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Fractional Factorial Designs for Legal Psychology

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Researchers considering novel or exploratory psycholegal research are often able to easily generate a sizable list of independent variables (IVs) that might influence a measure of interest. Where the research question is novel and the literature is not developed, however, choosing from among a long list of potential variables those worthy of empirical investigation often presents a formidable task. Many researchers may feel compelled by legal psychology's heavy reliance on full-factorial designs to narrow the IVs under investigation to two or three in order to avoid an expensive and unwieldy design involving numerous high-order interactions. This article suggests that fractional factorial designs provide a reasonable alternative to full-factorial designs in such circumstances because they allow the psycholegal researcher to examine the main effects of a large number of factors while disregarding high-order interactions. An introduction to the logic of fractional factorial designs is provided and several examples from the social sciences are presented. Copyright © 2002 John Wiley & Sons, Ltd.

One characteristic shared by both law and psychology is that in each discipline the answer to one's question usually begins with 'well . . ., it depends.' The world of human interactions is complicated. Moreover, the complexity of the networks of human relations in which we live is often reflected in the intricacies of the rules we develop to govern those relations. It is this complexity that makes legal psychology both interesting and demanding.

The answers to the fascinating questions of legal psychology necessarily depend upon numerous personal and contextual factors and the interactions among those factors. For any dependent variable of interest, it is often relatively easy for researchers to generate a long list of potentially relevant independent variables.

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The difficult task is not generating a list of potential independent variables; rather, it is deciding which of those variables are worthy of empirical testing. The first step in paring down the list is typically to consult the literature. However, in many instances the literature provides little direction either because the research questions being considered are novel, the variables being considered simply do not appear in the existing literature, or the content, context, or methods of the research under consideration differs fundamentally from existing studies.

The psycholegal researcher, then, is faced with the dilemma of whether to (i) arbitrarily choose certain independent variables to include in the research while omitting others, or (ii) attempt to include all of the potential independent variables. Too often, the final decision is to make arbitrary choices (or 'educated guesses') because investigating, in a single study, 10 or 15 independent variables is generally regarded as unworkable. Like its parent disciplines—social psychology, experimental psychology, cognitive psychology, etc.—the methodology of legal psychology is dominated by the full-factorial design (see West, Aiken, & Todd, 1993). Although full-factorial designs have the benefit of allowing for the examination of the independent contributions of factors with multiple levels, as well as their interactions, and often allow the researcher to draw causal conclusions, adding factors increases the complexity of the design exponentially. Designs with multiple factors at multiple levels quickly become unwieldy and impractical. Further, where there exists no coherent theory or prior research upon which to base predictions about main effects, there is almost no chance that there will exist coherent theory *a priori* to predict, or *post hoc* to explain, the higher-order interactions.

Eyewitness testimony provides a useful example because it is a well-researched area of legal psychology, where one *can* turn to the literature and to prior empirical research to learn about the independent effects of a large number of different IVs. Past research has demonstrated that eyewitness testimony is influenced by many contextual factors such as the presence of a weapon, the presence of violence, exposure to mugshots, lineup size, retention interval, presence of a disguise, similarity of members of the lineup, and lineup instructions (Cutler, Penrod, & Stuve, 1988). However, an investigation of all these factors and their interactions in a complete 2^8 full-factorial design would require 256 conditions and, using a rule of thumb of at least 15 subjects per cell, over 3,800 participants. The logistical difficulties inherent in organizing such a large number of experimental conditions and the large sample size required are major impediments to implementing such a design. Moreover, the financial costs can be prohibitive. Researchers wanting to increase external validity by abandoning student subject pools and collecting data from a sample of community members typically must pay participants a stipend of anywhere from $10 to $100 to entice their participation. At that rate, simply paying participants could cost between $38,000 and $380,000, perhaps raising a red flag before the eyes of most funding agencies. Further, a researcher proposing such a study will likely be unable, even in an area of legal psychology as well researched as eyewitness testimony, to provide a coherent theory of the likely fourth-, fifth-, and higher-order interactions that are, structurally, a significant part of the research design.

