Psychological Bulletin
2006, Vol. 132, No. 6, 895–919

0033-2909/06/$12.00 DOI: 10.1037/0033-2909.132.6.895

The Actor–Observer Asymmetry in Attribution: A (Surprising) Meta-Analysis

Bertram F. Malle
University of Oregon

The actor–observer hypothesis (E. E. Jones & R. E. Nisbett, 1971) states that people tend to explain their own behavior with situation causes and other people's behavior with person causes. Widely known in psychology, this asymmetry has been described as robust, firmly established, and pervasive. However, a meta-analysis on 173 published studies revealed average effect sizes from $d = -0.016$ to $d = 0.095$. A moderator analysis showed that the asymmetry held only when the actor was portrayed as highly idiosyncratic, when hypothetical events were explained, when actor and observer were intimates, or when free-response explanations were coded. In addition, the asymmetry held for negative events, but a reverse asymmetry held for positive events. This valence effect may indicate a self-serving pattern in attribution, but across valence, no actor–observer asymmetry exists.

Keywords: self–other, behavior explanations, social psychology, social perception, social cognition
Supplemental data: http://dx.doi.org/10.1037/0033-2909.132.6.895.supp

Self and other are the two chief targets of social cognition, and few assumptions are as compelling as the one that cognition about the self differs in important ways from cognition about others. Many self–other differences have been documented—for attention (Malle & Pearce, 2001; Sheldon & Johnson, 1993), memory (Rogers, Kuiper, & Kirker, 1977), personality description (Locke, 2002; Sande, Goethals, & Radloff, 1988), and evaluative judgment (Greenwald, 1980; Locke, 2002; Taylor & Brown, 1988). However, no difference is better known than the actor–observer asymmetry in attribution. In a famous paper, Jones and Nisbett (1971)[1] formulated the hypothesis that "actors tend to attribute the causes of their behavior to stimuli inherent in the situation, while observers tend to attribute behavior to stable dispositions of the actor" (p. 93). In the research literature on attribution, the classic actor–observer asymmetry has been described as "robust and quite general" (Jones, 1976, p. 304), "firmly established" (Watson, 1982, p. 698), and "an entrenched part of scientific psychology" (Robins, Spranca, & Mendelsohn, 1996, p. 376). Furthermore, "evidence for the actor–observer effect is plentiful" (Fiske & Taylor, 1991, p. 73), and "the actor–observer bias is pervasive" (Aronson, 2002, p. 168). With over 1,500 references to the original Jones and Nisbett paper, there can be little doubt that the actor–observer asymmetry in attribution is central to the cumulative knowledge base of social and cognitive psychology. As a result, the asymmetry is featured in textbooks of social psychology and general psychology alike (e.g., Fiske, 2004; Franzoi, 2006; Gray, 2002; Griggs, 2006; Kenrick, Neuberg, & Cialdini, 2005; Myers, 2004; Rathus, 2004; Taylor, Peplau, & Sears, 2006).

One would therefore expect that the robust claims made about the actor–observer asymmetry are backed by an equally robust evidence base. Surprisingly, however, there is no systematic review available of research testing the actor–observer hypothesis. One review, published more than 20 years ago (Watson, 1982), has often been cited as documenting clear support for the hypothesis, but the article covered only a small portion of studies already published at the time, and many additional studies have since become available. It is unknown how many studies have been conducted to date on the actor–observer hypothesis, how many have confirmed or disconfirmed it, what its precise effect size is, and what factors moderate the effect.

The present article reports the results of a meta-analysis on actor–observer studies published between 1971 and 2004. I begin with a brief review of the original hypothesis. I then describe the parameters of the meta-analysis and report its results, including attempts to identify moderator variables. Finally, I draw theoretical implications from the results and consider a revised treatment of actor–observer differences in attribution.

