

The Cognitive Components of Punishment

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THE COGNITIVE COMPONENTS OF PUNISHMENT

Jeffrey J. Rachlinski[†]
& *Forest Jourden*^{††}

INTRODUCTION	457
I. CONSISTENCY, LEGAL JUDGMENT, AND PUNISHMENT	
DECISIONS	459
A. Consistency in Judgment and Choice	459
B. Consistency in Legal Judgments	462
C. Cognitive Consistency and Punishment in the Criminal Context	464
II. METHODS	466
A. Contrast Effects on Years of Imprisonment	467
B. Contrast Effects Arising from a Co-Conspirator's Sentence	469
C. Contrast Effects Arising from a Similar Type of Crime	471
D. Contrast Effects Arising from a More or a Less Severe Crime	473
E. Demographic Information	475
III. RESULTS	477
A. Contrast Effects on Years of Imprisonment	477
B. Contrast Effects Arising from a Co-Conspirator's Sentence	477
C. Contrast Effects Arising from a Similar Type of Crime	478
D. Contrast Effects Arising from a More or a Less Severe Crime	480
IV. DISCUSSION	482

INTRODUCTION

Consistency plays a central role in a fair criminal justice system. A system that does not treat similarly situated people alike cannot be squared with norms of justice or fairness in any society. Arbitrary justice is an oxymoron. Consistency becomes more important as the severity of a penalty increases. Randomly assigned monetary fines are bad enough, but arbitrariness in the administration of the death pen-

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alty is intolerable. Random executions are unlikely to deter crime, do not further retributive goals, and are simply immoral.

Although the fear of executing an innocent defendant underlies much of the contemporary opposition to capital punishment, the perception that the death penalty is arbitrarily imposed also motivates opposition. Mistakes in the administration of criminal justice are inevitable (and may be unacceptable in the case of the ultimate penalty), but systematically arbitrary executions are completely intolerable. So strong are the fears of arbitrary executions that they provided the primary justification for the Supreme Court's temporary moratorium on executions in the mid-1970s. Arbitrary inflictions of the death penalty are inconsistent with constitutional protections against cruel and unusual punishment and constitutional requirements of due process.¹ Only when state legislatures provided assurance that juries were given adequate guidance to safeguard against arbitrariness did the Supreme Court allow resumption of the death penalty in the United States.²

Whether states in fact created or implemented jury instructions that truly prevent arbitrariness in the administration of the death penalty is a matter of much debate. Numerous studies have presented evidence suggesting that illegitimate factors such as race and jury misunderstanding continue to play a role in capital sentencing determinations.³ Nevertheless, the Supreme Court has maintained that guided jury instructions adequately protect against such sources of arbitrariness.⁴

In effect, the Supreme Court has adopted a simplistic model of decision making in capital cases.⁵ That is, the Court believes that each capital case presents some stable, underlying degree of deathworthiness and that trial courts must accurately measure this trait. Under this model, the decision that a juror faces in determining whether a capital conviction warrants a death sentence is conceptually similar to any binary decision. To make such a decision, jurors must assess the

¹ See *Furman v. Georgia*, 408 U.S. 238, 274 (1972) (Brennan, J., concurring) ("[T]he State must not arbitrarily inflict a severe punishment.").

² See *Gregg v. Georgia*, 428 U.S. 153, 195 (1976) (plurality opinion) ("[T]he concerns expressed in *Furman* that the penalty of death not be imposed in an arbitrary or capricious manner can be met by a carefully drafted statute that ensures that the sentencing authority is given adequate information and guidance.").

³ See, e.g., DAVID C. BALDUS, GEORGE WOODWORTH & CHARLES A. PULASKI, JR., *EQUAL JUSTICE AND THE DEATH PENALTY: A LEGAL AND EMPIRICAL ANALYSIS* 154 tbl.32, 155-57, 184-88 (1990) (presenting evidence that race influences post-*Furman* death penalty cases); Theodore Eisenberg & Martin T. Wells, *Deadly Confusion: Juror Instructions in Capital Cases*, 79 CORNELL L. REV. 1, 4-12 (1993) (presenting evidence that jurors in capital cases do not understand jury instructions).

⁴ See *McCleskey v. Kemp*, 481 U.S. 279, 302-04 (1987).

⁵ See *id.* at 305 ("[T]he State must establish rational criteria that narrow the decisionmaker's judgment as to whether the circumstances of a particular defendant's case meet the threshold.").

degree to which the defendant deserves such a sentence and then determine whether this degree crosses some threshold level. The first task is specific to each defendant and essentially requires some kind of scaling of the case for imposition of the death penalty on this defendant. The second task is then to compare that measurement with some absolute value to determine whether the defendant's conduct crosses a threshold that would merit the death penalty. To be consistent, this threshold should remain constant across defendants. The task in a capital case is surprisingly analogous to determining whether to purchase a used car. People must assess the merits of the car and then determine whether the merits are sufficiently high that they are worth the price being asked. In effect, the decision requires two cognitive tasks: measurement followed by comparison with some absolute standard.

Both of a juror's tasks in a capital case, measurement and comparison, are potential sources of arbitrariness. In the first task, jurors might be influenced by irrelevant or inappropriate factors in assessing the defendant's deathworthiness. Jurors might, for example, assess a defendant's conduct more harshly if the defendant is an African American than if the defendant is Caucasian. Similarly, irrelevant factors might influence where jurors draw the line between life and death. So long as jurors are consistent in their assessment of the defendant's conduct, however, and use the same threshold for the death penalty in all cases, the state can administer the death penalty consistently. The Supreme Court's faith that proper instructions will weed out inappropriate considerations in these two tasks underlies both its jurisprudence and its continued defense of the death penalty.⁶

I CONSISTENCY, LEGAL JUDGMENT, AND PUNISHMENT DECISIONS

A. Consistency in Judgment and Choice

Research in the cognitive psychology of judgment and choice suggests that consistency in binary choices is not the norm in human judgment. Several decades of research on human judgment and choice indicate that human judgment is profoundly sensitive to context.⁷ This sensitivity makes choices erratic. From judgments concerning the purchase of consumer products to assessments of the

⁶ See, e.g., *Maynard v. Cartwright*, 486 U.S. 356, 362 (1988) (citing cases) ("Since *Furman*, our cases have insisted that the channeling and limiting of the sentencer's discretion in imposing the death penalty is a fundamental constitutional requirement for sufficiently minimizing the risk of wholly arbitrary and capricious action." (citations omitted)).

⁷ See Jeffrey J. Rachlinski, *The Uneasy Psychological Case for Paternalism*, 97 Nw. U. L. REV. (forthcoming 2003).

value of environmental amenities, psychologists have been able to demonstrate profound inconsistencies in choice.⁸ These inconsistencies appear to arise because people construct preferences to suit choices.⁹ People do not simply measure the unchanging features of an option and compare the results to some threshold. Thus, the model that the Supreme Court adopted for capital decision making does not track how people make choices.

The principal evidence that human choice is inconsistent and contextual is the demonstration of contrast effects in choice. For example, when making choices about consumer goods that vary across multiple dimensions, people place more weight on dimensions that they can evaluate clearly.¹⁰ In one study of this phenomenon, researchers presented subjects with evaluations of two music dictionaries.¹¹ Dictionary A contained 20,000 entries but had a torn cover; dictionary B had 10,000 entries and was completely intact. The first group of subjects was presented with either dictionary A or B, but not both. When asked to determine how much they would be willing to pay for the dictionaries, subjects tended to state a lower amount for A than for B. When given a choice between the two dictionaries, however, subjects preferred A over B. Although people do not have a good sense of the relationship a torn cover should have with price, they clearly understand that more entries make for a better dictionary. Thus, when offered their choice of dictionaries, the number of words drives choice. Such a strategy might be normatively defensible, except that it can lead to reversals in judgment.¹² Researchers have identified this kind of contrast effect using a range of stimuli—from consumer products to job applicants.¹³ The independent weight people attach to dimensions that can easily be evaluated creates inconsistencies between preferences and choice.

Similarly, people's ability to evaluate the merit of choices seems to depend upon the presence of seemingly irrelevant alternatives. To

⁸ See Daniel Kahneman & Amos Tversky, *Choices, Values, and Frames*, in CHOICES VALUES, AND FRAMES 1 (Daniel Kahneman & Amos Tversky eds., 2000).