In addition, a complete factorial design this large has the potential of creating unlikely combinations among the high-order interactions (Rousseau & Aquino, 1993). Turning to a new example, in a study of perceptions of employment-contract obligations Rousseau and Aquino investigated the effects of six independent factors each at three levels—time on the job, formal commitment, reasons for termination, severance package, level of participation, and type of notice. Rousseau and Aquino noted that a complete 3^6 factorial design would create combinations unlikely to occur in a natural setting, such as 'high-seniority coupled with the absence of guarantees of long-term employment' (p. 141). And again, even if all the conditions produced by a large factorial design were externally valid, theory is unlikely to predict, *a priori*, effects more complex than first- or second-order interactions (West *et al.*, 1993).

The tradeoff is relatively clear, use of a full-factorial design allows for control and causal inference; however, the number of independent variables is limited by concerns of design complexity. Complete crossing of all levels of all factors has the potential to create combinations unlikely in natural settings and high-order interactions can seldom be predicted or explained by theory, making interpretation difficult at best.

One obvious but particularly inefficient alternative is simply to conduct a series of small full-factorial experiments. This approach may actually take much more time and effort than a single large experiment because procedures such as subject recruitment would have to be repeated multiple times. Likewise, a series of smaller studies ultimately would require as many participants and as much if not more expense than the full factorial. In addition, a series of smaller factorial designs would not allow for the exploration of two- or three-way interactions among factors appearing in separate studies and would result in less control (across the studies) than a single large experiment. An alternative to either a single large experiment or a series of smaller experiments does exist, and that alternative (the fractional factorial design) allows for the exploration of the main effects and selected interactions among a large number of factors.

INTRODUCTION TO FRACTIONAL FACTORIAL DESIGNS

Fractional factorial designs ('FFDs') provide one method for exploring the independent main effects and selected interactions of a large number of factors without the unwieldy size and complexity of a full-factorial design. Fractional factorial designs are not new, and have been in common usage among researchers in the physical sciences since the 1940s (see, e.g., Finney, 1945; Plackett, 1946). Psychology (including social psychology and legal psychology), however, has been slow to adopt the fractional factorial approach even though, over a decade ago, Kenny (1985) advocated the increased use of FFDs in social psychology as a method for overcoming many of the limitations of traditional full-factorial designs. Within the confines of legal psychology, our review of articles published in *Behavioral Sciences and the Law* and in *Law and Human Behavior* during the decade of the 1990s revealed only three studies that utilized fractional factorial designs (Cutler, Penrod, & Dexter, 1990; Slovic & Monahan, 1995; Smith, Penrod, Otto, & Park, 1996).

The logic of the FFD is quite simple. Rather than running a complete factorial design, the researcher runs only a systematically selected portion of the possible factor level combinations (Winer, 1971). The factor level combinations are carefully chosen to answer the questions of most interest to the researcher (McLean & Anderson, 1984; West *et al.*, 1993; Winer, 1971). Typically, this involves choosing combinations that will allow the researcher to assess the main effects, first-order interactions, and sometimes second-order interactions of the variables of interest, while confounding higher-order interactions (McLean & Anderson, 1984).

Analyzing Factorial Designs with Multiple Regression

Before describing FFDs in detail, it is helpful to briefly review multiple regression data analysis techniques with particular attention to coding for interaction effects. In analyzing a factorial design, analysis of variance (ANOVA) and multiple regression are mathematically equivalent statistical techniques. However, it is difficult in most modern statistics packages to isolate interactions of interest using ANOVA or MANOVA due to constraints on the extent to which the multivariate model may be customized. In contrast, multiple regression techniques using effects coding allow the flexibility to build codes that represent interactions of interest, and only the interactions of interest. As such, multiple regression techniques are preferable to ANOVA and MANOVA, which are typically associated with full-factorial designs.