The Actor–Observer Hypothesis

The actor–observer hypothesis tries to capture the powerful intuition that actors[2] explain their own behavior differently from the way an observer would explain that behavior. For example, senators might explain their votes against going to war by saying, "This war is unjustified," whereas political observers might ex-

Bertram F. Malle, Department of Psychology and Institute of Cognitive and Decision Sciences, University of Oregon.

Thanks to Mark Alicke, David Dunning, and Joachim Krueger for encouraging me to advance this meta-analysis and many thanks to Megan Campbell, Nathan Dieckmann, Jess Holbrook, and Courtney Powal for their coding help. Thanks also to all the authors who succeeded in publishing results that did not fit the classic hypothesis; without them, I would have never found out.

Correspondence concerning this article should be addressed to Bertram F. Malle, Department of Psychology, 1227 University of Oregon, Eugene, OR 97403. E-mail: bfmalle@uoregon.edu

[1] This paper is also frequently cited as having been published as a chapter in Jones et al. (1972, pp. 79–94).

[2] Actors explain a behavior that they have performed; observers explain a behavior that another person has performed.

plain the senators' votes by saying, "They are soft-hearted liberals." The actor–observer asymmetry, so Jones and Nisbett (1971) argued, consists of actors preferring situational explanations for their behaviors and observers preferring personal or dispositional explanations for the actors' behaviors. No generally accepted definitions of situational and personal/dispositional explanations are available, but two widely used measurement methods illustrate what appears to be meant by these constructs. Storms (1973, p. 168) asked his participants to rate the importance of two classes of causes for their behavior in a getting-acquainted interaction (and, with appropriate reformulation, for the other person's behavior):

> A. Personal characteristics: How important were your personality, traits, character, personal style, attitudes, mood, and so on in causing you to behave the way you did?
>
> B. Characteristics of the situation: How important were such factors as being in an experiment, the getting-acquainted situation, the topic of conversation, the way the other participant behaved, and so on in causing you to behave the way you did?

Another widely used method, especially in explanations of achievement outcomes, is to ask participants about the importance of four causal factors in causing the outcome: ability, effort, task characteristics, and luck (Heider, 1958; Weiner et al., 1972). Ability and effort are then averaged to yield an internal cause score (I); task characteristics and luck are averaged to yield an external cause score (E).

Over the years, studies have differed in their specific measurement methods, but all seem to have conceptualized situational explanations as representing or explicitly referring to causes that reside in the environment (e.g., test difficulty, chance, the weather, a stimulus, another person with whom the actor interacts) and personal explanations as representing or explicitly referring to causes that reside in the actor (e.g., effort, ability, attitudes, personality, mood, desires).

Despite the simplicity of the concepts, several caveats must be noted. First, the notion of personal or internal attributions is ambiguous (M. Ross & Fletcher, 1985). Sometimes such attributions refer to any factors that lie within the person (including temporary ones such as emotions or sensations and stable ones such as traits or attitudes); sometimes they refer specifically to stable dispositions (i.e., traits, attitudes). Some researchers therefore speak of person or internal attributions (Heider, 1958; Kelley, 1967); others speak of trait attributions or dispositional attributions (Jones & Davis, 1965). Past studies have not directly compared these two assessments, so one question for the present analysis was whether different attribution assessments differentially support the actor–observer hypothesis.

Second, Jones and Nisbett's (1971) original hypothesis refers to a perceptual or cognitive phenomenon, applying broadly to all kinds of behavior, whether intentional or unintentional, positive or negative. This broad application has continued into the present. However, a separate effect was discovered, dubbed the *self-serving bias in attribution,* according to which actors attribute their failures (or negative behaviors) to situational factors but successes (or positive behaviors) to personal characteristics, whereas observers either do not show this tendency or show the reverse (Ames, Ames, & Garrison, 1977; Bradley, 1978; Small & Peterson, 1981; Taylor & Koivumaki, 1976; G. L. Wells, Petty, Harkins, & Harvey, 1977; Zuckerman, 1979). If one thinks of the self-serving bias as a statistical interaction between perspective (actor–observer) and outcome valence (positive–negative), then the classic actor–observer asymmetry is a main effect of perspective. The focus of the present investigation was on this main effect, but the results—when broken down by attributions for positive and negative events—also speak to the self-serving bias.