⁹ See Paul Slovic, *The Construction of Preference*, 50 AM. PSYCHOLOGIST 364, 364 (1995).

¹⁰ See Christopher K. Hsee, George F. Loewenstein, Sally Blount & Max H. Bazerman, *Preference Reversals Between Joint and Separate Evaluation of Options: A Review and Theoretical Analysis*, 125 PSYCHOL. BULL. 576, 578–79 (1999).

¹¹ See Christopher K. Hsee, *The Evaluability Hypothesis: An Explanation for Preference Reversals Between Joint and Separate Evaluations of Alternatives*, 67 ORGANIZATIONAL BEHAV. & HUM. DECISION PROCESSES 247, 247–50 (1996).

¹² See *id.* at 249 (“[J]oint-separate evaluation [preference reversals] occur because one of the attributes involved in the stimulus options is *hard to evaluate independently* and the other attribute is relatively *easy to evaluate independently*.”).

¹³ See *id.* at 250–55.

be consistent, people's preferences should express invariance.¹⁴ As one group of scholars put it, "[a] person who prefers chicken over pasta should not change this preference on learning that fish is also available."¹⁵ Studies of consumer choice indicate that when people choose between two products that vary across multiple dimensions, adding a third option that is clearly inferior to one of the choices makes that choice much more attractive. For example, in one study, most subjects choosing between six dollars in cash and a Cross pen favored the Cross pen; when a third option of an inferior pen was added, the subjects' preference for the Cross pen over the six dollars increased.¹⁶ Even though almost no one chose the inferior pen,¹⁷ its availability induced people who would otherwise have preferred the six dollars to prefer the Cross pen.

The contrast effect can also lead to compromise effects.¹⁸ When presented with a choice of three products of increasing quality, people express a preference for the middle category.¹⁹ The addition of superior or inferior options shifts preferences in the direction of the added option.

The most stark demonstrations of the inconsistency that contrast effects create involve experiments that induce people to pay more for less of a commodity.²⁰ In one study, Christopher Hsee had subjects estimate how much they would be willing to pay for ice cream on a hot day at the beach.²¹ Subjects shown a drawing of eight ounces of ice cream served in a ten-ounce cup reported that they would pay an average of \$1.66 for the serving. Subjects shown a drawing of seven ounces of ice cream served in a five-ounce cup reported that they would pay an average of \$2.26. In other words, people were willing to pay \$0.60 more for less ice cream. When subjects evaluated the choices together, the preferences reverted to a more rational outcome, with people willing to pay \$1.85 for the eight-ounce serving and \$1.56 for the seven-ounce serving.

¹⁴ See Russell B. Korobkin & Thomas S. Ulen, *Law and Behavioral Science: Removing the Rationality Assumption from Law and Economics*, 88 CAL. L. REV. 1051, 1064 (2000) (identifying invariance as a fundamental assumption of rational choice).

¹⁵ Mark Kelman, Yuval Rottenstreich & Amos Tversky, *Context-Dependence in Legal Decision Making*, 25 J. LEGAL STUD. 287, 287 (1996).

¹⁶ See Itamar Simonson & Amos Tversky, *Choice in Context: Tradeoff Contrast and Extremeness Aversion*, 29 J. MARKETING RES. 281, 287 (1992).

¹⁷ Actually, 2 of the 115 subjects chose the inferior pen. See *id.*

¹⁸ See *id.* at 290–92.

¹⁹ See *id.*

²⁰ See generally Christopher K. Hsee, *Less Is Better: When Low-Value Options Are Valued More Highly than High-Value Options*, 11 J. BEHAV. DECISION MAKING 107 (1998) (presenting several demonstrations of this phenomenon).

²¹ See *id.* at 111–14.

This research demonstrates that people do not measure the value of commodities with a single, consistent metric. Different contextual cues can trigger different evaluative processes, even when the cues are irrelevant. Because these different processes lead to different outcomes, inconsistent choices result. In the ice cream example, people are not merely evaluating their desire for ice cream, but are also incorporating their sense of whether they are getting good value into their willingness to pay for the commodity.²² Similarly, compromise effects occur because the introduction of more extreme alternatives alters the metric by which people evaluate the value of commodities.

B. Consistency in Legal Judgments

The phenomena that psychologists have identified as causing inconsistency in consumer choice have also been documented in legal contexts. Mark Kelman and his coauthors reported a series of studies demonstrating contrast effects and compromise effects in legal judgments,²³ including two studies demonstrating compromise effects in verdict decisions by mock jurors. In one of these studies, subjects read a description of a crime; some subjects were asked to choose between a verdict of manslaughter and murder, while others were given verdict options of manslaughter, murder, and murder with special circumstances.²⁴ The availability of the third, more extreme option increased the proportion of subjects who chose murder relative to manslaughter.²⁵ In effect, the availability of the extreme option stretched the continuum on which the subjects had to place the defendant's conduct. The subjects' sense of moderation caused them to shift their evaluations in response to the extreme third option.

In another study of compromise effects by Kelman and his colleagues, subjects read a description of a homicide and were given four possible verdict options: murder with special circumstances, murder, manslaughter, and involuntary manslaughter.²⁶ Subjects were then told that judicial rulings had eliminated either the most severe or the least severe verdict as an option. The elimination of high- or low-severity options affected the proportion of murder verdicts to manslaughter verdicts in a way that revealed compromise effects.²⁷ Subjects who chose from the "upper set" of options (that is, special circumstances murder, murder, and manslaughter, but not involuntary manslaughter) tended to impose harsher verdicts than subjects

²² See *id.* at 113.

²³ Kelman, Rottenstreich & Tversky, *supra* note 15, at 290–300.

²⁴ See *id.* at 290–92.

²⁵ See *id.* at 291–92.

²⁶ See *id.* at 292–95.

²⁷ See *id.* at 294.

who chose from the “‘lower set’” of options.²⁸ In this case, subjects were fully aware of the possible scale, but truncating that scale in one way or another affected how they applied it to the defendant’s conduct.

Kelman and his coauthors also provided three demonstrations of contrast effects in legal judgments.²⁹ They presented results from three studies that used materials that were structurally analogous to the study involving the choice between six dollars and a Cross pen.³⁰ Subjects basically chose between two settlement or jury verdict options, although half of the subjects were presented with a third, clearly inferior option. The third option invariably increased the attractiveness of the option to which it was most apparently inferior.³¹ Although these studies do not demonstrate the phenomenon in actual choices, they do reveal that the legal decision-making context does not, by itself, inspire people to make more consistent choices.

Studies of damage award decisions by mock juries also reveal arbitrariness in how people assess value in the legal system. In cases assessing punitive damage awards, people do not convert their desire to punish liable defendants into a consistent damage award.³² Rather, they seem to use a range of unbounded conversion techniques that are subject to easy manipulation with arbitrary reference points.³³ Furthermore, conversions of legal judgments into dollar amounts also suffer from some of the contrast effects identified in studies of consumer goods.³⁴

In one demonstration of inconsistent dollar conversions in civil cases, Cass Sunstein and his coauthors asked subjects to assign punitive damage awards in two cases—one involving personal injury and the other involving financial fraud.³⁵ Subjects awarded larger damage awards in the personal injury cases when they evaluated the personal injury and financial fraud cases in tandem, than when they evaluated those cases separately from the fraud cases.³⁶ This study is closely analogous to Hsee’s ice-cream study.³⁷ The fraud case in the Sunstein study involved a particularly egregious case of fraud. In determining damage awards, subjects likely made mental comparisons between this

²⁸ See *id.*

²⁹ See *id.* at 295–300.

³⁰ See *supra* notes 16–17 and accompanying text.

³¹ See Kelman, Rottenstreich & Tversky, *supra* note 15, at 296–300.

³² See Cass R. Sunstein, Daniel Kahneman & David Schkade, *Assessing Punitive Damages (with Notes on Cognition and Valuation in Law)*, 107 YALE L.J. 2071, 2103, 2105–07 (1998).

³³ See *id.* at 2142–44.