In coding variables for use in a regression model, each manipulated variable is assigned a condition-specific effect code. Thus, for two levels of a variable (e.g., present and absent) a '+1' can be assigned when the factor level is present in the treatment combination and a '−1' when the factor level is absent in the treatment combination. The codes for each variable may then be used as predictors in a multiple regression model to examine the main effects (unique contributions) of the variables on the dependent measure of interest.

Codes that represent interaction effects in multiple regression are created by multiplying across the effect codes for each of the variables in the interaction. For example, the effect code for the interaction between two variables (e.g., A and B) is simply the product of the codes for those two variables. For a participant in a cell where both A and B are present (+1 for each), the AB interaction is coded as '+1', whereas the interaction is coded as '−1' for a cell where A is present (+1) and B is absent (−1). Three-way and higher-order interactions are calculated in a similar manner by multiplying across all relevant variable codes to create the code for the desired interaction.

The codes for a 2^3 factorial design are presented in Table 1. In this sign table, there are three variables (A, B, and C) each manipulated at two levels (+1 and −1). Each of the eight experimental conditions consists of a unique combination of the three variables. Multiplying the codes for the variables involved in each interaction

Table 1. Sign table for 2^3 factorial

	A	B	C	AB	AC	BC	ABC
1	+1	+1	+1	+1	+1	+1	+1
2	+1	+1	−1	+1	−1	−1	−1
3	+1	−1	+1	−1	+1	−1	−1
4	+1	−1	−1	−1	−1	+1	+1
5	−1	+1	+1	−1	−1	+1	−1
6	−1	+1	−1	−1	+1	−1	+1
7	−1	−1	+1	+1	−1	−1	+1
8	−1	−1	−1	+1	+1	+1	−1

creates the interaction terms. It becomes clear from the table that the effects of each variable and each interaction will make a unique contribution to the model because each column of codes is unique.

Confounding and FFDs

We now turn to an explanation of FFDs. FFDs are, at their essence, a method of selectively confounding conditions within a full-factorial design. Consider, for example, an experimenter interested in running a 2^4 factorial design with two levels each of factors A, B, C, and D. Imagine that the researcher is constrained to using only 64 participants. Evenly distributing the participants among the 16 conditions created by the full 2^4 factorial design will yield only four participants per condition, a situation with potentially inadequate statistical power. If the researcher is willing to assume that no significant or interesting variability will be attributable to the ABC three-way interaction, the researcher may confound the manipulation of variable D with the codes for this three-way interaction. In this way, the researcher can cut the size of the overall experimental design in half by adding variable D to a 2^3 factorial design, creating a 2^{3+1} design. Using a sign table as presented in Table 2,[1] a researcher can determine which effects are confounded in the fractional design. Variables with identical patterns of codes are confounded.[2]

The 2^{3+1} design requires one-half as many conditions (eight) as does the original 2^4 design (16). By employing an FFD, the researcher has doubled the power for detecting main effects: 64 participants will yield eight participants per condition in the 2^{3+1} FFD versus only four per condition in the full 2^4 factorial. There is a tradeoff for the efficiency of the FFD design, however. Because variable D has been confounded with the ABC interaction, the researcher has sacrificed interpretability of all two-way interactions and based the interpretation of one main effect (D) on the assumption that effect of the three-way interaction is negligible.

To see why this is true, note that the pattern of codes for the main effect of D matches the pattern for the ABC interaction, indicating that they are confounded.