Third, the relationship between internal and external attributions has been frequently debated. Originally, the two types of attributions were presumed to be polar opposites. Soon, however, empirical and theoretical doubts arose (Kelley & Michela, 1980; McArthur & Post, 1977; F. D. Miller, Smith, & Uleman, 1981; M. Ross & Fletcher, 1985; Solomon, 1978; Taylor & Koivumaki, 1976). Researchers adopted a more cautious approach by measuring the two types of attribution (internal, external) separately and examining which of them showed a particular effect of interest. Watson (1982) specifically concluded from his review that actors and observers differ only in their external attributions, not in their internal attributions. Even so, the internal–external difference score (I-E) is far more commonly used to test the actor–observer hypothesis than the component scores. The present meta-analysis examined all three scores (I, E, I-E) and thus tested three variants of the actor–observer hypothesis.

A final caveat is that the actor–observer hypothesis should be distinguished from the so-called correspondence bias (Gilbert & Malone, 1995; Jones, 1976), also labeled the *fundamental attribution error* (FAE; L. Ross, 1977). The latter normally refers to the claim that people are prone to infer stable traits from behaviors, even from single behaviors and even when external pressures or incentives operating on the behavior are made clear. Many textbooks have described the FAE as the observer's side of the actor–observer asymmetry (e.g., Deaux & Wrightsman, 1988; Hockenbury & Hockenbury, 2006; Kowalski & Westen, 2005), but this may be misleading because the FAE concerns trait inferences from behavior, whereas the actor–observer asymmetry concerns explanations of behavior (Herzberger & Clore, 1979). The word *attribution* unfortunately has been used both for explanations of behavior (attributing a behavior to its causes) and for inferences of traits in light of a behavior (attributing a trait to a person, given her behavior). However, in many circumstances, the two processes are distinct (Hamilton, 1998; Hilton, Smith, & Kin, 1995), perhaps even inconsistent with one another (Johnson, Jemmott, & Pettigrew, 1984). Suppose a colleague tutors a student who got behind with his class work and one infers that this colleague is generous, helpful, perhaps idealistic (a trait inference). One may not ask why this colleague is helping the student (a causal attribution) because the action does not appear puzzling. If there was something puzzling about the act (because, for example, the colleague had declared on her syllabus that she would not provide extra tutoring), one's trait inferences would not be particularly suitable explanations for why this colleague broke her own rule and helped the student; one would instead look for her motives, her reasons to do so (Malle, 1999). Of course, if one subscribes to a theory of attribution according to which traits are people's favorite mode of explaining behavior, trait inferences and explanations are treated as interchangeable. However, that would be begging the question and, in any case, is not supported by the data (Hamilton, 1998; Herzberger & Clore, 1979; Lewis, 1995; Malle, 1999, 2004). The present article therefore speaks directly to the classic actor–

observer asymmetry in explanations and provides only discussion material for the FAE.

Hypotheses

To be neutral with respect to the ambiguity of person versus dispositional attributions, I formulate the actor–observer hypotheses using the term *internal attributions* and subsume under this term attributions to all person factors, whether they are traits or not. A separate moderator analysis then examines possible differences between dispositional and nondispositional attributions.

The first and primary hypothesis expresses the classic actor–observer asymmetry in terms of a difference score of internal and external attributions. This is the most common way of testing the asymmetry and captures well Jones and Nisbett's (1971) original formulation. According to this hypothesis, observers show relatively more internal versus external attributions than actors do.

H_1: Observer (I-E) > Actor (I-E).

Because this hypothesis describes a difference (actor vs. observer) of differences (internal vs. external), it leaves open the question of whether actors and observers differ mainly in their internal attributions, their external attributions, or both (Watson, 1982). Two specific hypotheses disentangle this issue. One claims that observers offer more internal attributions than actors do; the other claims that actors offer more external attributions than observers do. Either one or both can be true if H_1 is true.