³⁴ See Cass R. Sunstein, Daniel Kahneman, David Schkade & Ilana Ritov, *Predictably Incoherent Judgments*, 54 STAN. L. REV. 1153 (2002).

³⁵ See *id.* at 1173–74.

³⁶ See *id.* at 1175–78.

³⁷ See *supra* notes 20–21 and accompanying text.

case and typical, less egregious cases of fraud, driving them to want to punish this conduct more severely. Subjects may have felt that this financial fraud case was worse than their general conception of fraud cases, just as Hsee's subjects may have seen seven ounces of ice cream as a surprisingly generous portion for a five-ounce cup. In contrast, the personal injury was probably less egregious than the subjects expected in a personal injury case, just as Hsee's subjects saw less ice cream than they expected in the ten-ounce cup. When viewed together, however, subjects were reminded that personal injuries really are much worse than financial losses (just as more ice cream is worth more money); thus, they awarded more to the personal injury cases.

Once again, these laboratory demonstrations cannot prove conclusively that the same phenomena affect real legal judgments. In the case of damage awards, however, studies of decisions by judges and juries support the proposition that these phenomena do influence real judgments. Analysis of judgments made by actual judges and juries reveals the kind of pattern Sunstein and his colleagues' laboratory work predicts.³⁸ Even the unusual hypothesis suggested by Sunstein and his colleagues' contrast-effect study finds empirical support in real decisions.³⁹ Their study suggests that experience should produce more consistency in assignment of damage awards because experienced decision makers are more comparable to subjects who have seen the two cases together. An experienced decision maker should not produce an excessive award for an extreme case involving a not-so-egregious type of conduct because the experienced decision maker is well aware of other, more serious, categories of conduct. In the legal system, this suggests that judges should be less erratic in awarding damages than juries. In particular, judges should tend to award more money than juries in personal injury cases, and less money than juries in financial fraud cases. Studies of actual awards suggest exactly this pattern.⁴⁰

C. Cognitive Consistency and Punishment in the Criminal Context

Although the implications of cognitive consistency for the system of civil liability are important, their implications in the context of the

³⁸ See generally Theodore Eisenberg, John Goerdt, Brian Ostrom, David Rottman & Martin T. Wells, *The Predictability of Punitive Damages*, 26 J. LEGAL STUD. 623 (1997) (demonstrating that, like subjects in the experiment by Sunstein and his colleagues, real jurors produce damage awards that are highly skewed, but correlated with the degree of harm caused).

³⁹ See Theodore Eisenberg, Jeffrey J. Rachlinski & Martin T. Wells, *Reconciling Experimental Incoherence with Real-World Coherence in Punitive Damages*, 54 STAN. L. REV. 1239, 1252-56 (2002).

⁴⁰ See *id.*

criminal justice system are even more serious. Criminal sentences are unjust to the extent that they are influenced by the kinds of cognitive incoherence that have been documented in the context of consumer choice and legal decision making. The studies Kelman and his colleagues conducted on compromise effects in mock criminal verdicts are among the more troubling examples of this inconsistency.⁴¹ Whatever harm consumers suffer when buying a stereo because of manipulative presentation of contrasting alternatives, the harm suffered by a criminal defendant when a jury is manipulated into delivering a more severe verdict is much more serious.

A conceptual extension of the recent work discussed above reveals how this inconsistency can arise in criminal cases. Sunstein and his coauthors demonstrated that inexperienced decision makers will tend to lose sight of the severity of the type of crime when the particular crime at issue is an extreme example of its type.⁴² Consequently, decision makers will punish more harshly than is sensible those types of crimes that are not so severe when the particular crime is among the worst of its type. They will also tend to underpunish crimes that are less severe instantiations of more serious crimes. To be sure, the criminal justice system does not usually rely on lay persons to assign punishment. Judges perform most criminal sentencing. In cases involving the death penalty, however, lay persons do have a role. Although the interjection of the jury into the capital sentencing process is meant as an extra-procedural safeguard, it might actually interject inconsistency at the very point in the criminal justice system when consistency is most crucial. If the psychological phenomena identified in consumer-choice research hold true for decisions in capital cases, the death penalty may be hopelessly arbitrary.

Cognitive inconsistency can arise in capital cases in several ways. First, jurors might suffer from contrast or compromise effects due to widely publicized capital cases in the media. Jurors might find it hard to resist comparing a defendant's conduct with that of defendants in other, well-publicized homicide cases. A recent, well-publicized, and extremely heinous crime might "stretch the scale" by which jurors measure homicide, thereby raising the threshold of heinousness necessary to impose a death sentence. Well-publicized homicides that jurors perceive as involving conduct less heinous than that of the defendant in a given case might have the opposite effect. Simply stated, recent public memory might create contrast or compromise effects in jurors. Inasmuch as the public memory can be short, and recent publicity for other murders are well beyond a defendant's control, such effects would create an arbitrariness in capital cases that

⁴¹ See *supra* notes 23–28 and accompanying text.

⁴² See Sunstein, Kahneman, Schkade & Ritov, *supra* note 34, at 1175–76.

would be hard to avoid. Similarly, the fate of a co-conspirator might also create the kinds of contrast or compromise effects that create arbitrariness.

To determine whether these kinds of cognitive inconsistency in judgment can influence assessments in capital cases, we conducted a "mock juror" study on a sample of undergraduate students. Our basic hypotheses were that contrast and compromise effects would affect punishment decisions. As an initial matter, we used a noncapital case to ascertain whether the kinds of contrast effects seen in the consumer-choice literature and in punitive damages assessments would be present in criminal sentencing by lay persons. In this study, we sought to replicate the findings of Sunstein and his coauthors in the context of jury determinations on years of imprisonment. We then sought to extend the research to capital cases using three different sets of materials.

II

METHODS

Our research methodology followed the logic of much of the research on judgment and choice. We created short, relatively simple descriptions of a number of crimes and a series of questions about the appropriate punishment for these crimes. Our methods were akin to asking subjects to read a description of a consumer product and make judgments about that product.

Studies like this have obvious limitations. We provided subjects with only brief descriptive materials—far short of the extensive information available to capital jurors. Undergraduates also differ demographically from a jury-eligible population (although the undergraduates were, for the most part, eligible to serve on juries). We also report the judgments of jurors, not juries, insofar as we did not allow for group deliberation. Most importantly, the subjects in our study knew full well that they were not actually deciding whether to sentence a real human being to death. Despite these limitations, we nevertheless feel that this simple demonstration of the potential role of cognitive inconsistency in capital decision making is of value. Although there are numerous empirical studies of the death penalty, using both real juries and mock juries, we are not aware of any other effort to relate the psychological research on cognitive inconsistency in judgment to decision making in capital cases. Thus, this study may be best viewed as a first step in assessing whether contrast effects influence sentencing decisions in capital cases.

Our subjects consisted of 277 undergraduate students enrolled in a psychology course at the University of Oregon. The subjects participated in the study as partial fulfillment of course requirements.

The materials consisted of a seven-page survey. It included four target survey items (one on each page, except for the first item, which was two pages long), a page containing questions about attitudes toward the death penalty, and a final page that asked for demographic information.⁴³ The order of these materials did not vary by subject. In all cases, the first page began with the label "Sentencing Decisions"; no further instructions were given other than what is included in the descriptions below. All subjects read and responded to the materials during a class session.

We present the materials and results in the order that follows the logic of our hypotheses, rather than the order in which the subjects reviewed the materials (although we note the actual presentation order as well). In all cases, the materials presented the subjects with one of three different versions: a control condition intended to elicit baseline preferences and two conditions intended to test for contrast effects that would push the choice in opposite directions. The first survey item tested for contrast effects in quantitative criminal sentences and the other three tested for contrast effects in the binary decision of life or death in a capital case. These three studies tested for contrast effects arising from three different sources: (1) a co-conspirator's sentence; (2) a similar type of crime; or (3) a crime differing in severity. In the latter two cases, in addition to the binary decision, we also evaluated the subjects' assessments of the propriety of the death penalty for the defendants described in the materials.

