americans were more likely to have a pedestrian stop compared to their Census estimates.
- Whites were less likely to have a pedestrian stop compared to their Census estimates. The difference in the pedestrian stops was greater than the difference for drivers.
- The Unknown/Other category is difficult to compare to the Census estimates. This group was more likely to have a pedestrian stop compared to their accident data estimate. This disparity was less in the pedestrian stops than for the drivers of this category.

Table 10 listed the reasons for pedestrian stops citywide. Given the very small number of pedestrians stopped in some racial/ethnic categories, it is difficult to draw conclusions on the data for the Asian, Native American, Hispanic and Unknown/Other pedestrians. Therefore the findings will focus on a comparison between African American/Blacks and Whites.

Table 10. Citywide Reasons for Stops of Pedestrians

RACE/ETHNICITY	City Code		Equipment		License		Major ¹		Minor ²		Other		Total	
	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent	Count	Percent
African American/Black	19	15.8%	8	6.7%	4	3.3%	21	17.5%	43	35.8%	25	20.8%	120	100.0%
Asian	0	0.0%	1	8.3%	0	0.0%	6	50.0%	4	33.3%	1	8.3%	12	100.0%
Hispanic	8	21.6%	2	5.4%	1	2.7%	7	18.9%	11	29.7%	8	21.6%	37	100.0%
Native American	3	30.0%	0	0.0%	0	0.0%	1	10.0%	2	20.0%	4	40.0%	10	100.0%
White	84	20.5%	27	6.6%	4	1.0%	78	19.0%	136	33.2%	81	19.8%	410	100.0%
Unknown/Other	2	8.3%	4	16.7%	1	4.2%	2	8.3%	11	45.8%	4	16.7%	24	100.0%
Grand Total*	116	18.9%	42	6.8%	10	1.6%	116	18.9%	207	33.7%	123	20.0%	614	100.0%

¹ Major Moving Violation (Traffic crime, A or B Infraction)

² Minor Moving Violation (Class C or D Infraction)

*Note includes one stop without race of pedestrian. This person was stopped for a major violation. It is included in the major violation and total columns.

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The main findings:

- The distribution of reasons for why African-American/Black and White pedestrians were stopped was fairly similar.
- African American/Blacks were slightly less likely than Whites to be stopped for a major violation (African American/Blacks = 17.5% and Whites = 19.0%) and slightly more likely to be stopped for a minor violation (African American/Blacks = 35.8% and Whites = 33.2%).
- African American/Blacks were more likely than Whites to be stopped for a license violation (African American/Black = 3.3% and Whites = 1.0%).

There were only 614 stops of pedestrians compared with 24,998 traffic stops during this period. However, there were approximately 2,000 stops forms cancelled which involved mere conversation type contacts. These contacts could include a wide variety of activities, but are different from stops in that the person being contacted is free to leave and is not being legally detained.

Summary

As noted previously in this report, analyzing and interpreting stop and search data has its challenges. Therefore, examining multiple analyses and considering multiple contributing factors to why disparities exist is important. Researchers specializing in analyzing disparities in stops data suggest examining various analyses and looking at patterns of disparate outcomes to help identify whether the findings are concerning. In these initial findings, of particular concern is the disparate impact on African Americans/Blacks. They are demonstrating the greatest disparities and concerning findings in the stops data, as the data shows consistent disparities for this group (in traffic stops and searches, reasons for the stop, consent searches, hit rates, and pedestrian stops). Native Americans and Hispanics had disparities in some of the analyses, suggesting they also should be looked at in more in-depth analyses. However, these disparities tended to be smaller and less consistent than those for the African Americans/Blacks.

Particularly since disparities were found in these initial analyses, it was important to conduct further analyses to better understand the reason for the disparities. For instance, past reports have found marked difference in the findings between the traffic and patrol divisions. Other findings have noted that the disparities in stops correlate with areas that have more crime and therefore more proactive patrol. Although the cause of racial disparities can be from multiple reasons that often overlap or are interrelated and therefore challenging to analyze, better understanding these relationships is a critical step to finding the solutions to reduce disparities.

Stops of pedestrians by patrol officers

Many agencies, particularly on the east coast, employ strategies which utilize large numbers of pedestrian stops (similar to “pre-text” stops of drivers, i.e. stopping a driver for a traffic investigation when the underlying reason for the stop is not to enforce traffic, but to look for other criminal activity such as property crime, violent crime or drug possession). Often called “stop-and-frisk” such practices have resulted in people of color being stopped at rates in excess of both demographic and crime-related variables (Gelman, Fagan, & Kiss, 2007).