H_{1A}: Observer (I) > Actor (I).

H_{1B}: Actor (E) > Observer (E).

Those studies that measured or manipulated the valence (negative vs. positive) of the explained event also permit a test of what can be considered a self-serving hypothesis: For negative events, observers show a greater internal–external preponderance than actors do; for positive events, it is actors who show a greater internal–external preponderance than observers do.

H_2: Negative: Observer (I-E) > Actor (I-E); and
Positive: Actor (I-E) > Observer (I-E).

Previous meta-analyses focused on actors' contributions to this pattern—that they provide a preponderance of internal over external attributions for positive but not for negative events (Mullen & Riordan, 1988; Whitley & Frieze, 1985, 1986). The present analysis examined both actors' and observers' attributions for positive and negative events.

In addition, the present analysis explored a number of possible moderators including familiarity between the actor and observer, time delay of attributions, observer involvement, visual perspective switch between actor and observer, and various methodological variables (e.g., free-response coding vs. ratings; real vs. hypothetical events; between-subjects vs. within-subject designs).

Method

Any meta-analysis comes with numerous choices—during the search for and inclusion of research articles, during the extraction of individual effect sizes, and during the integration of these effect sizes into the meta-analytic results. In the end, one always hopes that these factors will not matter if the pool of studies is sufficiently large and the results are sufficiently clear. Below, I describe the major decisions and procedures I adopted. In addition, details on effect size extractions and computations are available as supplementary material online to facilitate reanalyses by other researchers.

Article Identification and Selection

To survey the research literature on the actor–observer asymmetry in attribution, I searched four databases: PsycINFO, Web of Science (formerly, the Social Science Citation Index), ERIC, and ArticleFirst. I began by searching for articles since 1971 that contained the words *attribution* and *actor* or *observer* anywhere in title, abstract, or keywords. This search strategy resulted in about 700 references. Second, I searched for articles that had cited Jones and Nisbett (1971), resulting in about 900 additional references. Finally, some articles' reference lists suggested a small number of additional articles to consider (e.g., DeVader, Bateson, & Lord, 1986). I examined document type, title, and abstracts of all articles and selected 250 that were likely to be empirical studies assessing causal attributions from the actor and observer perspective. I then closely examined each article in this set and arrived at the final pool of 113 articles. For a subset of the excluded articles ($N = 95$), I tabulated the reasons for exclusion: Fifty-three failed to assess both actor and observer perspectives (e.g., observer explanations with and without empathy instructions), 31 did not provide causal attribution scores for internal and/or external causes (but rather behavior descriptions, person impressions, trait inferences, or responsibility attributions), 4 did not report empirical data, 4 did not contain enough information to compute or even reconstruct effect sizes, and 3 had other or mixed exclusion reasons. Four foreign-language articles remained in the pool, three of which I translated into English and one of which was translated by a native speaker of Chinese. After the final selection, the pool consisted of 113 articles that reported 173 studies (or independent samples) with data from 14,686 participants. A decision was made not to search for unpublished studies because the time span of 35 years of research on the hypothesis would make it impossible to acquire a representative sample of unpublished studies (especially from earlier years). However, I report a qualitative examination of one type of unpublished study that was available for systematic search, namely, dissertations (see Discussion section, below).

Effect Size Acquisition

I examined the methods and results sections of all selected articles and collected the reported data from which effect sizes could be computed—primarily means, standard deviations, and F or t values. In particular, I derived effect sizes for three dependent measures:

I-E: the difference score of internal minus external attributions (or the interaction term of an internal–external repeated-measures factor and an actor–observer factor);

I: a separate internal score; and

E: a separate external score.