Table 2. Sign table for 2^{3+1} factorial

	A	B	C	AB[a]	AC[b]	BC[c]	ABC[d]	D[d]	AD[c]	BD[b]	CD[a]
1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1	+1
2	+1	+1	−1	+1	−1	−1	−1	−1	−1	−1	+1
3	+1	−1	+1	−1	+1	−1	−1	−1	−1	+1	−1
4	+1	−1	−1	−1	−1	+1	+1	+1	+1	−1	−1
5	−1	+1	+1	−1	−1	+1	−1	−1	+1	−1	−1
6	−1	+1	−1	−1	+1	−1	+1	+1	−1	+1	−1
7	−1	−1	+1	+1	−1	−1	+1	+1	−1	−1	+1
8	−1	−1	−1	+1	+1	+1	−1	−1	+1	+1	+1

Note: Columns with matching superscripts are confounded.

[1]The same results can be reached using modular mathematics. For a review of modular mathematics see Winer (1971).
[2]The confounding employed in fractional designs can be conceived of as similar to that utilized in Latin square designs (Lentner & Bishop, 1986, p. 395).

This effect could be referred to as either an effect of D or an effect of ABC, thus D and ABC are said to be 'aliases' (Davies & Hay, 1950). The same is true of AB and CD, of AC and BD, and of BC and AD. Due to this confounding, it is impossible to examine the effects of the two-way interactions in this design. If D had not been confounded with the codes for the ABC interaction, no aliases would exist. In such a case, the patterns for each main effect and each interaction would be unique. Thus, using a sign table, a researcher can determine which effects are confounded in the fractional design. If the researcher has this information and knows which effects are of interest, the researcher can determine whether or not the design will yield the desired information.

If a researcher were willing to assume that all interactions in this study were negligible, the researcher could explore all main effects more efficiently than with a separate experiment for each effect. That is, if there is an effect of aliases D and ABC and the researcher is willing to assume that the effect of ABC is negligible, then the effect can be treated as an effect of D. Thus, it could be examined in addition to the effects of A, B, and C, which are not confounded. However, finding a researcher willing to assume that all interactions in a 2^3 factorial are negligible is unlikely. This being the case, the 2^{3+1} factorial serves well to demonstrate the logic of the fractional factorial approach, but is ultimately not a very good design.[3]

It is much more likely that a researcher interested in conducting a 2^6 design might be willing to assume that three-way interactions are of little interest and that four-way, five-way, and six-way interaction effects are negligible. Applying the same approach demonstrated for the 2^4 design above, a researcher could confound main effects with five-way and higher interactions, and confound two-way interactions with four-way and higher interactions. Some three-way interactions would, however, have to be confounded with other three-way interactions. Thus, the researcher would only be able to assess the main effects and the two-factor interactions with confidence. Information on three-way and higher interactions would be sacrificed.

It might be asked whether it is reasonable to assume that the higher-order interactions are negligible. An examination of the literature suggests that such an assumption is often reasonable. Higher-order interactions are rarely predicted, are even more rarely found to exist, and are exceedingly difficult to interpret when they are found to exist. Our review of articles published in *Law and Human Behavior* and *Behavioral Sciences and the Law* during the decade of the 1990s revealed at least 83 studies (in 72 articles) that permitted examination of three-way and higher interactions. Of these, only 24 studies found significant higher-order interactions.[4] Moreover, when significant higher-order interactions were found to exist, they were rarely discussed in detail and were often unhypothesized and/or uninterpretable (see, e.g., Douglas & Ogloff, 1996; Goodman *et al.*, 1998). Given the relative

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[3]The flaw of the design is its low resolution. The 'resolution' of a fractional design provides an indication of the highest level of interaction that is not confounded with other interactions of the same order (John, 1962). For example, a design with a resolution of III confounds main effects with two-factor interactions (John, 1962). In a higher-resolution design, such as a resolution V design, main effects and two-factor interactions are not confounded with one another, but are only confounded with higher-order interactions (John, 1962). Thus, a design of resolution V is usually more desirable than a design of resolution III.

