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1/*f* Noise and Effort on Implicit Measures of Bias

Joshua Correll
University of Chicago

Phenomena that vary over time can often be represented as a complex waveform. Fourier analysis decomposes this complex wave into a set of sinusoidal component waves. In some phenomena, the amplitude of these waves varies in inverse relation to frequency. This pattern has been called 1/*f* noise and, unlike white noise, it reflects nonrandom variation. Latencies in simple computer tasks typically reveal 1/*f* noise, but the magnitude of the noise decreases as tasks become more challenging. The current work hypothesizes a correspondence between 1/*f* noise and effort, leading to the prediction that increasing effort will reduce 1/*f* noise. In 2 studies, the author examined the relationship between an individual's attempts to avoid bias (measured in Study 1, manipulated in Study 2) and 1/*f* noise in implicit measures of stereotyping and prejudice. In each study, participants who made an effort to modulate the use of racial information showed less 1/*f* noise than did participants who made less effort. The potential value of this analytic approach to social psychology is discussed.

Keywords: 1/*f* noise, effort, stereotyping, prejudice

Recent decades have witnessed dramatic changes in the study of stereotyping and prejudice. As egalitarianism gained prominence in the United States (Devine & Elliot, 1995), psychologists responded by developing ever more delicate measures to assess bias. Initially, researchers simply replaced direct questions about intergroup attitudes with more subtly phrased, indirect questions about racially or ethnically sensitive issues (e.g., McConahay, Hardee, & Batts, 1981), but, more recently, computer-based measures have proliferated. These computer tasks are usually designed to be difficult or impossible to strategically control. For example, an evaluative priming task, developed by Fazio and his colleagues (Fazio, Jackson, Dunton, & Williams, 1995), primes participants with either a Black face or a White face before asking them to classify a target word as good or bad. Participants typically classify positively valenced words (e.g., *flower*, *rainbow*) more quickly after seeing a White face than after a Black face, but they classify negative words (e.g., *cockroach*, *death*) more quickly after seeing a Black face than after a White face. Other prominent measures include the Implicit Association Test (IAT; Greenwald, McGhee, & Schwartz, 1998), the Lexical Decision Task (Wittenbrink, Judd, & Park, 1997), and the Go/No-Go Association Task (Nosek & Banaji, 2001).

A common feature of many computer tasks is that they require numerous responses. A typical paradigm involves something on the order of 100 trials, each requiring a separate reaction. An implementation of Fazio et al.'s (1995) task might pair each of 5 Black and 5 White faces with 10 positive target words and 10 negative target words, yielding 200 trials over the entire study. The rationale for such high numbers is simple. A participant's responses on any single trial are subject to several influences. One influence should, at least in theory, be the participant's general evaluation of Whites and Blacks. To the extent that the participant reacts more negatively to Blacks than to Whites, a Black prime should facilitate responses to negative words and a White prime should facilitate reactions to positive words. But on any given trial, the effect of the psychological response in which the experimenter is interested is presumably quite small. It may be overwhelmed by other, less theoretically interesting sources of variance. The participant may blink at the moment the prime appears, or the particular face or target word presented may conjure up idiosyncratic associations (e.g., "She looks like my roommate"). If the task includes a sufficient number of trials and a sufficient variety of stimuli, these extraneous influences should cancel each other out and produce only a negligible effect. Averaging across all instances of a given trial type (e.g., all trials on which a Black face precedes a positive word) minimizes the influence of unintended variables, revealing the one factor that systematically influences responses on every trial: the participant's attitudes toward Whites and Blacks.

Aggregation, however, sacrifices a great deal of information about trial-by-trial variation in response times. Researchers generally assume that residual variability in the latencies (after accounting for the particular condition on each trial) is random and meaningless. Given that assumption, the sacrifice seems acceptable because a variable that fluctuates randomly cannot convey information. But what if residuals vary in a nonrandom fashion? Just as systematic differences in the average response times to Black and White faces can inform researchers about implicit bias, systematic differences in patterns of variability in those responses may hold important clues about the psychological states of the participants (e.g., the diffusion model; Ratcliff, Van Zandt, &

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Correspondence concerning this article should be addressed to Joshua Correll, Department of Psychology, University of Chicago, 5848 South University Avenue, Green 415, Chicago, IL 60637. E-mail: jcorrell@uchicago.edu

McKoon, 1999). In line with this possibility, in the current article, I examine measures of implicit racial bias to test the possibilities that (a) trial-to-trial fluctuations often vary in a nonrandom fashion and (b) the pattern of variability depends on participants' task-related effort. Although this research focuses on racial bias, the essential point of this article applies to other areas of research. In many areas of social psychology, variance that is treated as meaningless may contain valuable information. If so, analytic strategies like those presented here may provide social psychologists with additional techniques to investigate the phenomena that interest them.

It is important to note that this article does not dispute the value of aggregation as a means of isolating the influence of attitudes or associations on implicit tasks. The thrust of the argument is simply that researchers can glean additional, potentially valuable information by analyzing "error" variance rather than throwing it away. What makes this approach important for researchers in the field of social psychology is that it can be applied to the kinds of data that are already being collected in laboratories around the world. It can even be applied retroactively to existing data sets. With no additional equipment and no new measures, these data can be reharvested, allowing researchers to potentially gain new insight into human behavior. The strategy used here is based on work investigating 1/*f* noise.

1/*f* Noise and Response Times

1/*f* noise refers to nonrandom variation over space or time. 1/*f* noise is intriguing in part because it seems to characterize a wide range of phenomena, from variation in the magnitude of earthquakes and the intensity of pulsar emissions to fluctuations in the auditory frequency of music. Each of these phenomena can be represented as a complex waveform (e.g., the notes weaving up and down as one reads a sheet of music). In similar fashion, in the present work, I examine the fluctuations in response latency that occur from one trial to the next as a participant performs a computer task (see the top panel of Figure 1). This work draws on research by Gilden (2001) and others, which tests for 1/*f* noise in a trial series—treating the trial-by-trial variations in residualized latency data as a wave.

1/*f* noise is examined by applying a fast Fourier transform (FFT) to the complicated wave of latency data, decomposing it into several pure sinusoidal waves (see the middle panel of Figure 1). Adding these component waves back together reproduces the original wave. Component waves can vary in terms of their frequency and power (a function of the wave's amplitude), and one can plot the power of each component wave against its frequency—after log transforming both variables—to examine the relationship between them. This scatter plot is called a power spectral density (PSD) chart (see the bottom panel of Figure 1).