Where possible, groups of participants that resulted from between-subjects manipulations and that represented distinct levels of a potential moderator (e.g., negative vs. positive event explained) entered the meta-analysis as independent samples. Conditions in which the researchers explicitly aimed at reversing or eliminating the standard asymmetry (e.g., visual perspective switch, involved observers) did not enter the overall meta-analytic averages but were analyzed as potential moderators. Control conditions in such studies, with which the reversal conditions were compared, did enter the overall averages.

In some cases, extracting the necessary information required inferences with some measure of uncertainty, such as identifying means from a graph, estimating correlations among items (e.g., effort, ability, luck, task difficulty) or among scores (e.g., internal, external) for the purpose of aggregating them into the appropriate dependent measures, estimating *F*s or *t*s from *p* values, relying on nonnumerical statements (e.g., "did not significantly differ," "was greater than"), and estimating standard deviations from error terms or other effects of the design. (In nine cases, the original author was contacted with a request for clarifying information. Eight responded, but only four were able to provide the needed information.) To examine the impact of such uncertainty, I included the number of inferences required for any given effect size acquisition as a coding variable (see below for more details).

Computations of effect sizes from reported information were based on standard sources for meta-analysis (Cooper & Hedges, 1994; Glass, McGaw, & Smith, 1981; Hedges & Olkin, 1985; Hunter & Schmidt, 1990) and on a compilation by DeCoster (2004; see also DeCoster, 2005). Below, I highlight some noteworthy steps in the procedures.

For the proper treatment of within-subject designs (on the actor–observer variable), I followed the guidelines of Dunlap, Cortina, Vaslow, and Burke (1996). First, when computing effect sizes from means and standard deviations, the denominator of Cohen's formula is still the pooled standard deviation (based on s_A and s_O), not the smaller standard deviation of the difference scores (s_{A-O}), which benefits from the within-subject correlation and would lead to an overestimation of *d* (Dunlap et al., 1996). Second, because *F* or *t* values do benefit from this within-subject correlation, effect size calculations based on *F* or *t* values have to be adjusted for the within-subject correlation, here, r_{A*O}.

$$d = t_{paired}\sqrt{2(1 - r_{A*O})/N}$$

(Dunlap et al., 1996, p. 171, Equation 3). (1)

If the r_{A*O} correlation was not reported in the article, it was set to a default of .50. This was the average r_{A*O} across those studies in which the value was reported or was computable (Ashkanasy, 1997; Ender & Bohart, 1974; Franzoi & Sweeney, 1986; Malle & Pearce, 2001; Manusov, Floyd, & Kerssen-Griep, 1997; Wortman, Costanzo, & Witt, 1973). In nine studies, the actor–observer factor was treated as a matched-pairs variable (because actor and observer were nested within pairs). None of the studies reported the relevant within-pairs correlation, so its default value was set to 0.30. The value of 0.30 generated good convergence, where computable, between effect sizes calculated from means and standard deviations (requiring no r_{A*O}) and from *F* or *t* values (requiring r_{A*O}).

Whenever individual (component) scores in a repeated-measures design are combined into aggregate scores, the standard deviation of the aggregate derives from the standard deviations of the components, corrected for the correlation among the component scores. This applies to computing I as an average of ability and effort, computing E as an average of chance and task characteristics, computing I-E as the difference score of I and E, and computing across-valence scores as the average of positive and negative scores. Below is a sample formula.

$$s_{I\text{-}E} = \sqrt{s_I^2 + s_E^2 - 2r_{I*E}s_I s_E}. \quad (2)$$

Where available, reported intercorrelations (e.g., r_{I*E}, $r_{ability*effort}$) were used in these formulas. For all studies in which they were not available, a default value took their place, which was the average value across those studies in which the correlation had been reported or was computable. For r_{I*E}, the default value was −0.10 (from Howard, 1987; Malle & Pearce, 2001; McGill, 1989; Nesdale & Moore, 1984; Taylor & Koivumaki, 1976; Wilson, Levine, Cruz, & Rao, 1997.) For $r_{ability*effort}$ and $r_{chance*task}$, the default value was 0.40 (from Ender & Bohart, 1974; Huber, Podsakoff, & Todor, 1985, 1986.) For $r_{success*failure}$, the default value was 0.20 (from Ashkanasy, 1997; Huber et al., 1986).