A. Contrast Effects on Years of Imprisonment

The third item in our materials was our extension of Sunstein and his coauthors' findings on punitive damages to jury determinations of years of imprisonment. Our goal was to demonstrate that the kinds of contrast effects observed in consumer choice and other legal contexts extend to criminal sentencing. The logic of this study mimicked that of Sunstein and his coauthors on punitive damage awards and of Hsee's work on preference reversals: We presented subjects either with an outrageous exemplar of a generally less harmful type of crime or with a somewhat benign exemplar of a generally more harmful type of crime, or with both. Our theory was that when evaluated separately, the more outrageous crime would draw substantially more punishment than the somewhat benign crime. When asked to evaluate both crimes together, however, subjects would be reminded that the somewhat benign crime was still the type of crime that is generally more severe than the more outrageous crime.

⁴³ Although information about the subjects' gender was collected, a data-entry error prevented the information from being properly recorded. Thus, gender data are not discussed as part of this analysis.

Because sentencing was the target in this case, we asked subjects to assume the role of a judge. Specifically, we instructed them to "[s]uppose that you are sitting as JUDGE in the following case [or two cases]. Please review the facts and answer the question which follows." The materials then presented either one of the two crimes or both. The outrageous exemplar of a less serious crime was a fraud. The description read as follows:

Q is a 22-year-old male convicted of defrauding an elderly widow with a phony home-repair business. Testimony indicated that Q convinced his victim that her home was in desperate need of added insulation (even though it was in nearly perfect shape). He convinced her that the federal government had adopted a new program to assist senior citizens in insulating their homes as a way of reducing energy consumption in the Pacific Northwest. He promised her that he would take care of the program application, which would reimburse her for all expenses. He then had her take out a mortgage on her home to finance "up-front costs" of the expensive project and he subsequently had her withdraw all of her savings to finance the rest of this project. Rather than do any repairs, he spent the money on a lavish vacation and a sports car (which he subsequently destroyed in an episode of reckless driving before he acquired insurance on it). He was then arrested for defrauding the elderly woman. Unfortunately, she has been able [sic] to make payments on their [sic] mortgage and the bank is threatening to foreclose on her home. The stress also caused her to experience severe heart problems, which required hospitalization.

The benign exemplar of a more serious crime was an armed robbery. The description read as follows:

P is a 22-year old male convicted of armed robbery of a convenience store in his neighborhood. Testimony and a surveillance video indicated that P walked in, handed a note to the clerk which demanded money, and opened his coat to show the clerk a handgun. The clerk handed him money from the cash register and triggered a silent alarm. Police apprehended P while he was running away. P's gun was not loaded.

In both cases, the materials informed the subject of the sentencing range with the following: "In Oregon, a conviction for [fraud/an armed robbery] carries a sentence of between 2-15 years. You have complete discretion to sentence [Q/P] within this range." Subjects were then asked to sentence the defendant or defendants by filling in the following: "I would sentence [Q/P] to ___ years in prison for [fraud/armed robbery]."

We predicted that when evaluated separately, subjects would be outraged by such an egregious case of fraud and consequently would provide sentences at the high end of the available range, but would

not be so outraged by the armed robber and consequently would provide sentences at the low end of the range. When viewing both scenarios together, however, subjects would recognize that armed robbery is a generally more serious crime than fraud, and would feel compelled to give a lengthier sentence to the armed robber.

B. Contrast Effects Arising from a Co-Conspirator's Sentence

The second item in our materials evaluated the influence of contrast effects arising from the fate of a co-conspirator. Our assumption in this case was that the availability of an inferior sentence alternative would influence the choice between a sentence of life and death. Subjects in the control condition had two choices, and subjects in the experimental condition had three choices. The experimental conditions included the two target choices available in the control condition plus one of two alternatives, each designed to be perceived as inferior to one of the two choices available in the control condition. The hypothesis was that the availability of an inferior choice would influence the target choice. In this case, the target choice was whether a defendant merited a death sentence or life in prison. As the description below notes, we introduced a co-conspirator to create inferior sentence alternatives.

In this item subjects were asked to "[s]uppose that you are a criminal prosecutor in a case with the following facts." The materials then presented the following description of a crime:

A and B have been convicted of committing several murders during the course of a bank robbery. A is a male, 30 years old, who has been convicted of numerous felonies, including the armed robbery of a convenience store. A has been in prison most of his adult life. B is his younger, 20 year-old brother who has no criminal record other than a shoplifting incident as a juvenile.

Testimony at trial indicated that A had recently been paroled and had convinced B to accompany him on an armed robbery. A had told B that he needed help to do "one last bank job to get me on my feet, so I can lead an honest life." After B agreed to help him, A borrowed two semi-automatic handguns and planned the holdup.

Testimony of the witnesses to the robbery were quite consistent (and were supported by a surveillance camera of the bank). Both A and B entered the bank together, but B remained by the door during the robbery. A ordered everyone onto the floor and then demanding [sic] that a cashier fill a garbage bag with money. A shouted at the cashier, calling her "too slow" and shot her to death. B apparently shouted to A, saying, "murder was not part of the plan." A then had the bank manager take the cashier's place. After the manager filled the bag, A shot him as well and the two prepared

to leave. As they did, a guard on the floor reached for his gun and both A and B began firing. B, being inexperienced with weapons, ended up spraying the ceiling with bullets while A shot the guard several times. One of the bullets from B's gun ricocheted off of a light fixture and also hit the guard. Forensic analysis of the bullets in the guard's body were inconclusive as to which bullet or bullets caused his death. As they left the bank, the two were apprehended by nearby police officers, who saw the two fleeing the bank with guns drawn.

The materials then asked the subject to recommend a sentence combination that she felt was appropriate. Specifically, the instructions read: "The judge has asked you to recommend an appropriate sentence for both A and B. The judge is considering the following [two/three] options. Which is the most appropriate sentence combination? (Please check one):". The materials then listed either two or three sentence combinations, depending on the condition.

The two target sentence alternatives were designed to assess the merits of the death penalty for the main perpetrator in the hypothetical. The primary choice was as follows: "death by lethal injection for A; 25 years in prison with no chance of early parole for B" or "life in prison without parole for A; 25 years in prison with no chance of early parole for B." We attempted to create inferior choices by adding a third alternative meant to affect the basic choice between life and death for A. In one case, the third option was meant to inspire leniency on A by offering a choice that was too lenient on B: "life in prison without parole for A; 10 years in prison with no chance of early parole for B." This choice was intended to be inferior to the "life/25 years" choice by being similar to that choice, but too lenient for B. The second experimental condition was meant to inspire a harsh sentence for A by offering a choice that was too harsh on B: "death by lethal injection for A [and] death by lethal injection for B." This choice was intended to be inferior to the "death/25 years" choice by being similar to that choice, but too harsh for B.

We predicted that the availability of an inferior alternative would create a contrast with the most comparable option, thereby increasing the proportion of subjects who chose that comparable option. The inclusion of a third, inferior option can also be viewed as creating compromise effects. The addition of a third, more lenient alternative extends the range of sentences downward, whereas the addition of a third, harsher alternative extends the range of sentences upward. As in the studies by Kelman and his coauthors, we felt that this manipulation would alter the choices made.

C. Contrast Effects Arising from a Similar Type of Crime

The first item in our materials evaluated the influence of contrast effects arising from exposure to a similar, but less egregious, crime. The study was intended to simulate the consumer-choice contrast studies closely. In those experiments, the choice between two products was influenced by the introduction of a third product that was comparable to one of the two primary choices, but inferior on an important dimension. Here, we sought to influence subjects' assessments of the relative merits of two crimes for the death penalty by introducing a third alternative that clearly presents an inferior profile for the death penalty. We theorized that the contrast with an inferior alternative would make the similar crime seem more appropriate for the death penalty, just as the addition of an inferior consumer choice affects consumer purchasing decisions.

To test for the hypothesized contrast effect, we asked subjects to identify the defendant that was most appropriate for a death sentence from among a group of defendants. The materials asked subjects to choose between two different types of murders as being most appropriate for the death sentence. One type was a domestic homicide committed in a grizzly fashion, and the other was a murder-for-hire. Our goal in writing these two scenarios was to present two crimes that subjects would perceive as equally appropriate for the death penalty. All subjects reviewed these two crimes. In the control condition, subjects chose one of these two crimes as more appropriate for the death penalty. In two experimental conditions, subjects also reviewed one of two other crimes: either another domestic violence murder or another murder-for-hire. In both cases, however, the additional crime presented a much weaker case for the death penalty than either of the two crimes that all subjects reviewed. Thus, subjects in the experimental conditions reviewed a domestic murder and a murder-for-hire that were similar to each other in terms of merit for the death sentence plus a third crime that was similar to one of the target crimes in terms of type of murder, but was inferior in terms of merit for the death penalty.