If trial-by-trial variations are random (i.e., if the residual on trial X_n is independent of the residual on trial X_{n+k}), the PSD should show white noise: The power and frequency of the component waves should not covary in any systematic fashion (after all, the wave is random). On average, a random trial series therefore yields a PSD in which the slope between power and frequency does not differ from zero. 1/*f* phenomena, by contrast, produce a PSD with a negative slope: Lower frequency waves have more power than do higher frequency waves. That is, the power of a component

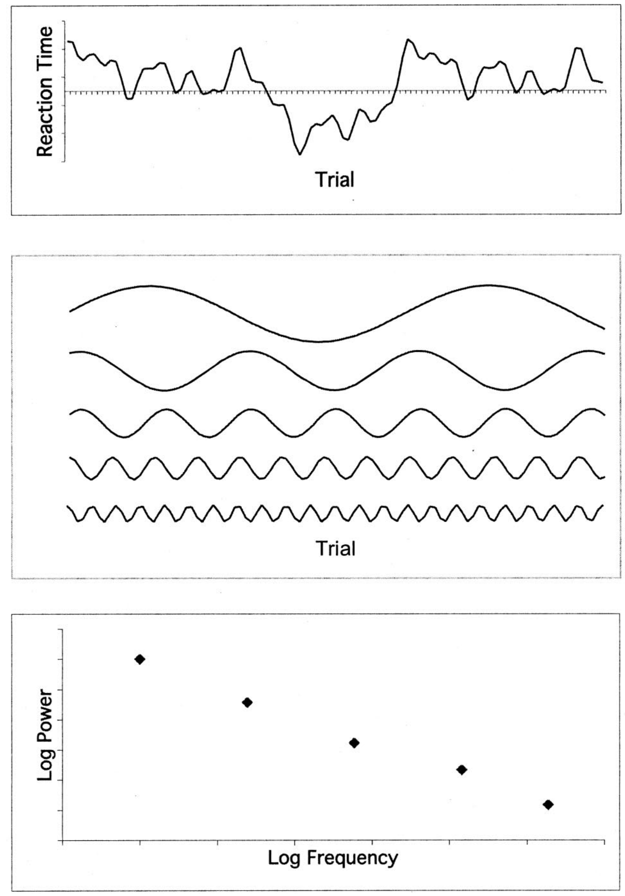

Figure 1. Simulated reaction time data (top panel), Fourier decomposition of reaction time data into component waves (middle panel), and power spectral density graph showing a scatterplot of the power and frequency of each component wave (bottom panel).

wave is proportional to the inverse of its frequency, or power ∝ (1/frequency). This relationship is the origin of the term 1/*f* noise. The pattern is also called $1/f^{\alpha}$ noise, where α designates the slope of the power–frequency relationship, or *pink noise* because the PSD of the color pink similarly reveals that lower frequencies have greater power.

Research in cognitive psychology has demonstrated that individuals performing simple tasks (e.g., determining whether a target is present vs. absent, pressing a button at regular intervals) show 1/*f* noise in their responses. That is, after the effects of any independent variables under investigation are partialed out, a PSD of the residual latencies reveals the characteristic negative slope of 1/*f* noise. This pattern is strongest or most negative at lower frequencies and tends to flatten as frequency increases. Gilden (2001; Gilden, Thornton, & Mallon, 1995; Thornton & Gilden, 2005) has proposed a dual-component model of 1/*f* noise in cognition. According to this model, 1/*f* variation (which may reflect cognitive functioning) is masked at high frequencies because of truly random variation or white noise from other sources. Gilden attributed this high-frequency noise to muscle activity. Figure 2 (left panel) shows a fairly typical PSD for a participant performing a response-latency

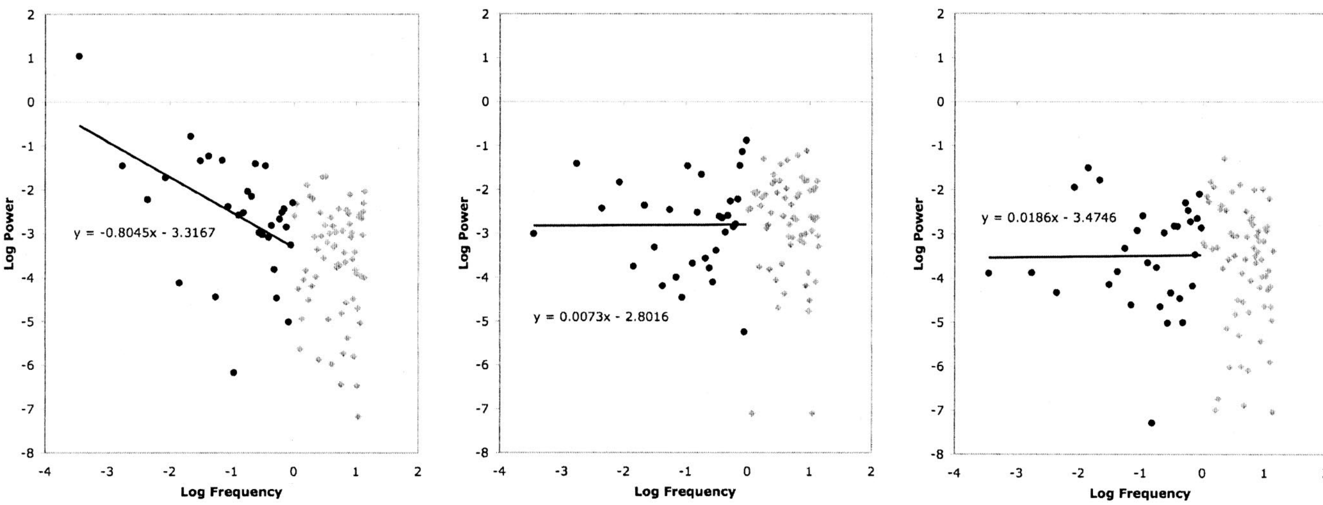

Figure 2. Power spectral density (PSD) graphs for latency data for Participant A, who reported minimal effort to avoid bias (left panel); for Participant A, again, after reshuffling the data to destroy the temporal sequence (center panel); and for Participant B who reported high effort to avoid bias (right panel), Study 1. Black data points represent log frequencies below 0, which were used to calculate the 1/*f* slope; gray points represent high-frequency waves, which were excluded from the 1/*f* analysis.

task. The negative slope suggests 1/*f* noise. It is important to note that these patterns are inherently a function of the temporal order of the data series. Compromising that order eliminates the 1/*f* noise pattern. For example, if the residual data that produced Figure 2's left panel are shuffled randomly, a Fourier analysis yields a flat, white PSD (see Figure 2, center panel).

The source of 1/*f* noise in reaction times is something of a puzzle—one that is beyond the scope of the current article. The following discussion merely attempts to give a rough overview of some of the important issues. In an influential review, Gilden (2001) referred to 1/*f* noise as a "carrier signal" (p. 55) for the working mind. This is an intriguing and provocative statement, but it leaves the genesis and meaning of the phenomenon unclear. Van Orden and his colleagues (Van Orden & Holden, 2002; Van Orden, Holden, & Turvey, 2003) have observed that 1/*f* noise emerges from dynamic systems operating on the verge of chaos (Bak, 1990). Partly on the basis of this correspondence, they argued that 1/*f* noise in response times stems from self-organized criticality in the brain itself. Usher has tested neural network models, which offer oblique support for this possibility (Usher, Stemmler, & Olami, 1995, cited in Ward, 2002). When these networks are calibrated to behave in a manner consistent with real neurons (i.e., exhibiting priming effects at the level of individual neurons), two critical elements emerge: The system's activation fluctuates chaotically and the individual artificial neurons emit 1/*f* noise. Van Orden ultimately suggested that if the brain functions in a self-organized, verge-of-chaos fashion, psychologists may need to reconsider the basic assumptions of their science. He argued that self-organization implies bidirectional causal influences. Goals and efforts, which are presumed to generate or cause behavior, must also be seen as consequences of the actions to which they give rise. It is important to note, however, that this position is contested (Wagenmakers, Farrell, & Ratcliff, 2004, 2005). Although the existence of 1/*f* noise is superficially consistent with chaotic phenomena, it may also stem from purely nonchaotic processes. For example, autocorrelation can produce 1/*f*-like patterns if the correlations exist at several time lags and those lags span several orders of magnitude (Ward, 2002). Wagenmakers argued that 1/*f* noise need not imply chaos and that the pattern can be explained through mechanisms like fluctuations in attention or the aggregation of several sources of short-term serial dependence. For the purposes of the current research, it is not especially important whether 1/*f*-like patterns stem from nonlinear, chaotic processes (long-range dependencies) or simpler, short-range dependencies. The point of this research is simply to test whether such 1/*f* patterning characterizes data from implicit measures in social cognition and to test the degree to which those patterns vary as a function of task-relevant effort.