[4]These comparisons actually underestimate the relative frequency with which higher-order interactions are found given how often they are examined. In each of the studies in which higher-order interactions were examined, multiple analyses were conducted which looked for such interactions. Thus, a single study might have involved several analyses that could have revealed numerous higher-order interactions.

infrequency with which higher-order interactions are found to exist, it may often be reasonable to assume that such effects are negligible.

More Complex Fractional Designs

As the number of factors in an experiment increases, confounding one main effect with a high-order interaction may be insufficient to yield a manageable design (McLean & Anderson, 1984). For example, a full-factorial design manipulating nine two-level factors in a 2^9 design requires 512 experimental conditions. Confounding one main effect in a 2^{8+1} design cuts the number of cells in half, but still the design requires 256 experimental conditions. In such a large design, it may become desirable to run a smaller fraction of the total conditions. Confounding two of the nine variables in a 2^{7+2} design would allow for clear analysis of all main effects and two-way interactions using only 128 experimental conditions. This model allows substantial statistical power for examining main effects and two-way interaction effects. Because only main effects and two-factor interactions are to be measured, a smaller number of participants can be assigned to each condition. Thus, even if only two participants were assigned to each condition, analyses of main effects would have 128 participants per condition and simple effects analyses for two-way interactions would have 64 participants per condition.[5]

How might such a design look? And how might one go about creating such a design? Again, the sign table provides the simplest method of mapping out the fractional design. A standard 2^9 factorial would involve factors A–I. A 2^{7+2} fractional factorial uses codes identical to a 2^7 factorial, which would involve factors A–G. A researcher can simply map out the 2^7 sign table (see Appendix A), laying factors H and I over two different five-way interactions.[6]

Within-Subject Fractional Designs

Adding within-subject design advantages to the already efficient FFD is yet another option available to researchers. When within-subject manipulations are theoretically sound, researchers may simply counterbalance conditions within subjects in a fractional factorial design, just as researchers might do with a traditional factorial design.

Within-subject designs are more powerful for detecting effects, not only because of the increased number of observations, but also because individual differences (between-subjects variance) can be statistically controlled. Specifically, within-subject power and precision in multiple regression analysis can be improved by employing a method described by Cohen and Cohen (1983) for removing between-subject variance from the error term. Because the variance explained by the manipulated factors is exclusively within-subjects, between-subjects variance only contributes

[5]As with the development of all factorial designs, statistical power in fractional factorial designs is an important issue worthy of substantial consideration during the design development phase. For a complete discussion of statistical power see Cohen (1998) and Kraemer and Thiemann (1987).
[6]This design is preferable to a design in which H and I are confounded with higher-order interactions. If higher-order interactions are used as confounds for H and I, then the interaction between H and I would be confounded with main effects or with other two-way interactions.

unecessary noise to the error term. By following several steps outlined by Cohen and Cohen (1983), between-subjects variance may be extracted from the error term.

RETURNING TO THE EXAMPLES

Although FFDs are used in the social sciences much less frequently than traditional factorial designs, examples of their use can be found (see, e.g., Cutler *et al.*, 1990, 1988; Haider & Ewing, 1990; Kline & Wagner, 1994; Robben & Verhallen, 1994; Rousseau & Aquino, 1993; Slovic & Monahan, 1995; Smith *et al.*, 1996; Stolle, 1998; Tziner, 1988). Recall the example of eyewitness testimony. In an interesting and creative use of FFDs, Cutler and colleagues (1988) identified eight factors from the empirical literature that had been shown to influence the accuracy of eyewitness testimony. To that list they added two additional factors (presence of voice samples and witness confidence), both of which are plausible predictors of eyewitness accuracy but had not been supported in previous research (Cutler *et al.*, 1988). Rather than run a complete 2^{10} factorial design, they ran a 2^{6+4} fractional factorial design.[7] Six of the factors were fully crossed and the remaining four factors were confounded with high-order interactions. This design allowed for the measurement of all main effects and most first-order interactions, assuming that high-order interactions were negligible.