At the outset of this item, the materials asked subjects to "[s]uppose that you are a criminal prosecutor with limited resources. You are considering whether to seek the death penalty in one of the following cases. Please read the case summaries and then answer the questions which follow." The materials presented brief descriptions of either two defendants (in the control condition) or three defendants (in the experimental conditions).

The two target crimes consisted of a grizzly domestic homicide and a murder-for-hire. The text of the target domestic homicide was as follows:

W is a 24 year-old male who has been convicted of a murdering [sic] his live-in girlfriend and the girlfriend's 3-year-old daughter (which she had from a previous marriage). W had been living with the victim for two years and they had recently gotten engaged. Testimony at trial from neighbors indicated that W and this victim had begun fighting; W accused his girlfriend of resuming her relationship with her ex-husband. At trial, W confessed that during one of their arguments, while W was very drunk, he began beating his girlfriend until she was nearly unconscious. In a rage, he then tied her up and slit her throat. He then entered the daughter's bedroom and stabbed the child to death. W had never been convicted of any other crime. As a double-murderer, W is eligible for the death penalty.

The text of the target murder-for-hire was as follows:

M is a 24-year old male who has been convicted of murder. At trial, testimony indicated that M committed the murder of a complete stranger in exchange for \$5,000. The money was paid by the nephew of the victim, who hoped to gain a large inheritance from the wealthy victim's estate. The nephew apparently contacted M through a friend connected with organized crime, who testified at trial that M regularly commits murder for hire (although the police have been unable to connect M to any other murders). M has been convicted of several other crimes, including robbery and possession of an illegal automatic weapon. As a murder committed in exchange for something of value, M is eligible for the death penalty in this jurisdiction.

As noted above, all subjects read descriptions of these two crimes. In one of the two experimental conditions, subjects also read a description of a second domestic murder designed to present a less compelling case for the death penalty. The text of this crime was as follows:

C is a 24 year-old female who has been convicted of murdering her husband and one of her husband's friends. C had been married to the victim for several years. Testimony at trial indicated that C was the victim of domestic abuse by her husband for at least a year. The victim apparently physically abused her on several occasions, resulting [in] at least one a visit [sic] to a hospital emergency room with a broken nose. At trial, C confessed that her husband had come home drunk with a friend of his and he began verbally threatening her in front of the friend. She left, broke into a neighbor's house, and stole the neighbor's shotgun. She then returned home and shot her husband and his friend to death. C never been [sic] convicted of any other crime. As a double-murderer, C is eligible for the death penalty.

The other experimental condition included the two target crimes plus a second murder-for-hire designed to present a less compelling case for the death penalty. The text of this crime was as follows:

F is a 24-year old male who has been convicted of murder. At trial, testimony indicated that F committed the murder of a stranger as part of an initiation ritual for a local gang in his neighborhood. The leader of this gang convinced F that he should join their group. Apparently, murder had not been a normal initiation ritual in this gang, but the gang leader told F that he needed to kill a rival gang leader to become a member. F was told that if he successfully committed the murder, the gang would give him jacket [sic] with gang colors. Testimony also indicated that F has a learning disability and, as a result, had been unable to hold steady employment. He had lived life [sic] with his mother, who had been able to keep him out of trouble before this time. As a murder committed in exchange for something of value, F is eligible for the death penalty in this jurisdiction.

The materials then asked the subjects to identify which of the two or three crimes was most appropriate for the death penalty. Specifically, the materials asked: "As the prosecutor, you are charged to apply for the death penalty where it is warranted but because of the great expense in pursuing a death penalty, you can only ask for it in one of the cases—which case would you ask for the death penalty?" The materials then listed the two or three letters corresponding to the criminals in the above crimes, with a blank line in front of each letter for the subjects to check. The materials also asked subjects to evaluate the propriety of the death penalty for each of the two or three criminals on a seven-point scale. The numbers one through seven were listed below the question, "How appropriate is the death penalty as a punishment for [the defendant]? (circle one)," with three qualitative descriptions below the numbers: "1 (Very appropriate)," "4 (somewhat appropriate)," and "7 (not at all appropriate)."

We predicted that the addition of a crime that presented a less compelling case for the death penalty would facilitate a contrast effect with respect to the similar type of crime, thereby increasing the proportion of subjects who felt that the contrasting target crime was the most appropriate case for the death penalty.

D. Contrast Effects Arising from a More or a Less Severe Crime

The fourth (and final) item in our materials evaluated whether extreme cases create contrast effects. In this item, the goal was to expose subjects to fact patterns that presented either a strong case or a weak case for the death penalty in order to determine whether evaluation of such extreme cases would affect the evaluation of a case that presented a close call for the death penalty. We theorized that evalu-

ating a strong case for the death penalty would have the effect of “stretching” what subjects perceive as the higher end of the criminal activity spectrum that might warrant the death penalty. Thus, after evaluating a strong case, subjects might perceive an intermediate case for the death penalty as more benign than they otherwise would have. In contrast, evaluation of a weak case for the death penalty might act to “compress” subjects’ perceptions of the range of criminal activity that might warrant the death penalty. After evaluating a weak case, subjects might see an intermediate case as more deserving of the death penalty.

In this item, subjects were asked: “Suppose that you are sitting as a JUDGE in the following [case/two cases] and must determine whether the death penalty is an appropriate punishment.” Subjects in the control condition then evaluated our intermediate case only, the text of which read as follows:

S is a 24-year-old male who has been convicted of murder, breaking and entering, and robbery. Testimony at trial, supported by video from a surveillance camera, revealed that S broke into a jewelry shop in the early morning hours, in an apparent burglary attempt. S admitted that he planned the robbery and believed that the store would be empty. On most days, the store would have been empty, but on this occasion, the owner happened to be inside conducting an inventory. As S was putting items into a sack, the storeowner startled him. S testified that he “shot the owner, in a panic.” S then fled as the storeowner bled to death. S was caught after attempting to sell the stolen jewelry at a nearby pawn shop a few hours later. S had been convicted of burglary in the past, but had never been convicted of a violent crime. In Oregon, murder during a robbery is punishable by death.

The materials then asked the subjects to assess on a seven-point scale whether the death penalty would be an appropriate punishment for this defendant. The numbers one through seven were listed below the question, “How appropriate is the death penalty as a punishment for [the defendant]? (circle one),” with three qualitative descriptions below the numbers: “1 (Very appropriate),” “4 (somewhat appropriate),” and “7 (not at all appropriate).” The materials then stated, “I would sentence [the defendant] to death: (please circle one),” with the words “Yes” and “No” listed below.

In the two experimental conditions, subjects also evaluated this intermediate case for the death penalty, but before doing so they evaluated a fact pattern that presented either a strong or a weak case for the death penalty. The description of the strong case was as follows:

U is a 25 year-old male who has been convicted for the murder of two people. Testimony at trial revealed that U was selling cocaine in his neighborhood. The two victims were a young husband and wife

who were distributing cocaine for him. U told friends that he believed that the couple were skimming some of the cocaine for themselves and holding back some of the money they made selling "his cocaine". He told several witnesses that he "needed to teach the neighborhood a lesson." Witnesses saw him enter the couples' [sic] apartment with his gun drawn. One person walking past the window actually saw U shoot the wife. Neighbors reported they heard much screaming from the apartment, and one neighbor called the police. [The] [p]olice apprehended U on his way out of the apartment. They found the young couple dead in the apartment, covered in blood. Their 5 month-old infant was found in a rear room, brutally stabbed to death. The husband's ear was cut off and the coroner's report indicate [sic] that the wife had been raped, in front of her husband's body before being shot in the head. DNA evidence revealed that U committed the rape.