1/*f* Noise and Task Difficulty

The various possible sources of 1/*f* noise (e.g., chaotic, self-organizing processes in the brain or the cumulative input of various short-term dependencies) raise the possibility that 1/*f* noise may be sensitive to more familiar processes like attention and cognitive effort. It is notable that 1/*f* noise seems to depend on the nature of the task that participants are asked to perform. In simple cognitive tasks (e.g., detecting a right-to-left gradient), a robust 1/*f* pattern is usually obtained. But in more difficult paradigms (e.g., with more response options, when a simple task is complicated by high memory load), the signature slope of 1/*f* noise weakens (Clayton & Frey, 1997; Ward & Richard, 2001, as cited in Ward, 2002). Clayton and Frey asked participants to perform one of three tasks. The easiest involved pressing one button if the stimulus on a given trial was an *X* and another button if the stimulus was an *O*. More difficult tasks involved indicating whether the stimulus on the current trial was identical to the stimulus on the previous trial (a one-back task) or—in the most difficult condition—indicating whether the current stimulus matched the stimulus two trials earlier (a two-back task). 1/*f* noise was clearly evident in the easiest condition, but on the more difficult tasks, the negative relationship between power and frequency flattened dramatically. A task's difficulty, in other words, moderated the emission of 1/*f* noise. PSDs for simple tasks reveal more negative, steeper, *pinker* slopes, whereas PSDs for complex tasks produce more positive, flatter, *whiter* slopes.

Whereas extant work has examined variation in 1/*f* noise as a function of the nature of the task a participant performs, the current research explores a different question. In this article, I test the possibility that even when participants perform an identical task, differences in the way they approach that task will affect the expression of 1/*f* noise. In tasks related to prejudice and stereotyping, there is reason to expect that some individuals will exert more effort than others. Some participants express strong motivation to avoid prejudiced behavior for a variety of reasons, and this motivation significantly alters their performance on both explicit questionnaire measures and implicit measures of racial bias (Devine, Plant, Amodio, Harmon-Jones, & Vance, 2002; Dunton & Fazio, 1997; Plant & Devine, 1998). More recently, Glaser and Knowles (in press) have measured implicit motivation to control prejudice. Someone who is high in this motivation should strive to appear—or even to be—egalitarian. Someone who is low in this motivation typically worries less about the potentially racist implications of his or her behavior.

In the current studies, I hypothesized that a participant's effort on a given task will affect the way they approach implicit measures of racial bias, even if those measures were designed to be impervious to strategic control (Fazio et al., 1995). Highly motivated participants should attempt to control their behavior (Monteith, 1993; Richeson & Trawalter, 2005), and their desire to avoid bias should prompt increased cognitive effort. In line with the effects reported by Clayton and Frey (1997) and Ward and Richard (2001, as cited in Ward, 2002), it was predicted that effort would reduce the prevalence of 1/*f* noise. If this hypothesis is correct, high effort should be associated with more positive PSD slopes, and low effort should be associated with more negative slopes.

This hypothesis was tested in two studies. In Study 1, participants performed a video game task in which they made shoot/don't-shoot decisions for a series of armed and unarmed targets who were either White or Black (Correll, Park, Judd, & Wittenbrink, 2002). This task has been used to measure racial bias. Participants subsequently reported (a) the extent to which they had tried to avoid showing bias and (b) the perceived difficulty of the task. In Study 2, a different task was used to measure racial bias, and participants' effort was experimentally manipulated. Participants performed the computer task after receiving explicit instructions either to use racial stereotypes or to avoid using stereotypes or in the absence of any specific instructions (the control condition). In both studies, participants who exerted greater effort, either on their own or because they were instructed to do so, were expected to demonstrate reductions in 1/*f* noise.

Study 1

Method

Participants and Design

Twenty-four White undergraduates participated in partial fulfillment of a course requirement. During the experimental session, each participant performed a shoot/don't-shoot video game task, which presented Black and White targets who were either armed or unarmed. The goal of this task was to shoot all armed targets but to indicate *don't shoot* for all unarmed targets. Participants also completed a modified version of a Stroop (1935) task, described below, which was included to allow the examination of discriminant validity. This second task helps determine whether results on the video game are specific to that measure or, instead, emerge in any reaction-time task. The results reported by Clayton and Frey (1997) suggest that more challenging tasks reduce 1/*f* noise, but it is not clear what drives this effect. Task-relevant effort and perceived task difficulty might both be associated with reductions in 1/*f* noise in the video game. On completion of both tasks, participants therefore reported the degree to which they tried to avoid biased behavior in the video game task (*rated effort*) and the degree to which the task was perceived as difficult (*rated difficulty*). The study followed a 2 (target race: Black vs. White) $\times$ 2 (object type: gun vs. nongun) within-participants design, with rated effort and rated difficulty measured continuously as between-participants factors.

Materials

Video game. This study used a simple video game task (for details, see Correll et al., 2002). As implemented in Study 1, the task consisted of 200 trials in which participants were asked to respond to an image of a young man (i.e., a target). One hundred trials presented a Black target; 100 trials presented a White target. Within each race, half of the targets held pistols and half held innocuous objects (e.g., cell phones or wallets). Fifty targets, then, appeared in each of the four cells defined by the 2 $\times$ 2 repeated-measures design: unarmed White, unarmed Black, armed White, armed Black. Participants were instructed to press a button labeled *shoot* if the target was armed but to press a separate button labeled *don't shoot* for any unarmed targets. This task typically imposes a response window of 850 ms, but to avoid missing data (which is problematic for Fourier analysis), this response deadline was eliminated in the present study. A 500-ms intertrial interval preceded each trial. A 16-trial practice round preceded the test phase.

Stroop task. The modified Stroop task involved a series of trials on which a fixation cross (500 ms) appeared on the computer screen, followed by a simple letter string (*xxxx*) in one of four colors: red, green, blue, or yellow. Participants were asked to categorize the color of the typeface by pressing one of four color-designated keys. The task began with a 40-trial block of practice trials after which participants were asked to contact the experimenter if they had any questions. After the experimenter addressed any questions, participants pressed the space bar to resume the task, which started with 10 buffer trials prior to a 200-trial test block. A 1,000-ms intertrial interval preceded each trial.