Effect size bias correction. The raw effect size *d* is not an unbiased estimator of the population effect size; it slightly overestimates the latter, except in large samples. All extracted raw *d* values were therefore corrected for this bias, using Hunter and Schmidt's (1990, p. 281) approximation of Hedges and Olkin's (1985) formula.

Sampling variance of effect sizes. The variance of each (bias-corrected) *d* was computed separately for within-subject and between-subjects designs. When the actor–observer variable was a within-subject (or within-pairs) factor, the variance formula was based on S. B. Morris (2000, Equation 7). When the actor–observer factor was between-subjects, the variance formula was based on S. B. Morris and DeShon (2002, p. 117).

Reliability of effect size acquisition. To establish the reliability of acquiring relevant parameters and combining them in the correct way, an additional researcher was trained to extract the parameters of interest (see instruction sheet included in the supplementary materials online. After a training and practice phase (on 78 parameters from 10 articles), the two coders independently extracted 80 parameters from 13 articles. The intraclass correlation was 0.97, and the effective difference between coders in the averaged effect size across the 13 articles was 0.05. However, 29 of the specific parameters showed disagreements above the tolerance of 0.05. The disagreements were transparent (e.g., twice incorrect contrast weights were used, three times incorrect *N*s were used) and could be easily remedied through discussion. An additional 118 parameters from 17 articles were analyzed in a third phase, and this time, all values were identical to the fourth decimal. (All effect size computations are available in the supplementary material online.

Moderator Variables

The extant literature and the present data pool of actor–observer studies suggested several moderator variables, and others were added for reliability and exploratory purposes. Table 1 lists all moderator variables, their values, and frequencies. Notably absent are personality traits because too few studies explicitly tested their impact. Also absent is gender because results were rarely broken down by this variable, and when they were, no interactions with actor–observer differences emerged.

Table 1 also lists the source of information for each variable—whether it was directly garnered from the published record or inferred and coded by independent judges. If the latter, reliability is reported in the table as well. What follows is a description of these coded moderator variables. In all cases, two independent judges followed variable-specific coding instructions (available in the supplementary material online), practiced on a small subset of the studies, and discussed their disagreements. Then, each coded the test set, followed by discussion and resolution of any disagreements.

Valence. Two judges coded the valence of explained behaviors or events (except those events that researchers had explicitly defined or manipulated as positive and negative). Positive events referred to a success, an achievement, something to be proud of, something one strongly identified with. Negative events referred to a failure, a socially undesirable behavior, something that threatened one's self-esteem, something to distance oneself from. Cases of mixed-valence events (e.g., averages of within-subject success–failure manipulations) or events of unknown valence were coded as neutral.

Reference to stable traits. Traitlike aspects in internal attributions were coded as a percentage of trait items out of all internal items. A trait code was maximal (100%) when the measure defined or exemplified the internal attribution exclusively as stable characteristics (e.g., personality, attitude, skill, ability, personal qualities). A trait code was minimal (0%) when the measure asked exclusively about transitory factors (e.g., specific reasons or desires, mood, effort). A trait code was mixed ($0\% < x < 100\%$) to the extent that there were some traitlike items among all internal items. For example, Storms's (1973) measure asks, "How important were your (his) personality, traits, character, personal style, attitudes, mood, and so on?" Here, one finds five stable factors and one variable factor (mood), yielding a trait percentage of 83%. All coding was based on short excerpts from the relevant portion of a study's methods section containing no other information about the article or its effect size.