Recall also the example of perceptions of contract obligations in the employment context. Rousseau and Aquino (1993) identified six factors from the empirical literature that might influence the perceived establishment of a contractual obligation. Each factor had three levels. Rather than conducting a complete between subjects 3^6 factorial design, which would have required 729 conditions and perhaps over 10,935 participants, Rousseau and Aquino adopted a fractional factorial design originally presented by Connor and Zelen (1959).[8] Using the plan of Connor and Zelen, Rousseau and Aquino developed 27 stimulus patterns to represent the independent effects of six factors at three levels each. Each of 121 research participants responded to each of the 27 stimulus patterns, and the order in which the factors were presented was randomized to reduce order effects (Rousseau & Aquino, 1993). This design allowed Rousseau and Aquino to efficiently and reliably assess the effects of six independent factors with the relatively small sample size of 121.

An Illustrative Example from Recent Research

It is relatively easy to identify existing psycholegal research that could be conducted efficiently using FFDs. For example, we have selected three studies (from two articles) published in recent volumes of *Law and Human Behavior* to demonstrate how several smaller factorial designs could be efficiently combined into a single

[7]Note that the 2^{10-4} design can also be conceived of as a 2^{6+4} design, without changing the nature of the design. Smith *et al.* (1996) used a similar design in a more recent study of jurors' use of probabilistic evidence.
[8]Connor and Zelen (1959) also appears as an appendix in McLean and Anderson (1984).

fractional factorial design (Goodman *et al.*, 1998; Schmidt & Brigham, 1996).[9] Each of these studies focuses on juror perceptions of child eyewitnesses.

Goodman *et al.* (1998) examined the effects of closed-circuit television (CCTV), witness age, and defendant guilt on child witnesses and mock-jurors. Their study comprised a 2 (age—which we will term variable A) × 2 (testimony in open court or via CCTV—variable B) × 2 (defendant guilty or innocent—C) factorial design. Schmidt and Brigham (1996) also examined mock-jurors' perceptions of child witnesses. In their first study, Schmidt and Brigham examined the effects of witness age, witness communication style, and attorney questioning style on deliberating jurors in a 3 (age—A) × 2 (powerful or powerless communication style—D) × 2 (leading or non-leading questioning style—E) factorial design. In their second study, Schmidt and Brigham examined the effects of these same factors on non-deliberating jurors (a possible factor F).

These three studies could be combined into a single 2^{5+1} fractional factorial design (see sign table in Appendix B). The six independent variables would be the age of the child witness (A), the presentation mode of the testimony (open court or via CCTV—B), whether the defendant is guilty or innocent (C), powerful or powerless communication style (D), the style of the prosecuting attorney's questions (leading or non-leading—E), and whether or not the mock-jurors deliberate (F).[10] In this fractional design one main effect could be confounded with the five-way interaction (i.e., F could be confounded with the ABCDE interaction). In such a design, two-way interactions are primarily confounded with four-way interactions (and some three-ways) and information about the five-way interaction is lost. If the researcher is willing to assume that the three-way and higher interactions are negligible, then this design allows for examination of all main effects and two-way interactions.

Such a design can be conducted with fewer participants than were the separate studies. The Goodman *et al.* (1998) study was conducted with 1,201 mock-jurors comprising 88 twelve-person juries (i.e., approximately 11 juries per experimental condition). Main effects in their study were based on 44 observations per group. The Schmidt and Brigham (1996) studies were conducted with 480 participants comprising 120 four-person juries (i.e., approximately ten juries per experimental condition—and 30 to 40 observations per group in tests of main effects) and 207 non-deliberating participants (i.e., approximately 17 participants per experimental condition and approximately 70 to 103 observations per group in tests of main effects), respectively. The total number of subjects in the three studies was 1,888.