Questionnaire. After the computer tasks, participants completed a short questionnaire, which included questions assessing demographics, measures of prejudice, and two critical questions. The first question (rated effort) simply asked, with reference to the video game task, "How hard did you try to avoid showing racial bias?" Response options ranged from 1 (*I didn't try at all*) to 9 (*I tried very hard*). The second question (rated difficulty) also referred to the video game task: "How difficult was the task?" Response options ranged from 1 (*not at all difficult*) to 9 (*very difficult*).

Procedure

A White female research assistant met participants individually and introduced the shoot/don't-shoot and Stroop tasks as tests of vigilance. After the tasks, participants completed the questionnaire. They were then thanked and debriefed.

Results and Discussion

Mean-Level Effects

Reaction times for correct trials on the video game task were log transformed (data are reported in the original metric for ease of comprehension). Log-transformed latencies were then averaged separately for each of the four target types: unarmed White, unarmed Black, armed White, armed Black. Similarly, error rates were calculated for each trial type. These averages were submitted to separate Target Race × Object Type repeated-measures analyses. In this paradigm, participants typically respond more slowly and with a greater number of errors to targets that violate cultural stereotypes (unarmed Blacks and armed Whites) and more quickly and with fewer errors to targets that conform to those stereotypes (armed Blacks and unarmed Whites). In line with prior work, latencies from Study 1 revealed a pattern of bias, manifested as a significant interaction between target race and object type, $F(1, 23) = 13.91$, $p < .002$. Participants shot armed Black targets ($M = 627$ ms) more quickly than they shot armed Whites ($M = 648$ ms), $F(1, 23) = 5.38$, $p < .03$, and they were faster to choose the *don't shoot* response when an unarmed target was White ($M = 684$ ms) rather than Black ($M = 705$ ms), $F(1, 23) = 14.06$, $p < .002$. For each participant, an index of bias was calculated, reflecting the magnitude of the within-subject interaction (i.e., bias = [armed White–armed Black] + [unarmed Black–unarmed White]).

In an analysis of error rates, the Target Race × Object Type interaction was similar in direction but did not approach significance, $F(1, 23) = 0.99$, $p < .34$. This is likely a consequence of the extended time window, which tends to reduce error rates (see Correll et al., 2002).

A regression was performed for each participant to analyze $1/f$ noise, modeling the log-transformed latency on a given trial as a function of the target type (armed Black, armed White, unarmed Black, unarmed White), accuracy (correct response vs. incorrect response), and trial number. These regressions were designed to remove known sources of variability from the reaction times. Trial number was included to account for increases in speed over the course of the task—a nonstationarity that can disrupt the Fourier analysis. PSD was calculated for each participant using the Statistical Analysis Software SPECTRA procedure with a Tukey–Hanning window (see Appendix for example syntax). With 200 trials, the FFT decomposed each trial series into 100 component waves. The analysis thus provides estimates of the power and frequency of 100 waves for each participant. The lowest estimable frequency describes a wave with a single cycle over the course of the task ($f = 2\pi$ radians/200 trials = .031 radians/trial). The highest estimable frequency (the Nyquist frequency) is half the sampling rate, referring to a wave that cycles once every 2 trials ($f = 2\pi$ radians/2 trials = 3.142 radians/trial).

A visual inspection of the video game PSDs revealed a generally flat slope at high frequencies. This pattern, suggestive of white noise, conforms to Gilden's dual-component model (2001; Gilden et al., 1995), which argues that $1/f$ patterns emerge only at lower frequencies. It is interesting that at lower frequencies, participants seemed to vary dramatically. Some showed a relatively steep slope; others showed a fairly flat slope (see Figure 2, left and right panels, respectively).

A second within-subject regression was performed to quantify the power–frequency relationship for each participant. The goal was to estimate the relationship at low frequencies, but this effort is complicated by the general prevalence of white noise at high frequencies (Gilden, 2001). In essence, the power–frequency relationship is not a straight line; it has a kind of elbow. Clayton and Frey (1997) addressed this nonlinear relationship by excluding high-frequency data and estimating $1/f$ noise as the linear relationship for the component waves below the elbow. Accordingly, the linear relationship between power and frequency was estimated for log frequencies below zero. As discussed above, if the residual latencies contain nothing but random error, the average slope estimated by this regression should be flat (it should hover around zero). Such a slope would indicate white noise. By contrast, $1/f$ noise would produce a negative slope and a significant deviation from zero. Across participants, the average linear slope was, indeed, significantly negative, mean slope = −0.18, $t(23) = -2.21$, $p < .04$, suggesting that these residuals are not entirely random. This test provides the first evidence of $1/f$ phenomena in social–cognitive latency measures.

Correlational Analyses

My primary goal in Study 1 was to examine the relationship between a participant's PSD slope in the video game and (a) efforts to avoid bias and/or (b) the perceived difficulty of the task. Prior literature offers no real basis for an a priori prediction about the distinction between rated effort and rated difficulty in this study. Participants low in rated effort and participants low in rated difficulty might both be expected to exhibit a more steeply negative $1/f$ slope, whereas those reporting high effort and those reporting high difficulty should produce a more positive, flatter slope. Rated effort and rated difficulty were, somewhat surprisingly, unrelated, $r(22) = .24$, $p < .24$. They are therefore treated separately below. The relationship between rated effort and $1/f$ noise was tested by regressing participants' slopes (derived from the PSD analyses) on their explicit ratings of effort. The relationship was significant and positive, $F(1, 22) = 6.33$, $p < .02$ (see Figure 3 and Table 1). This relationship remains significant when controlling for task difficulty, $F(1, 21) = 5.24$, $p < .04$. PSD slopes were then estimated for participants with high and low rated effort. Participants reporting relatively low effort (1 standard de-

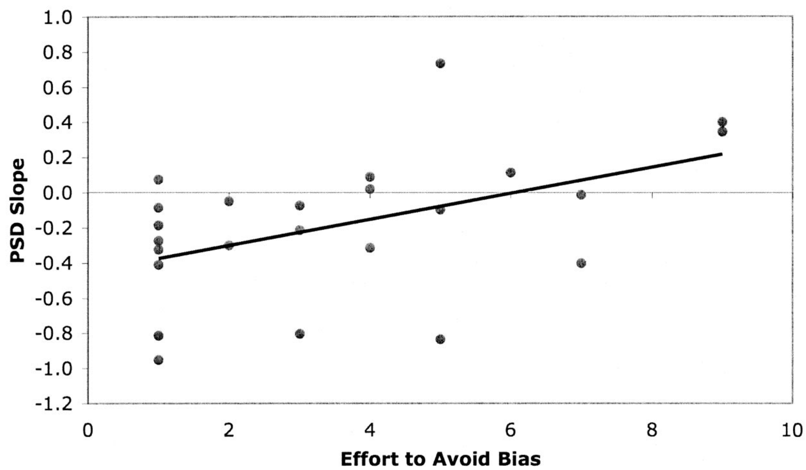

Figure 3. Power spectral density (PSD) slopes for participants plotted as a function of rated effort to avoid bias, Study 1.

viation below the mean) showed clear evidence of 1/*f* noise. Their simple linear slopes were negative and significantly different from zero, mean slope = -0.39, $t(22) = -3.51$, $p < .002$. Participants reporting greater effort (1 standard deviation above the mean) showed less negative (more positive) slopes, which did not differ significantly from zero, mean slope = -0.02, $t(22) = -0.16$, $p < .88$. This nonsignificant slope (white noise is, in essence, a null effect) is not especially important in its own right. The critical point of this analysis is that there is a significant relationship between the questionnaire measure (self-reported effort to avoid bias) and the expression of 1/*f* noise in this task. Higher effort is associated with increasingly flat (relatively white) PSD slopes.