The description of the weak case was as follows:

V is a 20-year old male who has been convicted of first degree murder arising from a bar fight. Testimony at trial indicated that V was drinking heavily at a crowded bar that had a reputation for violence, in Portland, Oregon. V got into a shouting match with two other people, apparently over which sporting event to watch on the bar's television. V was apparently punched and knocked to the ground by someone and the bartender ordered V to leave. V became very angry, shouted that he would "get back at them", and then walked outside. He got into his car, and deliberately drove it straight through a plate glass window into the bar. In the process, V killed two people and injured several others. V had been arrested before for assault, and had some similar incidents as a juvenile.

Following the written descriptions, the materials also presented the same two questions concerning appropriateness of the death penalty for these two crimes. Thus, in the experimental conditions, subjects read either the strong or the weak case, evaluated that case for the death penalty, and then read and evaluated the intermediate case.

We predicted that evaluation of the strong case would lead fewer subjects to support the death penalty for the intermediate case as compared to the control group, and that evaluation of the weak case would lead more subjects to support the death penalty as compared to the control group.

E. Demographic Information

The remaining two pages of the survey asked subjects to provide a set of demographic information. We requested this information both to get a sense of the makeup of the subject population and to identify some demographic variables that might correlate with answers to our survey items.

Previous work on the death penalty indicates that some people assert that they would refuse to impose the death penalty in any case.⁴⁴ The legal system itself recognizes that some people refuse to consider imposing the death penalty and excludes from service on capital juries those who express such beliefs.⁴⁵ Although at least one study shows that even these people will nevertheless tend to support the death penalty in some specific cases, they are far less likely to impose the death penalty than people who express a willingness to consider imposing the death penalty.⁴⁶ This variable has been the target of considerable previous research; thus, we sought to identify its influence on our predictions.

To assess attitudes toward the death penalty, we asked two questions (both taken from other researchers' previous work).⁴⁷ First, we asked subjects: "Assume that you have been called as [a] juror in a first degree murder trial in which the death penalty might be imposed. Which of the following statements best describes your attitude towards the death penalty: (check one)." Below this question were two statements: "I would never be willing to impose it in any case, no matter what the evidence was" and "I would consider voting to impose it in at least some cases." Second, we asked, "Which of the following best describes your attitudes towards the death penalty," below which were listed four statements: "I strongly favor the death penalty"; "I somewhat favor the death penalty"; "I somewhat oppose the death penalty"; and "I strongly oppose the death penalty." We report our results both inclusive and exclusive of the set of subjects who claimed that they would never vote to impose the death penalty.⁴⁸

The final page requested more general demographic information. Specifically, we asked subjects the following: "Your age"; "Your gender"; "Your year in college (Freshman, Sophomore, Junior, Senior, or Postgrad)"; "Have you ever been the victim of a violent crime?"; "Have you ever been the victim of a property crime (such as theft)?"; "Have you ever committed a violent crime?"; "Have you ever

⁴⁴ See generally Craig Haney, *On the Selection of Capital Juries: The Biasing Effects of the Death Process Qualification*, 8 LAW & HUM. BEHAV. 1, 21 (1984) (reviewing the issues presented by the disqualification of potential jurors who refuse to impose the death penalty).

⁴⁵ People who claim that they would never vote to impose death can constitutionally be excluded from service on capital juries. See *Witherspoon v. Illinois*, 391 U.S. 510, 522 n.21 (1968).

⁴⁶ See Michele Cox & Sarah Tanford, *An Alternative Method of Capital Jury Selection*, 13 LAW & HUM. BEHAV. 167, 176-78 (1989).

⁴⁷ Our questions were adapted from Robert Fitzgerald & Phoebe C. Ellsworth, *Due Process vs. Crime Control: Death Qualification and Jury Attitudes*, 8 LAW & HUM. BEHAV. 31 app. at 49-50 (1984).

⁴⁸ The subjects' attitudes toward the death penalty did not have any effect on our results over and above their effects on the binary question of whether subjects would ever vote to impose the death penalty.

committed a property crime (such as theft)?"; "Is English your first language (native language)?"; and "Are you an American citizen?"⁴⁹

III RESULTS

A. Contrast Effects on Years of Imprisonment

Table 1 presents the results from our study of whether contrast effects influenced subjects' assessments of the appropriate number of years of imprisonment for noncapital crimes. The results provided evidence of a contrast effect. The 64 subjects who evaluated only the fraud scenario provided a mean sentence of 12.27 years, as compared to a mean sentence of 6.83 years among the 76 subjects who evaluated the robbery, a difference of 5.44 years. Among the 137 subjects who evaluated both crimes, the mean sentences were 10.39 years and 7.63 years: a difference of 2.76 years. The gap between the mean sentences narrowed by 2.68 years, which is a statistically significant difference.⁵⁰

TABLE 1
MEAN YEARS IN PRISON BY CONDITION

Condition (number of subjects)	Robbery	Fraud
Separately (76/64)	6.83	12.27
Together (137)	7.63	10.39

B. Contrast Effects Arising from a Co-Conspirator's Sentence

Our test of the potential contrast effect of a co-conspirator's sentence necessarily described a fact pattern involving two defendants, but we were primarily interested in the subjects' decisions whether to sentence the main perpetrator to death or life. The sentence for the secondary defendant was designed to create contrast effects on the sentencing determination for the primary defendant. In the control condition, 53.9% of the 89 subjects chose to sentence the primary defendant to death. When a third sentencing alternative was offered that included a less severe sentence for the secondary defendant, 53.4% of the 58 subjects who selected one of the two sentencing alternatives available in the control condition selected death for the pri-

⁴⁹ None of these variables affected the analysis.

⁵⁰ $t(265) = 5.15$, $p < .001$. The test performed compares the difference between the means in the two separate conditions with the difference between the means in the concurrent condition. Formulas used to make the calculation are available in R. LYMAN OTT, AN INTRODUCTION TO STATISTICAL METHODS AND DATA ANALYSIS 261-63 (4th ed. 1993). Throughout this Article, the term "significant" is used to denote rejection of the null hypothesis at $p < .05$.

mary defendant. In addition, 40.8% of the 98 subjects in this condition chose the option involving a life sentence for the primary defendant and a less severe sentence for the secondary defendant. This percentage did not differ significantly from the percentage in the control condition.⁵¹ When a third sentence alternative was offered that included a more severe sentence for the secondary defendant, 54.8% of the 84 subjects who selected one of the two sentence alternatives available in the control condition selected death for the primary defendant. Additionally, 6.7% of the 90 subjects in this condition chose death for both defendants. This percentage also did not differ significantly from the percentage in the control condition.⁵²

Although exclusion of the subjects who claimed that they would never vote for death increased the overall percentage of subjects who favored death for the primary defendant, exclusion of these subjects did not otherwise affect the analysis. Without these subjects, 58.2% favored death in the control condition, as compared with 57.7% and 66.2% in the two experimental conditions offering weaker and harsher sentences for the co-conspirator, respectively. These percentages did not differ significantly from the percentage in the control condition.⁵³

C. Contrast Effects Arising from a Similar Type of Crime

Table 2 presents the results from our study of the contrast effects caused by comparisons to similar crimes.

TABLE 2
RESULTS FROM CONTRAST WITH SIMILAR CRIMES

Condition (number of subjects)	% Choosing Domestic Murder as Most Appropriate	Appropriateness Measure (higher value indicates less appropriate)	
		Domestic	For Hire
Control (38)	65.8	2.90	3.29
Domestic Contrast (127)	73.6*	2.48	3.32
For-Hire Contrast (112)	75.9	2.63	3.06

* excludes 6 subjects who chose the "inferior" domestic contrast as most appropriate.

As Table 2 suggests, the contrast effect did not influence the subjects' decisions. Of subjects in the control condition, 65.8% chose the domestic murder as most appropriate for the death penalty. Although this number rose to 73.6% for the subjects who also evaluated a do-

⁵¹ By Fisher's exact test, $p = .54$.

⁵² By Fisher's exact test, $p = .52$.