The Portland Police does not employ a strategy based on “stop-and-frisk.” On the contrary, officers are trained to ask for consent to search, even if they believe they are justified in performing a non-consensual pat down for weapons¹¹. This leads to a larger number of consent searches in Portland compared with weapon pat downs elsewhere. There is also less reliance on stops and a greater emphasis on “mere conversation.” The difference between a stop and “mere conversation” is that the subject is free to leave if the officer engages in “mere conversation,” but can be legally detained (although not necessarily arrested) in a stop. Some community members have expressed concern that “mere conversation” contacts are not tracked. This concern is valid in that the number of such contacts exceeds the number of pedestrian stops. Despite this concern, the logistics of collecting data on every “mere conversation” contact would be considerable. The Portland Police respond to approximately 400,000 calls annually and most resulted in at least one such contact and many result in multiple contacts¹².

Table 22 examines the race of pedestrians stopped by patrol officers between August 5 and December 31, 2011 (this does not include mere conversations):

Table 23. Citywide Race at Stop of Pedestrians (Patrol)		
Race/Ethnicity	Count	Percent
African American/Black	107	22.1%
Asian	10	2.1%
Hispanic	32	6.6%
Native American	10	2.1%
White	305	63.0%
Unknown/Other	20	4.1%
Total	484	100.0%

The total number of pedestrian stops by patrol during this time was 484. It is important to remember this does not count the number of unique individuals stopped, but all stops regardless of if the person is stopped multiple times by the same or different officers. Many individuals are known to the police and are repeatedly stopped (this

¹¹ Per conversations with Training Division officers, this is still standard practice as of 1/25/12. The benefits of having consent versus a pat down are related with issues of admissibility of evidence in court.

¹² The PPB recognizes that if the community is sufficiently concerned it may be necessary to collect such data. However, the costs of collecting and analyzing the data would be considerable. If officers average 1.5 routine contacts per call and collecting data on such contacts took only 3 minutes on average the amount of time spent collecting data (filling out contact forms) would be the equivalent of approximately 14.5 full-time police officers annually.

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is particularly true of a small number of very active gang members and individuals who are prohibited from being in drug impact areas). Being able to separate these stops would allow for a more refined analysis of the impact on different demographic groups (one person being stopped repeatedly due to gang involvement would be less impactful on the community than a large number of individuals stopped).

Tables 23 through 25 examine these stops by precinct:

Table 23. Central Precinct Race at Stop of Pedestrians (Patrol)		
Race/Ethnicity	Count	Percent
African American/Black	39	17.6%
Asian	2	0.9%
Hispanic	13	5.9%
Native American	5	2.3%
White	157	71.0%
Unknown/Other	5	2.3%
Total	221	100.0%

Table 24. East Precinct Race at Stop of Pedestrians (Patrol)		
Race/Ethnicity	Count	Percent
African American/Black	17	19.1%
Asian	3	3.4%
Hispanic	4	4.5%
Native American	1	1.1%
White	55	61.8%
Unknown/Other	9	10.1%
Total	89	100.0%

Table 25. North Precinct Race at Stop of Pedestrians (Patrol)		
Race/Ethnicity	Count	Percent
African American/Black	37	30.6%
Asian	4	3.3%
Hispanic	10	8.3%
Native American	2	1.7%
White	63	52.1%
Unknown/Other	5	4.1%
Total	121	100.0%

What does the data mean?

Benchmarking pedestrian stops is difficult without using observational data to examine the proportion of individuals walking and/or violating pedestrian rules. The PPB performs very limited enforcement of jaywalking and other offenses (although some officers may enforce these rules and very occasionally a mission is run in response to a pedestrian death). Anecdotally¹³, drug enforcement in areas with open air drug markets¹⁴ often involves pedestrian stops. Another major activity involving stops of pedestrians are citations for having open alcohol containers. Officers also focus on violent crime and contacting individuals in areas with where violent crime (particularly gang crime) has occurred.

¹³ Based on the author's personal experiences and conversations with officers still working in patrol.

¹⁴ Low-level drug deals typically involve a seller and buy who know each other communicating via phone, text or alternate means and arrangements to meet. Some areas (Old Town or the area beneath the Burnside Bridge for instance) are open air drug markets where drug buyers and sellers, who may or may not know each other, meet to sell/buy drugs.

Variations in where different racial groups live as well as different enforcement priorities may account for some of the variation in stops between precincts.