A similar regression offered no evidence that rated difficulty was related to 1/*f* noise, $F(1, 22) = 0.91$, $p < .35$. Although these data show no relationship, this study obviously does not provide a conclusive test of the relationship between perceived difficulty and 1/*f* noise. It seems plausible that social desirability concerns affected participants' willingness to report that a task (one with clear implications for racial bias) was difficult for them. Because the questionnaire followed both the video game and the Stroop task, it is also certainly possible that participants understood the question as referring to the entire study (both tasks) rather than just the video game (see below).

It was predicted that the that residual latencies in Study 1's shoot/don't-shoot task would show evidence of nonrandom trial-to-trial variation consistent with 1/*f* noise. Further, the magnitude of this noise was expected to covary with participants' orientation to the computer task. The PSD analyses showed clear evidence of 1/*f* noise, on average, and also revealed the anticipated relationship between 1/*f* noise and participants' self-reported attempts to avoid bias during the course of the game. Participants who made little effort showed a relatively steep PSD slope. Participants who put forth greater effort showed a flatter slope.

The significance of these results is manifold. First, at the simplest level, they show that residual variance in a participant's response times during a computer-based measure of bias (variance that is usually treated as meaningless error) is not completely random. These findings add to the growing body of work on 1/*f* noise by demonstrating the patterns in socially oriented response-time tasks. In fact, to my knowledge, this is the first demonstration of 1/*f* noise in implicit measures of stereotyping and, indeed, the first demonstration in any kind of social psychological research. Second, whereas Clayton and Frey (1997) showed that 1/*f* noise varies as a function of participants' task, the current work shows that even when participants perform the identical task, the magnitude of 1/*f* noise depends on individual differences in effort. It is critical to note that in the current work, all participants performed exactly the same task. Variability in 1/*f* noise was due entirely to differences in participants' orientation to that task. This study therefore extends understanding of the variables that can moderate 1/*f* noise but also holds out promise that spectral analysis can shed light on phenomena important to social psychology.

Table 1
Means, Standard Deviations, and Correlations, Study 1

Variable	*M*	*SD*	1	2	3	4	5
1. Rated effort	3.58^{**}	2.59	—				
2. Rated difficulty	3.16^{**}	1.95	.24	—			
3. Video game PSD slope	-0.18^{*}	0.40	$.47^{*}$	.20	—		
4. Bias in latencies (ms)	38.89^{**}	42.74	−.26	.15	−.05	—	
5. Stroop PSD slope	-0.47^{**}	0.34	.06	$.37^{\dagger}$	.10	.26	—

Note. PSD = power spectral density.
$^{\dagger} p < .10$. $^{*} p < .05$. $^{**} p < .01$.

Ancillary Analyses

A number of additional tests were conducted to clarify the nature of these effects and rule out alternative interpretations. First, one might imagine that the relationship between rated effort and 1/*f* noise emerges simply because high-effort participants respond more slowly (or more quickly) than do low-effort participants. To address this question, I regressed PSD slopes simultaneously on both rated effort and average latencies from the video game. In this analysis, mean latencies were marginally related to slope estimates, $F(1, 21) = 3.06$, $p < .10$, such that longer latencies were associated with flatter slopes. Critically, however, the relationship between effort and PSD slope remained significant when controlling for average reaction time, $F(1, 21) = 6.15$, $p < .03$. Slower responding cannot account for these effects.

Second, as shown in Figure 2, randomly sorting Participant A's residuals prior to the Fourier analysis eliminated the pattern of 1/*f* noise. All participants' residuals were randomly sorted to provide a slightly more rigorous test of the importance of serial order. Next, both the average PSD slope in the video game task (across all participants in the sample) as well as the correlation between those individual slopes and rated effort were computed. This process was repeated 25 times, yielding 25 estimates of the average slope and 25 estimates of the correlation between slope and effort. When the temporal order of the trials was disrupted, the average participants' PSD slope across these 25 replications hovered around zero, mean slope = 0.009, $SD = 0.053$, $t(24) = 0.88$, $p < .39$. Similarly, the average correlation between the slope (once order had been disrupted) and rated effort did not differ from zero, mean correlation = −0.010, $SD = 0.262$, $t(24) = -0.19$, $p < .86$. These analyses suggest that the phenomenon in question is highly dependent on the temporal order of trials. The primary results of this study cannot be explained as an artifact of the distributions of the latencies. They are not due to changes (as a function of effort) in the mean, standard deviation, skewness, or kurtosis of the latency data.

Third, in addition to the video game task, participants in this study completed a modified version of the Stroop (1935) task. The log-transformed latency data from the Stroop task were submitted to a spectral analysis, similar to the video game analysis above, and a PSD slope was calculated. On average, this slope was steeply negative, mean slope = −0.47, $t(23) = -6.92$, $p < .0001$, suggesting a strong pattern of 1/*f* noise. This task is useful in the context of this study because it provides a test of the specificity of the relationship between 1/*f* noise and rated effort, reported above. Does this correlation reflect a general orientation on the part of the participants or rather—as hypothesized—a task-specific orientation to a particular measure of racial bias? It is possible, for example, that individual differences in 1/*f* noise, demonstrated in the video game task, reflect stable differences between participants. Those who show less 1/*f* noise on one task may show less 1/*f* noise on any task. Conceivably, of course, such a reduction could still be driven by effort, such that some participants exert more effort on any task they perform. In either case, if the patterns reported above reflect a general orientation to response-latency tasks, one might expect (a) rated effort on the video game to predict 1/*f* noise in the Stroop task and (b) 1/*f* noise in the video game task to correlate with 1/*f* noise in the Stroop task. The general-orientation account was tested by examining correlations of PSD slopes from the Stroop task with rated effort and rated difficulty (measures by which participants were supposed to assess the video game task), PSD slopes from the video game task, and bias in the video game task (see Table 1). Of critical importance, rated effort to avoid racial bias did not predict the Stroop PSD, nor were the PSD slopes from the video game related to those from the Stroop task. Further, when PSD slopes from the video game were regressed on rated effort while controlling for slopes in the Stroop task, the critical relationship between rated effort and 1/*f* noise in the video game task remained significant, $F(1, 21) = 5.95$, $p < .03$.

Surprisingly, however, Stroop PSD slopes were marginally correlated with rated difficulty, $r(22) = .38$, $p < .07$. This unexpected relationship raises an interesting, although admittedly post hoc, possibility. When completing the difficulty ratings, participants were asked to rate only the difficulty of the video game task. It may be that the instructions were not sufficiently clear and that participants inappropriately rated the overall difficulty of the session or, perhaps, the difficulty associated specifically with the Stroop task. This tendency may have been exacerbated because the initial question, on which participants rated their effort, focused on attempts to avoid racial bias, which necessarily referred exclusively to the racially relevant video game. The question would make no sense for the Stroop. Participants may then have expected that the other question (on which participants rated task difficulty) applied to the other task (the Stroop; e.g., Heibeck & Markman, 1987; Waxman & Klibanoff, 2000).