If, in our FFD design, we sought to achieve roughly the same levels of statistical power, we might seek 64 observations per group for tests of main effects. The fractional design consists of 32 conditions. Even if as few as four participants or

[9]We selected these studies because they address a psycholegal topic of broad interest and because they represent related studies by different researchers that examine numerous variables of interest. Our selection of these studies should not be construed as a criticism of the studies.

[10]We have simplified for purposes of illustration. One difficulty that arises in combining these studies is that Goodman *et al.* used actual children who experienced an event in which the defendant was either guilty or innocent, while Schmidt and Brigham used child actresses so that they could manipulate the child witnesses' communication style. We believe it is possible to combine these variables into a single study, perhaps by pre-testing child participants on one or more measures of communication style. However, we recognize that no set of existing studies will be a perfect illustration due to differences in methodologies and goals. The choices made by investigators in the selection of variables and methods are often driven by concern for internal or external validity, difficulty accessing participants, etc. These considerations must also be taken into account when designing complex fractional factorial experiments.

juries are assigned to each condition, then the main effects and two-factor interactions would be based on 64 individuals or juries per condition and the simple effects analyses for any two-way interactions would be based on 32 individuals or juries per condition. If 12-person juries were used, this design would require 64 non-deliberating jurors (four jurors per cell in the 16 non-deliberating cells of the design) and 768 jury members (four observations $\times$ 12 jury-members $\times$ 16 deliberating cells), a total sample size of only 832—fewer than one-half of the number employed in the three separate studies. If smaller juries were used (such as the four-person juries in the Schmidt and Brigham (1996) study), only 320 participants would be needed.

Although this design has clear advantages with respect to efficient use of research participants, what is especially noteworthy is that the design permits testing and exploration of at least one main effect (F), a number of two-way interactions (F with all other effects and cross-study interactions involving B and C with D and E) and some three-way interactions across studies that were impossible to examine in the original sequence of three studies. Of course, if we observe significant two-way (or three-way) interactions (that were impossible to observe in the three-study design) we may be cautious about our interpretations because the two- or three-way interactions are confounded with other high-order interactions. In the event we observe interactions of theoretical or practical significance, we may be motivated to further explore those effects in an unconfounded design. We will no doubt lose interest in interactions that fail to produce significant effects. In a sense, by choosing the FFD we have traded off clarity of interpretation for the opportunity to explore a wider array of main effects and interactions than is possible in the three-study approach.

THE POTENTIAL UTILITY OF FRACTIONAL FACTORIALS IN FUTURE PSYCHOLEGAL RESEARCH

In legal psychology, it is often relatively easy to generate a long list of potential independent variables that may influence a measure of interest. In novel areas of research, however, existing theory and literature often will provide little guidance regarding which of the potential IVs are likely to produce main effects and virtually no guidance with regard to predicting or explaining high-order interactions. As such, designs with many factors at two levels (high and low) that focus on main effects and disregard high-order interactions are particularly useful for novel or exploratory psycholegal research. A researcher may investigate two levels of many factors and discard those factors that show no differences between levels. The researcher may then conduct follow-up experiments using more than two levels, representing fine-grained variations on the stimulus, for only those factors that have not been discarded based on the results of the prior experiment. Such designs are most efficiently conducted as FFDs. We have provided a handful of examples in this article of areas of legal psychology in which FFDs have been, or could be, used successfully. We believe that there are many more areas of legal psychology that could benefit from the use of FFDs, including research on the impact of expert testimony, the comprehensibility of jury instructions, the impact of pretrial publicity, the impact of cross-examination techniques, factors impacting juror damage awards, juror or community perceptions of mental illness, etc. Indeed, any area of legal psychology involving numerous potential independent variables is a candidate for the use of FFDs.