Table 1
Moderator Variables and Numbers of Studies in Each Category

Variable and values	Number of studies/ samples	Reliability
1. Valence of event explained		Agree = 74%, κ = 0.61
Negative	61	
Neutral, mixed, or unknown	67	
Positive	45	
2. Visual perspective switched (actor sees own behavior from observer perspective and vice versa)	6	Published record
3. Involved observers (those who have a special interest in the actor's behavior)	14	Published record
4. Familiarity between actor and observer		Agree = 98%, κ = 0.95
High: romantic partner, parent, good friend	29	
Low: stranger, new acquaintance	143	
Mixed	1	
5. Age groups		Published record
Children: 5–17 years old	19	
College students: 19 years on average	113	
Adults: 20 years and older	37	
6. Standard/nonstandard methodology		Published record
Standard studies (containing no independent variable presumed to increase or decrease the asymmetry; akin to control groups)	48	
Nonstandard studies	127	
Explanations of positive or negative events	106	
Explanations of hypothetical events	24	
Presentation of fictitious base rates	7	
Time-delayed attributions (2 are within-subject)	6	
Non-Western samples	3	
7. Event type		Agree = 88%, κ = 0.75
Behavioral or psychological event (e.g., actions, emotions)	82	
Outcome (e.g., success, failure)	87	
Unknown	4	
8. Intentionality of event		Agree = 81%, κ = 0.70
Unintentional	94	
Mixed, unknown	45	
Intentional	34	
9. Realism of event		Agree = 93%, κ = 0.69
Real to both actor and observer	144	
Hypothetical/imagined to actor and observer	23	
Mixed	6	
10. Time of attribution (for real events)		Agree = 87%, κ = 0.71
Immediately after event occurred	101	
Delayed	43	
11. Source of valence		Published record
Experimentally manipulated	71	
Naturally occurring	102	
12. Attribution assessment format		Published record
Rating scales	152	
Open-ended responses	20	
Mixed	1	
13. Types of rating scales		Published record
Bipolar (I-E) scale only	22	
Unipolar scales I and E	64	
Single-item I and E ratings	44	
Multiple-item I and E ratings	20	
I and E based on ability, effort, luck, and task scales	48	
I and E based on scales with specific content	18	
14. Reference to stable traits in internal attribution measure		
Continuous percentage (range: 0–100%)		ICC = 0.68
15. Degree of inference required for effect size extraction		ICC = 0.93, κ = 0.71
0	84	
1	62	
2	24	
3	3	
16. Study design for actor–observer factor		Agree = 93%, κ = 0.84
Between-subjects	123	
Within-subject	50	
17. Study setting		Published record
Actors and observers complete questionnaires	93	
Observer observes actor	61	
Observer interacts with actor	19	
18. Did actor and observer explain the identical event?		Agree = 84%, κ = 0.69
Identical event	87	
Not identical event	86	
19. Median split by publication year		Published record
First half of studies: 1971–1981	85	
Second half of studies: 1982–2004	88	
20. Citation impact factor (range: 0.10–3.10)		Archival record

Note. The numbers of studies within each variable category are based on the analysis of I-E scores (I and E have smaller cell sizes) and vary because of missing values. I = internal cause score; E = external cause score; I-E = internal–external difference score; ICC = intraclass correlation coefficient.

Inferences required for effect size calculation. The two coders who independently extracted effect sizes for a subset of the studies also counted the number of inferences they had to make to extract those effect sizes. A score of 0 was assigned to all extractions that involved no inference (i.e., means and standard deviations or F or t values were available, and all measurement correlations were based on reported or default values); scores of 1, 2, or 3 were assigned to extractions with the corresponding numbers of inferences.