In any event, the primary findings of Study 1 provide support for a relationship between task-relevant effort and 1/*f* noise. Self-reported effort to avoid bias uniquely predicted reductions in 1/*f* noise in a task that was relevant to racial bias (the shoot/don't-shoot video game). This measure had no impact on 1/*f* noise in a race-neutral Stroop task. Study 1, then, provides a correlational demonstration of the relationship between 1/*f* noise and effort on a single measure of racial bias. Study 2 was conducted to replicate and extend these findings by using an experimental manipulation to test the causal relationship between effort and 1/*f* noise.

Study 2

In Study 1, effort was measured using a questionnaire. The correlation between participants' self-reported effort and the PSD patterns derived from their response times suggests that effort can moderate 1/*f* noise. Study 2 sought to bolster this claim by experimentally manipulating participants' effort on a separate computer-based measure of racial bias.

This research essentially strives to recreate a study conducted by Payne and his colleagues (Payne et al., 2002), who asked participants to either accentuate or avoid the use of racial cues in a weapon-identification paradigm. Like the shoot/don't-shoot task in Study 1, this task assesses racial bias in reactions to guns and harmless objects. On a series of trials, participants were primed with either a Black face or a White face. They were then asked to identify a target image as either a gun or a tool. In their study, Payne and his colleagues varied the instructions they gave to participants. Participants in the control condition were simply asked to perform the task to the best of their ability. A second group was asked to perform the task but was specifically instructed to avoid the use of race. They were not to allow the race of the prime to influence their judgments. A third group was explicitly asked to make use of race during the performance of the task. They were asked to behave like racial profilers. Payne found greater bias in both experimental conditions. This finding makes perfect sense for the participants who were instructed to act like profilers. They were simply doing what they were told. But, ironically, the participants who were asked to avoid using the racial cues showed increases in bias. Relative to controls, they showed stronger associations between Blacks and guns. The authors suggested that because the instructions called attention to the race of the prime, participants in the experimental conditions were more susceptible to race-based associations (whether that influence was consistent or inconsistent with their instructions).

In addition to demonstrating an interesting and counterintuitive effect, Payne et al.'s (2002) study has clear implications for task-relevant effort. Participants in the control condition approached the task without any experimentally imposed demands. By contrast, participants in both the avoid-race and use-race conditions were asked to consciously control or modulate their performance. Whether or not their efforts were ultimately successful, these participants approached the task with additional experimentally imposed demands. If 1/*f* noise is sensitive to increased effort, spectral analysis should reveal differences between the relatively minimal effort required of the control condition and the greater effort required of the experimental conditions.

Method

Participants and Design

Seventy-one undergraduates participated in return for course credit. All participants performed a simple task in which they were asked to classify objects that appeared on a computer screen as either guns or tools. Prior to the presentation of the target object, however, participants were primed with either a Black or a White face (Payne, 2001). Although all participants performed the same task, individuals were randomly assigned to one of three conditions, each imposing a different task-relevant goal. Participants in the control condition were given no specific instructions; participants in the avoid-race condition were instructed not to use racial information when making their decisions; participants in the use-race condition were instructed to let racial cues guide their behavior during the task. This study followed a 3 (instructions: control vs. avoid race vs. use race) × 2 (prime race: Black vs. White) × 2 (object type: gun vs. tool) mixed-model design, with repeated measures on the last two factors.

Materials

The weapon-identification task was based on the paradigm developed by Payne (2001, Study 1). The task consisted of a 25-trial practice phase followed by a test phase of 200 trials. Each trial began with the presentation of a prime face (200 ms). Primes consisted of black-and-white photographs (faces only) of five Black men and five White men. Immediately after the prime, a target object appeared (200 ms). Target stimuli consisted of black-and-white photographs of 5 guns and 5 power tools (drills, screwdrivers, etc). After 200 ms, the target image was replaced with a mask (a random pattern of black and white rectangles), which remained on screen until the participant responded. A 1,000-ms intertrial interval preceded the onset of the next prime.

In the original study, Payne and his colleagues (2002) forced participants to respond within 700–1,200 ms after the onset of the object image. They also presented the target object for only 100 ms (rather than 200 ms, as in the present research). Payne et al.'s parameters were presumably motivated by their interest in process dissociation procedure estimates of control and automatic bias, which are derived from patterns of errors (see also Payne, 2001, Study 2). More restrictive time limits tend to increase the likelihood of errors in these tasks. By contrast, the current research focuses on variance in reaction times, and forcing participants to respond within a given time window carries with it the possibility that participants will respond too slowly on some trials, resulting in missing data, which (as noted above) are problematic for the analysis. The test phase of Study 2 therefore imposed no response deadline. To encourage participants to respond quickly, the practice phase imposed a deadline of 1,000 ms. Participants who responded too slowly during the initial 25 trials received a message to that effect. Fewer than 1% of responses exceeded 1,000 ms during the test phase.

Procedure

A White male experimenter introduced the study as an investigation of vigilance. He seated participants in individual rooms, each equipped with a computer. A randomly selected computer program delivered the instructions, which constituted the experimental manipulation. These instructions were taken directly from Payne et al. (2002).

Results and Discussion

Mean-Level Effects

In the weapon identification task, analyses of reaction times were based on the log-transformed latencies from trials on which participants had responded correctly. These values were averaged for each of the four trial types (Black–gun, White–gun, Black–tool, White–tool). Similarly, error rates were calculated for each trial type. These averages were submitted to separate Prime Race × Object Type repeated-measures analyses, which revealed an interaction (i.e., bias) in participants' errors, $F(1, 70) = 10.05$, $p < .003$, and a nonsignificant interaction (similar in direction to the error effects) in their latencies, $F(1, 70) = 1.23$, $p < .30$. The error analysis suggests that participants were more likely to classify a tool as a weapon after a Black face ($M = 2.66$) than after a White face ($M = 2.44$) but more likely to identify a gun as a tool after a White face ($M = 2.66$) than after a Black face ($M = 1.87$). These results are somewhat surprising given the lack of any response deadline in this task. For subsequent analyses, two parallel indices were computed to reflect the Prime Race × Object Type interactions for errors and, separately, for latencies. In both cases, bias = (White–gun − Black–gun) + (Black–tool − White–tool).

Analysis of 1/*f* noise was conducted as in Study 1. For each participant, FFTs were applied to the residuals of the latencies (after statistically removing effects of trial type, accuracy, and trial number). The resulting estimates of power and frequency were log transformed. The linear relationship between power and frequency was assessed with a second within-subject regression, excluding log frequencies greater than zero to eliminate the white noise at higher frequencies. Averaging across participants, the average slope of this relationship was negative, $M = -0.33$, $t(70) = -8.48$, $p < .001$, indicative of 1/*f* noise.

Effects of Condition

Estimates of the slopes as well as estimates of bias in both the errors and the latencies were then analyzed as a function of condition. These analyses used two orthogonal codes. The first (control = −2, avoid race = +1, use race = +1) assessed differences between the control condition and the two experimental conditions. The second (control = 0, avoid race = −1, use race = +1) assessed differences between the avoid-race and use-race conditions.