Table 26 examines the demographic characteristics of pedestrians stopped by patrol officers against the rate of victimization in violent crime by precinct:

Race/Ethnicity	Central/Viol Exposure	Central/Stops	East/Viol Exposure	East/Stops	North/Viol Exposure	North/Stops
African American/Black	8.8%	17.6%	21.0%	19.1%	26.2%	30.6%
Asian	3.9%	0.9%	8.9%	3.4%	4.7%	3.3%
Hispanic	8.8%	5.9%	10.1%	4.5%	9.9%	8.3%
Native American	1.4%	2.3%	1.9%	1.1%	3.0%	1.7%
White	77.1%	71.0%	61.5%	61.8%	56.1%	52.1%

This table compares stop rates against exposure to violent crime (as measured by victimization in Part I violent crimes reported to the police)¹⁵. For instance, when using violent Part I crime it appears that:

- The percentage of stops consisting of African-American/Black pedestrians is approximately twice the amount than would be expected in Central Precinct, slightly greater than would be expected in North Precinct and is slightly less than would be expected in East Precinct.
- The number of stops consisting of Asian and Native American pedestrians stopped is small (10 stops for each group), making it difficult to draw meaningful conclusions.
- The percentage of stops consisting of Hispanic pedestrians stopped is below what their prevalence as victims would indicate in all three precincts.
- The number of stops consisting of Asian pedestrians is lower than their violent exposure rate in all three precincts and substantially lower in Central and East precinct.
- Native Americans received more pedestrian stops than would be expected by their violent exposure in Central precinct and less than would be expected in East and North precinct.
- The percentage of stops consisting of White pedestrians stopped is slightly lower than would be expected in Central and North Precincts and at a rate almost exactly the same as exposure in East.

While there are substantial disparities in victimization for violent crime reported to the police these disparities do not account for differences in stops of pedestrians in Central Precinct. By examining the data more closely, it becomes apparent that nearly all the disparities in stops of pedestrians observed in Central Precinct occurred in District 822.

¹⁵ This table has no Unknown/Other race category because officers identified the race of all individuals victimized.

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Table 27. Central Precinct Race at Stop of Pedestrians (Patrol) - the Impact of Oldtown					
Race/Ethnicity	District 822 (Oldtown east of Broadway)		Central without 822		Central/Violent Exposure
	Count	Percent	Count	Percent	Percent
African American/ Black	22	44.0%	17	9.9%	8.8%
Asian	0	0.0%	2	1.2%	3.9%
Hispanic	2	4.0%	11	6.4%	8.8%
Native American	1	2.0%	4	2.3%	1.4%
White	25	50.0%	132	77.2%	77.1%
Unknown/Other	0	0.0%	5	2.9%	0.0%
Total	50	100.0%	171	100.0%	100.0%

It is important to note that these are stops which occur in District 822 (not stops by any particular officer working in District 822). This district, in the heart of Old Town, is roughly composed of the area east and south of NW Broadway and north of West Burnside Street. Historically, this area has experienced a high volume of complaints regarding drug activity, street drinking and other livability issues.

Summary

The examination of pedestrian stops provides an excellent illustration of the many potential pitfalls associated with determining appropriate benchmarks for police units who are responsible for responding to issues beyond simply traffic enforcement. While pedestrian stops are disproportionate to Census estimates, it is likely that patrol units are responding to increased victimization in parts of the city which are disproportionately inhabited by residents of color. However, even after accounting for disparate victimization, certain parts of the city have disproportionate numbers of people of color stopped. Better understanding the cause of these disparities is important and the following sections will explore three potential contributing factors that PPB officers, PSU researchers and the public have noted: differential exposure to law enforcement, the impact of local gangs and the impact of racial bias.

CERTIFICATE OF FILING AND SERVICE

I certify that on September 16, 2014, I filed the original of this BRIEF ON THE MERITS OF *AMICUS CURIAE* OREGON JUSTICE RESOURCE CENTER with the State Court Administrator by the eFiling system.

I further certify that on September 16, 2014, I served a copy of the BRIEF ON THE MERITS OF *AMICUS CURIAE* OREGON JUSTICE RESOURCE CENTER on the following parties by electronic service via the eFiling system:

Mr. Daniel C. Bennett
Office of Public Defense Services
1175 Court Street NE
Salem, OR 97301

Attorney for Respondent on Review
William Rick DeLong

Mr. Michael A. Casper
Assistant Attorney General
400 Justice Building
1162 Court Street NE
Salem, OR 97301

Attorney for Petitioner on Review
State of Oregon

/s/ Sara F. Werboff
Sara F. Werboff, OSB No. 105388