⁵³ By Fisher's exact test, $p = .55$ and $p = .21$ for the comparison with the weaker and stronger contrasts, respectively.

mestic murder designed to present a less compelling profile for the death penalty, the difference was not significant.⁵⁴ Similarly, although the percentage selecting the domestic murder as most appropriate for the death penalty rose to 75.9% when subjects also evaluated a murder-for-hire designed to present a less compelling profile for the death penalty, the difference was not significant.⁵⁵

The availability of contrasting crimes also had little effect on the subjects' beliefs that a crime warranted the death penalty. The mean appropriateness measure in the control condition (2.90) was reduced (meaning that subjects evaluated the crime as more appropriate for the death penalty) when a contrasting domestic murder was present (2.48). It was also reduced, however, by the contrasting murder-for-hire. More importantly, the differences between these conditions were not significant.⁵⁶ Similarly, the ratings of the death penalty appropriateness of the murder-for-hire did not differ significantly by condition.⁵⁷

The lack of significant contrast effects cannot be attributed to the failure of the stimulus materials to offer suitably contrasting crimes. The ratings of appropriateness confirm that the contrasting crimes present inferior profiles for the death penalty. Only 6 of the 127 subjects (4.7%) in the domestic contrast condition chose the "inferior" domestic murder as the most appropriate for the death penalty, and they rated the murder as more appropriate for the death penalty on the seven-point scale.⁵⁸ None of the subjects stated that the inferior "for-hire" murder was the most appropriate for the death penalty, and they also rated the murder as more appropriate for the death penalty on the seven-point scale.⁵⁹

Exclusion of the 48 subjects (17.3% of the 277 total subjects) who claimed that they would never impose the death penalty had little effect on the analysis. With these subjects excluded, the percentage who favored the domestic murder in the control condition was 68.8% (22 of the 32 remaining subjects) as compared with 79.4% in the condition with the contrasting domestic murder (77 of the 103 remaining subjects, excluding the 6 subjects who chose the contrasting domestic murder) and 76.6% in the condition with the contrasting murder-for-hire (72 of the 94 remaining subjects). The difference in percentages

⁵⁴ By Fisher's exact test, $p = .23$.

⁵⁵ By Fisher's exact test, $p = .16$.

⁵⁶ $F(2, 274) = 0.63$, $p > .5$.

⁵⁷ $F(2, 274) = 0.69$, $p > .05$.

⁵⁸ The mean difference was 1.28 points, which was significantly different. $t(110) = 8.00$, $p < .001$.

⁵⁹ The mean difference was 1.98 points, which was significantly different. $t(125) = 9.45$, $p < .001$.

across these conditions was not significant.⁶⁰ Also, controlling for the influence of subjects who claimed they would never impose the death penalty had no effect on the stated degree of appropriateness of the death penalty for the target murder-for-hire case: the differences in appropriateness across the three conditions were still not significant.⁶¹ Controlling for the influence of these subjects on the target domestic murder, however, did affect the analysis.⁶² In this case, the contrast with the domestic murder seemed to make subjects perceive the death penalty as somewhat more appropriate in the domestic contrast condition.

D. Contrast Effects Arising from a More or a Less Severe Crime

Table 3 presents the results for our study of the effect of contrasting appropriateness on the death penalty. In the control condition, 25.4% of the subjects chose death. Although only 18.8% of the subjects who first evaluated the strong contrast case for death chose a death sentence in the target case, the difference was not significant.⁶³ Furthermore, evaluating the weak case for death also reduced the percentage favoring death (to 21.9%), which also was not a statistically significant difference.⁶⁴ The subjects' mean ratings of appropriateness for the death penalty likewise did not differ significantly by condition.⁶⁵

The subjects' assessments of the appropriateness of the death penalty also provided the assurance that the contrasting fact patterns actually presented strong and weak cases for death. Subjects who evaluated both the strong case and the target case assigned significantly higher ratings to the target case (meaning that they considered it less appropriate for the death penalty).⁶⁶ Also, 72.7% of these subjects favored the death penalty for the strong case, which was significantly greater than the 18.8% of the subjects who favored death for the target case.⁶⁷ Subjects who evaluated both the weak case and the target

⁶⁰ Fisher's exact test on control versus contrasting domestic murder: $p = .16$. Fisher's exact test on control versus contrasting murder-for-hire: $p = .26$.

⁶¹ For the main effect of condition, $F(2, 271) = 0.50$, $p > .5$. For the interaction between condition and the subjects' willingness to impose death, $F(2, 271) = 2.21$, $p > .10$. Willingness to impose the death penalty had a significant effect. $F(1, 271) = 39.72$, $p < .001$.

⁶² For the main effect of condition, $F(2, 271) = 3.35$, $p < .05$. For the interaction between condition and the subjects' willingness to impose death, the effect was marginally significant: $F(2, 271) = 2.33$, $p < .10$. Willingness to impose the death penalty had a significant effect: $F(1, 271) = 63.01$, $p < .001$.

⁶³ By Fisher's exact test, $p = .19$.

⁶⁴ By Fisher's exact test, $p = .40$.

⁶⁵ $F(2, 274) = 1.11$, $p = .33$.

⁶⁶ This difference was significant; $t(152) = 15.6$, $p < .001$.

⁶⁷ By Fisher's exact test, $p < .001$.

TABLE 3
CONTRAST EFFECTS WITH CRIMES OF VARYING SEVERITY

Condition (number of subjects)	% Choosing Death	Appropriateness Measure (higher value indicates less appropriate)
Control (59)	25.4	3.78
Strong Contrast (154)	18.8	3.91
Weak Contrast (64)	21.9	4.20

case assigned significantly lower ratings to the target case (meaning that they considered it more appropriate for the death penalty).⁶⁸ Also, only 9.4% of the subjects in this condition favored the death penalty for the weak case, which was significantly less than the 21.9% of those subjects who favored the death penalty for the target case.⁶⁹

Removing the subjects who stated that they would refuse to vote for the death penalty did not significantly affect this analysis. As to the binary choice (death or life), 25.4% of the remaining 59 subjects in the control condition favored death; 21.0% of the remaining 124 subjects in the strong contrast condition favored death; and 26.4% of the remaining 53 subjects in the weak contrast condition favored death. These differences were not significant.⁷⁰ As to the "appropriateness" ratings, after removing subjects who stated that they would never vote for the death penalty, the mean appropriateness ratings were 3.69, 3.68, and 3.93 in the control, strong contrast, and weak contrast conditions, respectively. The differences were also not significant.⁷¹

Interestingly, among the subjects who claimed that they would never vote for the death penalty, no one in the control condition and no one in the weak contrast condition supported the death penalty for the target case, but 3 of the 30 subjects (10%) in the strong contrast condition supported the death penalty for the target. It is possible that exposure to the strong case for death made these subjects realize that they would, in fact, vote for the death penalty in some instances, and then led them to vote for death in a close case, whereas they would not otherwise have done so. However, 1 of these 3 was 1 of the 22 who voted against death in the strong case for the death penalty. The sample size is also too small to provide much support for this theory.

⁶⁸ This difference was significant: $t(62) = 2.22$, $p < .03$.

⁶⁹ By Fisher's exact test, $p = .04$.

⁷⁰ By Fisher's exact test, $p = .31$ in the strong condition and $p = .54$ in the weak condition.

⁷¹ $F(2, 226) = 0.5$, $p > .5$.

IV DISCUSSION

The data obtained in this study do not show evidence of contrast effects in decision making about the death penalty. None of the three different means of identifying contrast effects employed in this study revealed any significant evidence of such effects. Only the materials that involved a decision about years of imprisonment demonstrated contrast effects. Death, it seems, is different after all. Decisions about who deserves execution proved uniquely resilient to contrast effects. Subjects appeared to have resisted reliance on comparisons and made judgments that seemed more absolute. These results suggest that the difficulties in measurement and scaling that cause inconsistency in other contexts do not affect decisions in capital cases in the same ways.