Study 2's primary question involves the slopes derived from the PSD estimates. As task-relevant effort increases, participants were expected to exhibit flatter, more positive slopes. It was therefore predicted that participants in the use-race and avoid-race conditions (conditions that imposed additional task demands) would show more positive (less negative) PSD slopes. As predicted, analysis revealed a significant difference between the control condition and the average of the two experimental conditions in terms of the magnitude of the PSD slopes, $F(1, 68) = 5.52$, $p < .02$ (see Table 2 and Figure 4). Pairwise comparisons showed that participants in both the avoid-race condition and the use-race condition exhibited more positive slopes (suggesting greater effort) than did participants in the control condition, Fs$(1, 68) = 5.24, 3.17$, ps $<$ .03, .08, respectively, although the latter comparison was only marginally significant. Participants in the avoid-race condition did not differ from those in the use-race condition, $F(1, 68) = 0.19$, $p < .66$. When participants' average latency was included as a covariate, $F(1, 67) = 3.13$, $p < .09$, these results did not change: Control participants showed steeper slopes than did experimental participants, $F(1, 67) = 5.90$, $p < .02$, and participants told to avoid race did not differ from those told to use race, $F(1, 67) = 0.51$, $p < .70$. This analysis suggests that the condition differences in 1/*f* noise are not a simple by-product of slower or faster responses in general.

Analysis of bias in the errors revealed no effects of condition, Fs$(68) < 1.44$, ps $> .23$. (Signal detection and process dissociation procedure analyses yield similar results.) Although these data replicate those of Payne et al. (2002) on a mean level, the relatively long time window in the current study may have reduced the reliability of estimates based on error rates and attenuated the likelihood of detecting condition differences (see Table 2).

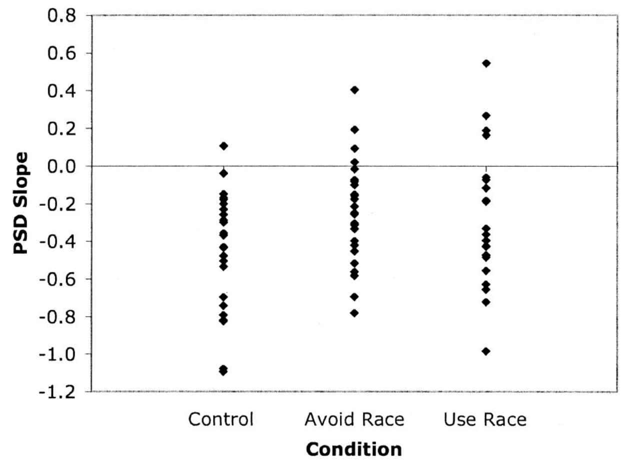

Figure 4. Power spectral density (PSD) slopes for participants in the absence of any instructions (control), for participants instructed to avoid the use of racial cues, and for participants instructed to use racial cues, Study 2.

Analysis of bias in reaction times yielded a marginal effect, such that participants instructed to either avoid or use racial cues during the weapon-identification task showed greater bias than did participants in the control condition, $F(1, 68) = 2.96$, $p < .09$. This effect is consistent with that found by Payne et al. (2002), who showed that either type of instruction induced greater bias. Participants instructed to use race did not differ from those instructed to avoid using race, $F(1, 68) = 0.16$, $p < .70$.

As in Study 1, these results show that participants who attempt to modulate their behavior on a measure of implicit racial attitudes show reductions in 1/*f* noise relative to participants who approach the task in a more naive fashion. It is important to note that, unlike Study 1, the current study used an experimental manipulation of effort by providing participants with randomly assigned instructions. Accordingly, Study 2 provides evidence that effort causally affects the emission of 1/*f* noise.

General Discussion

In two studies, I examined residual latencies from computer-based measures of racial bias. The residuals revealed clear evi-

Table 2
Means and Standard Deviations by Condition, Study 2

	Condition					
	Control (n = 24)		Use race (n = 25)		Avoid race (n = 22)	
Measure	*M*	*SD*	*M*	*SD*	*M*	*SD*
Slope in PSD	-0.45^{**}	0.32	-0.29^{**}	0.36	-0.25^{**}	0.28
Bias in errors	0.63	2.86	1.67^{*}	3.25	0.80^{*}	1.71
Bias in latencies (ms)	−4.98	38.43	$11.69^{\dagger}$	28.14	7.38	30.54

Note. PSD = power spectral density.
$^{\dagger}p < .10$. $^{*}p < .05$. $^{**}p < .01$.

dence of nonrandom variability. Consistent with work in cognitive psychology, error variance was characterized by 1/*f* noise—a complex pattern of serial dependency that can be assessed through spectral analysis (Gilden, 2001). Research has shown that this phenomenon is more pronounced when tasks are easy (rather than difficult), and in the absence (rather than in the presence) of memory load (Clayton & Frey, 1997). Whereas in previous work, the nature of the materials has been manipulated through the use of fundamentally different computer tasks, in the current research, the task was held constant across all participants in each study. These studies investigated variability only in participants' effort or orientation to a given task. Specifically, in these studies, I examined the impact of dispositional and experimentally induced efforts to avoid biased behavior. The hypothesis, building on the work of Clayton and Frey, suggested that effort would decrease 1/*f* noise in tasks related to racial bias.

In Study 1, participants performed a shoot/don't-shoot task with Black and White targets. On average, the data showed clear evidence of 1/*f* noise (a negative PSD slope). Moreover, the magnitude of the effect was attenuated (yielding PSD slopes closer to zero) for participants who reported greater effort. By contrast, the 1/*f* pattern was more pronounced among participants who reported that they did not try to avoid bias during the task. Study 2 experimentally manipulated participants' effort to control their behavior in a weapon-identification task (a measure of racial bias; Payne, 2001). Those who were given additional task demands—explicit instructions to either use or avoid the use of racial cues—showed reduced 1/*f* noise compared with participants in the control condition who were given no additional demands. Together, these studies suggest that 1/*f* noise characterizes response times in social–cognitive measures of stereotyping and prejudice and (importantly) that these patterns are sensitive to a participant's task-relevant effort.

These data have implications for the continuing debate about the meaning and utility of implicit measures. Like other measures before them, measures of implicit or automatic attitudes have gained prominence in social psychology by virtue of their purported ability to assess participants' views in ways that are relatively insensitive to concerns over social desirability. Clearly, computer-based implicit measures can be differentiated from simple questionnaire measures, and they seem to offer unique predictive power (Dovidio, Kawakami, & Gaertner, 2002; McConnell & Leibold, 2001). But there is still a lack of clarity regarding what, exactly, they measure and how susceptible they truly are to demand (e.g., Karpinski & Hilton, 2001). A number of studies (Fiedler & Bluemke, 2005; Glaser & Knowles, in press; Hausmann & Ryan, 2004) have recently shown that a participant's motivation to behave in an unbiased fashion (or at least to present him- or herself as unbiased) can impact measures like the IAT. The current studies are similarly predicated on the idea that individuals approach even implicit measures with an awareness of the purpose of the research and with an intention to bring their performance into line with their goals and standards (Monteith, 1993). Of course, intention does not, in and of itself, mean that participants will be successful in their efforts. Indeed, their efforts may even backfire (Study 2; Payne et al., 2002). But it does imply that individual and situational differences may affect participants' orientation to implicit measures in fundamental ways with consequences that can be hard to predict.