In this article, only designs in which each factor can be represented at two levels have been discussed. However, the logic applied to designs with factors at two levels can also be applied to designs with factors at three or more levels (Lentner & Bishop, 1986; McLean & Anderson, 1984; Winer, 1971). Larger, more complex designs can be tedious to map out. However, it is not difficult to find preexisting templates of common factorial designs, such as those provided by McLean and Anderson (1984). Some statistical software packages, such as *Macanova*, can also help reduce the time and effort involved in designing complex fractional factorial experiments.

Fractional factorial designs do, of course, result in some limitations on the interpretations of the data. However, these limitations are often a small price to pay for the ability to efficiently investigate the independent effects of a large number of factors on a dependent measure of interest. In using FFDs, the researcher must be very careful to ensure that all effects of interest can be interpreted. Overall, in circumstances in which it makes sense to investigate the independent effects of a large number of factors but high-order interactions are unlikely or cannot be predicted based upon sound theory, FFDs are an efficient and effective design for psycholegal research.

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APPENDIX A

The following is an abbreviated sign table for a 2^{7+2} design showing the codes for the main effects only. Factors H and I are confounded with two different five-way interactions.

	A	B	C	D	E	F	G	...	H [ABCDE]	I [CDEFG]
1	+1	+1	+1	+1	+1	+1	+1		+1	+1
2	+1	+1	+1	+1	+1	+1	−1		+1	+1
3	+1	+1	+1	+1	+1	−1	+1		+1	−1
4	+1	+1	+1	+1	+1	−1	−1		+1	+1
5	+1	+1	+1	+1	−1	+1	+1		−1	−1
6	+1	+1	+1	+1	−1	+1	−1		−1	+1
7	+1	+1	+1	+1	−1	−1	+1		−1	+1
8	+1	+1	+1	+1	−1	−1	−1		−1	−1
9	+1	+1	+1	−1	+1	+1	+1		−1	−1
10	+1	+1	+1	−1	+1	+1	−1		−1	+1
11	+1	+1	+1	−1	+1	−1	+1		−1	+1
12	+1	+1	+1	−1	+1	−1	−1		−1	−1
13	+1	+1	+1	−1	−1	+1	+1		+1	+1
14	+1	+1	+1	−1	−1	+1	−1		+1	−1
15	+1	+1	+1	−1	−1	−1	+1		+1	−1
16	+1	+1	+1	−1	−1	−1	−1		+1	+1
17	+1	+1	−1	+1	+1	+1	+1		−1	−1
...										
32	+1	+1	−1	−1	−1	−1	−1		−1	−1
33	+1	−1	+1	+1	+1	+1	+1		−1	+1
...										
64	+1	−1	−1	−1	−1	−1	−1		+1	−1
65	−1	+1	+1	+1	+1	+1	+1		−1	+1
...										
128	−1	−1	−1	−1	−1	−1	−1		−1	−1

APPENDIX B

The following is an abbreviated sign table for a 2^{5+1} design showing the codes for the main effects only. Factor F is confounded with the five-way interaction.

	A Age	B Mode of testimony	C Guilt	D Communication style	E Method of questioning	. . .	F Deliberation [ABCDE]
1	+1	+1	+1	+1	+1		+1
2	+1	+1	+1	+1	−1		−1
3	+1	+1	+1	−1	+1		−1
4	+1	+1	+1	−1	−1		+1
5	+1	+1	−1	+1	+1		−1
6	+1	+1	−1	+1	−1		+1
7	+1	+1	−1	−1	+1		+1
8	+1	+1	−1	−1	−1		−1
9	+1	−1	+1	+1	+1		−1
10	+1	−1	+1	+1	−1		+1
11	+1	−1	+1	−1	+1		+1
12	+1	−1	+1	−1	−1		−1
13	+1	−1	−1	+1	+1		+1
14	+1	−1	−1	+1	−1		−1
15	+1	−1	−1	−1	+1		−1
16	+1	−1	−1	−1	−1		+1
17	−1	+1	+1	+1	+1		−1
. . .							
32	−1	−1	−1	−1	−1		−1