The failure to find contrast effects in this study cannot be attributed to the methodology or to a lack of statistical power. We mimicked the methods that produced sizeable contrast effects in other contexts and had available a sizeable subject population in which to observe the phenomenon. With the 277 subjects in our study, any one of our three studies had an 85.5% chance of detecting an effect as small as the smallest effect in the study of contrast effects in legal judgments by Kelman and his coauthors, which was the closest study to ours in terms of methodology.⁷²

Why would juror assessments in capital cases resist contrast effects? Although our materials were designed to identify contrast effects, not to determine why they would fail to occur, we offer some speculations arising from the nature of contrast effects in other contexts. In other studies, contrast effects seem to occur in unfamiliar contexts. The undergraduate subjects in the studies by Kelman and his coauthors were complete novices in assessing appropriate settlement offers and verdicts in homicides.⁷³ Likewise, the subjects in the study by Sunstein and his coauthors were unfamiliar with setting puni-

⁷² The contrast condition in that study produced a fourteen percentage point shift with 158 subjects. See Kelman, Rottenstreich & Tversky, *supra* note 15, at 297 tbl.3. This is equivalent to a correlation coefficient of 0.18. See JACOB COHEN & PATRICIA COHEN, APPLIED MULTIPLE REGRESSION/CORRELATION ANALYSIS FOR THE BEHAVIORAL SCIENCES 39 (2d ed. 1983) (providing an equation for calculating correlation coefficients). If the population correlation coefficient in any of our studies were as large, we would have had an 85.5% chance of detecting a significant effect (at $p < .05$) with the 277 subjects in any one of our three studies. See *id.* at 59-61 (discussing the calculation of statistical power). Assuming that each study represented an independent chance to detect a significant effect, we had a 99.7% chance of detecting a significant effect in at least one study.

⁷³ See Kelman, Rottenstreich & Tversky, *supra* note 15, at 289 (describing the subject population).

tive damages award amounts.⁷⁴ Psychologists have argued that, in such contexts, people look for comparisons and tradeoffs that make sense to them; this is Hsee's "evaluability" hypothesis.⁷⁵ People may not know how many millions of dollars is an appropriate assessment of a personal injury, but they at least know it should exceed their assessment of a purely financial harm. Even in Hsee's study involving the pricing of ice cream, although the subjects were generally familiar with the price of ice cream, they did not have an exact sense of the appropriate price.⁷⁶ Not knowing exactly how much they valued the ice cream, subjects in the study knew only that an overfilled cup looks like a good deal and an underfilled cup looks like a rip-off. Lack of experience with the task leads people to rely on contrasts and make relative judgments, even though those judgments are intended to be absolutes.

Although few people have had personal experience with capital sentencing—and, in all likelihood, none of the subjects in our study had ever had personal experience—the task is nevertheless incredibly familiar. Serious crimes regularly generate local news stories and are the subjects of numerous television shows and movies. Violent crime is part of popular culture in the United States, and the availability of the death penalty is often a substantial part of stories involving violent crime. People make, and are exposed to, many judgments about what kinds of crimes result in the death penalty and may have well-formed opinions on the subject. In all likelihood, subjects in our study did not just compare the fact patterns we provided as contrasts, but also considered the wealth of contrasting violent crimes from popular culture. Our simple contrasts thus added only a single exemplar to that wealth of experience, and hence had little impact on subjects' judgments. In effect, a wealth of experience with issues of violent crime and criminal justice insulated subjects from contrast effects. As to the death penalty, the intended absolute judgments were, in fact, somewhat absolute, and were not made relative to the other cases we presented.

Other explanations for the lack of contrast effects are also possible. In the crime-type contrast, the subjects might not have seen the contrasting crime as sufficiently analogous. The target domestic murder was a violent crime of anger, whereas the contrasting domestic crime was one in which the murder was essentially the defensive response of a battered spouse. These crimes may not have been sufficiently analogous in the subjects' eyes to create a contrast, inasmuch

⁷⁴ See Sunstein, Kahneman & Schkade, *supra* note 32, at 2146 (describing the subject population).

⁷⁵ See Hsee, *supra* note 11, at 249–50.

⁷⁶ See Hsee, *supra* note 20, at 111–14.

as they arose from different motives. Similarly, the target murder-for-hire was essentially the work of a paid assassin, whereas the contrast murder was a crime arising from the misguided behavior of a mentally handicapped individual. Although both crimes conform to the legal definition of murder-for-hire, the contrast might have been lost on the subjects.

Alternative accounts for the lack of a contrast effect in the other two studies seem less plausible. In the crime-severity contrast study, it is possible that the target crime did not present a difficult decision for the subjects. Although we had intended that the case draw approximately a 50% rate of death sentences in the control condition, only about one-quarter of the subjects favored the death penalty. The fact pattern might simply have presented too weak a case for death. Kelman and his coauthors observed the weakest contrast effects in the case in which the control condition presented an option that was favored by three-quarters of the subjects.⁷⁷ Nevertheless, they still observed contrast effects in this condition.⁷⁸ Thus, although more extreme cases might reduce the contrast effect, they do not seem to eliminate it altogether. Our target fact pattern presented enough indecision that it should have been able to elicit a contrast effect. In this study, we believe that the best explanation for the absence of measurable contrast effects is that people are already so aware of other violent crimes that the contrasting crime presented in the study could not have shifted their core understanding of the kinds of conduct that merit a death sentence.

The contrasting co-conspirator study should likewise have been able to elicit a contrast effect. Arguably, in the condition that presented an alternative with a less severe sentence for the co-conspirator, so many subjects favored the less severe alternative that a contrast was not feasible. In the studies by Kelman and his coauthors, and in other previous work, the contrasting alternative was so inferior that few subjects selected it.⁷⁹ In our study, however, 40.8% of the subjects favored the contrasting alternative.⁸⁰ Nevertheless, the other 59.2% of the subjects felt that this option was inferior; thus, the data should still have demonstrated some contrast effect. Furthermore, the condition in which a more severe sentence was available for the co-conspirator mimicked the pattern found in the contrast studies by Kelman and his coauthors in that the vast majority of the subjects (94%) chose between the target alternatives rather than the contrast. And yet this condition still produced no contrast effects. In this study, it seems as

⁷⁷ See Kelman, Rottenstreich & Tversky, *supra* note 15, at 295-97.

⁷⁸ See *id.* at 297.

⁷⁹ *Id.* at 296-300.

⁸⁰ See *supra* Part III.B.

if the subjects separated out their preferred sentence for the co-conspirator and for the target defendant; this is exactly what they should do if they are making consistent, rational choices, and exactly what the previous choice literature suggests they will not do.

Finally, the present set of studies illustrates that contrast effects can impact judgments in criminal cases in general. The imprisonment contrast demonstrated exactly the kinds of contrast effects that have been observed in other contexts.⁸¹ Consistent with the theory that inexperience with the relevant subject matter is a necessary condition for contrast effects, subjects in this study likely had no meaningful experience with the decision they were asked to make. Like the subjects in previous studies by Sunstein and his coauthors, our subjects had no idea how to translate their punitive intent into a quantitative scale. Consequently, when they had two crimes to evaluate, they compared the two and made relative, rather than absolute, judgments. These results suggest that there is nothing special about crime in general that makes it immune to contrast effects, but rather that widespread understanding of violent capital crime makes judgments in capital cases more resilient to contrast effects than judgments in many other legal contexts.

This theory, as supported by the data in this study, suggests that judgments about the death penalty are somewhat stable and not subject to the kinds of arbitrariness psychologists have demonstrated in other contexts. People seem to have a relatively stable understanding of what conduct merits the death penalty, which is not easily manipulated. These results are consistent with the findings Theodore Eisenberg and his coauthors presented in this Symposium.⁸² Eisenberg and his coauthors worried that the victim's social status might play a role in creating arbitrary assignment of the death penalty.⁸³ Their study of actual jurors in capital cases, however, finds no evidence of this effect.⁸⁴ People seem to have fairly stable and consistent beliefs about who deserves the death penalty.

This is not to say that other factors do not create forms of undesirable arbitrariness in capital sentencing. The present study, however, tends to rule out the influence of an otherwise damning source of arbitrariness in the death penalty. Jurors may be biased, bigoted, confused, or misled. But this study suggests that they nevertheless can express a coherent, internally consistent set of underlying beliefs about who deserves the death penalty.

⁸¹ See *supra* Part III.A.

⁸² See Theodore Eisenberg, Stephen P. Garvey & Martin T. Wells, *Victim Characteristics and Victim Impact Evidence in South Carolina Capital Cases*, 88 CORNELL L. REV. 306 (2003).

⁸³ See *id.* at 321.

⁸⁴ See *id.* at 329–30.