Limitations and Future Directions

The current studies constitute the first known application of 1/*f* analysis to social psychological research in general and to the domain of stereotyping and prejudice in particular. This work has accordingly focused on a single question: Does effort affect 1/*f* noise on measures of racial bias? In the current studies, I examined this question using two operationalizations of effort and two measures of bias. Although some researchers may find these effects intriguing, it is important to note a few key limitations.

Sensitivity Versus Specificity

First, the current research deals exclusively with the issue of sensitivity. Each of the studies reported here shows that 1/*f* noise depends on or is sensitive to task-relevant effort. At present, there is no evidence concerning the specificity of the relationship between effort and 1/*f* noise. Treating 1/*f* noise as a proxy for effort is therefore logically problematic (Cacioppo & Tassinary, 1990). To the extent that other variables increase or decrease the magnitude of the PSD slope, variation in 1/*f* noise may emerge independent of effort. Future research on other causal factors will likely improve understanding of 1/*f* noise and ultimately increase the utility of this approach.

Mechanism

A second issue, related to the first, is that research has not yet specified the psychological processes through which effort affects 1/*f* noise. Previous work has suggested that task difficulty affects 1/*f* noise (Clayton & Frey, 1997; Ward & Richard, 2001, as cited in Ward, 2002), and the fact that effort reduces 1/*f* noise in these studies certainly seems consistent with the putative role of effort and/or the possibility that effort imposes additional demands. But effort is likely to induce a number of more specific psychological states, which may also give rise to 1/*f* noise. For example, effort may produce anxiety if participants worry about revealing some undesirable bias. It may also increase attention to the task at hand or even create a dual-task situation such that participants attempt to simultaneously (a) perform the basic task (e.g., accurately differentiate guns from tools) and (b) modulate the influence of race on their responses. The mediating role (or roles) of anxiety, attention, and difficulty must be tested.

Baby or Bathwater?

The measures of 1/*f* noise used in the current studies are derived from nothing more than latency data—the kind of data that are routinely collected in thousands of studies in hundreds of laboratories every year. These data not only yield estimates of the principal construct under investigation (e.g., racial bias in the decision to shoot), they also seem to offer hints about the participants' orientation to the task. This information, embedded in the residuals of the latencies, is typically ignored. To the extent that a researcher's question does not concern the participants' effort, researchers may lose little by disregarding patterns of trial-to-trial variability. In some cases, however, research may profit tremendously by virtue of this inconspicuous and generally untapped measure. That is, researchers may be able to harvest additional information from nothing more than the data they have already collected.

As understanding of this phenomenon improves (e.g., as researchers learn about the specificity of 1/*f* noise), spectral analysis may provide a tool for the researcher who hopes to mine data sets (new and old) to examine questions of orientation and effort. Reciprocally, existing data sets may improve understanding of 1/*f* noise by allowing for the identification of variables that moderate and mediate the types of effects reported here. These possibilities are particularly interesting in light of recent attempts to measure implicit efforts to control prejudice (Glaser & Knowles, in press). There has been some speculation that measures of effort to control prejudice may, themselves, be subject to social desirability concerns. In an individualistic society like the United States, for example, there may be pressure to endorse statements such as "I always express my thoughts and feelings, regardless of how controversial they might be" (Dunton & Fazio, 1997). With additional testing, spectral analysis may allow researchers to assess efforts to avoid prejudice in an almost completely covert and demand-free fashion.

It may be useful to speculate, briefly, about the IAT, which has been adopted as the implicit measure of choice in many laboratories. There is some reason to think that 1/*f* noise will be difficult or impossible to evaluate in the context of this popular measure. The IAT consists of two separate judgment tasks (e.g., classifying a face as Black or White, classifying a word as good or bad), which are mapped onto a single pair of response options. On one block of trials, a participant might be asked to press Button 1 if a face is Black or if a word is bad but to press Button 2 if a face is White or a word is good. If the participant holds a negative view of Blacks (or a positive view of Whites), these pairings are congruent. On another block, the participant would be asked to reorient one of the judgments, now pressing Button 1 if a face is White or if a word is bad but pressing Button 2 if a face is Black or a word is good. If this orientation is inconsistent with his or her views, the participant should experience greater difficulty. From the perspective of a spectral analysis, the IAT may prove problematic because the two blocks of trials involve vastly different degrees of effort, which should yield different patterns of 1/*f* noise. In fact, a highly prejudiced participant is expected to respond quickly, accurately, and easily on the congruent block (which may yield steep PSD slopes) but slowly, inaccurately, and with great difficulty on the incongruent block (which may yield flatter slopes). Analyzing the differences in slope between the blocks may address this problem, but the FFT procedure requires many trials to estimate low-frequency waves. Unless the IATs include sufficient trials in each block, low-frequency data (which are diagnostic of 1/*f* noise) may not be available.

For social psychology, 1/*f* noise and other dynamic approaches (e.g., Markov analyses) seem to hold tremendous promise. 1/*f* noise itself is based on the idea that events at time t may have implications for time $t + 1$ (or $t + 100$). That is, 1/*f* noise can reflect a dynamic system, where events are not isolated or disconnected but intertwined. Social psychological phenomena that unfold over time (e.g., conversations between individuals, behavioral mimicry, attraction, intergroup competition) reflect similarly complex influences. Given concern with these processes, analytical techniques that can address dynamic relationships seem like a natural fit for the discipline (Vallacher, Read, & Nowak, 2002).

Conclusions

1/*f* noise is striking because it violates expectations. A visual inspection of residual latencies in a sequential priming task yields little in the way of an obvious pattern. The data look random. It is only through spectral analysis that the underlying pattern becomes clear. In the current studies, I explore the utility of 1/*f* noise in the study of racial bias, but the prevalence of computer-based measures in the discipline of psychology raises the possibility that social psychologists can readily use spectral analyses to explore data on a wide variety of topics (e.g., self-esteem, attitudes, negotiation), gaining a deeper understanding of the processes they study. By so doing, social psychologists may reciprocally provide a unique perspective on the meaning of 1/*f* noise.

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Appendix

Example SAS Code

**This section regresses latencies on independent variables ;

```
data working;
set datafile;
lntime = log(latency);
accurate = 1*(response = 'correct')
-1*(response = 'incorrect');

proc reg noprint;
model lntime = racecode objectcode
racexobjcode trialnumber accurate;
output out=outputfile1 r=residual;
by participantnumber;
run;

data outputfile1;
set outputfile1;
```

**This section performs a fast Fourier transform for each participant ;

```
proc spectra adjmean p s out=outputfile2;
var residual;
weights tukey;
by participantnumber;
run;
```

**This step removes a constant or DC component from the PSD estimates ;

```
data outputfile2;
set outputfile2;
if freq ne 0;
```

**This step log transforms power and frequency estimates ;

```
lnpower = log(p_01);
lnfreq = log(freq);
```

**This section removes high-frequency data (LN>ZERO) and computes linear relation between power & frequency for each participant, saving coefficients to a final data file called outputfile3 ;

```
data outputfile2;
set outputfile2;
if lnfreq < 0;
proc reg noprint outest = outputfile3;
model lnpower = lnfreq;
by participantnumber;
run;
```

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