Additional moderator variables that were coded by pairs of judges included the time when attributions were made (right after the explained

Table 2
Average Effect Sizes (d) for Actor–Observer Asymmetries on Three Attribution Scores

Events explained	σ^2-weighted random effects	95% CI	σ^2-weighted fixed effects	*Q*	*N* (studies)
All studies					
I-E	0.095*	0.032, 0.159	0.032*	809.4**	173
I	0.062	−0.019, 0.143	−0.016	509.8**	113
E	0.023	−0.064, 0.109	0.016	531.2**	107
Standard studies					
I-E	−0.001	−0.105, 0.104	−0.049*	235.1**	48
I	−0.093	−0.194, 0.008	−0.070*	107.9**	37
E	0.007	−0.100, 0.115	−0.015	121.3**	36

Note. CI = confidence interval; I-E = internal minus external attributions; I = internal attributions; E = external attributions.
* Different from 0 at $p < .05$. ** Different from 0 at $p < .001$.

event or later), the degree of familiarity between actor and observer, the realism of explained event, the study design (within- vs. between-subjects), the type of event explained (an outcome of success or failure or a behavioral event), and whether the explained event was the same for actor and observer (see Table 1). All remaining moderator variables were gleaned directly from the published record. Age of participants was reported in most articles, but in some articles, only the label *undergraduate students* was given, in which case I assigned a default age of 19 years (which was the average age of those students whose age had been reported).

Results

Basic Analyses

Effect size estimation. Small samples produce greater variability of effect sizes and hamper precision in estimating the population average. A correction that weights each effect value by its inverse variance (precision; Hedges & Olkin, 1985) is recommended. This precision-weighted effect size can be estimated under either a fixed-effects model or a random-effects model. Fixed-effects models are more intuitively descriptive of the data at hand, but they assume that all studies estimate a single population effect size, an assumption that is unlikely to be true in the face of significant heterogeneity (Hedges & Vevea, 1998; Shadish & Haddock, 1994). The parameter *d* reported here was therefore based on random-effects analyses of precision-weighted effect sizes, and for comparison, the fixed-effects parameter is displayed in summary tables. (All random-effects analyses were conducted with the method of moments, using SPSS macros by David B. Wilson, available at http://mason.gmu.edu/~dwilsonb/ma.html.)

Distributional characteristics. The raw effect sizes in this pool of studies deviated in some respects from normality. Score I-E had both negative skewness [−0.85, −0.12][3] and positive kurtosis [3.19, 4.63]. Score I had slight positive skewness and noticeable positive kurtosis [0.36, 2.12], and Score E had both negative skewness [−1.18, −0.26] and positive kurtosis [1.51, 3.32]. All three scores had a high central peak, indicating a large number of studies at or around the study sample mean, and a substantial standard deviation (0.63 and higher). Given the raw means and standard deviations for I-E, I, and E, there were no more extreme values (outside the 90% area) than would be expected under normal distribution assumptions. However, given the random-effects estimates of means and standard deviations, there were twice as many extreme values as would be expected under normal distribution assumptions.

Central tendency. Measures of central tendency for all three attribution scores (see Table 2) provide scarce evidence for the classic actor–observer hypothesis. Random-effects estimates for single I and E scores were indistinguishable from zero: $\bar{d}$(I) = 0.062 [−0.019, 0.143], $\bar{d}$(E) = 0.023 [−0.064, 0.109]. The I-E difference score differed significantly from 0 but was very small, $\bar{d}$(I-E) = 0.095 [0.032, 0.159]. The values in a fixed-effects model were even smaller, ranging from −0.015 to 0.032. Moreover, raw estimates and sample size-weighted estimates of central tendency led to the very same conclusions. Thus, independent of the method of estimating effect sizes and the specific attribution score used, the classic actor–observer asymmetry was very small or nonexistent. In the units of the correlation coefficient, the effect measure that has defined many discussions in social and personality psychology (e.g., Funder & Ozer, 1983; Mischel, 1968; Ozer, 1985), the actor–observer asymmetry ranged between $r = -0.01$ and $r = .05$.

I now consider three main options of accounting for these findings.

Standard studies. Perhaps the lack of an actor–observer effect and the substantial effect variability is due to a large number of nonstandard studies in this data sample—studies in which researchers tried to isolate factors that account for the asymmetry or factors that are expected to eliminate the asymmetry. Thus, I analyzed only those studies that were conducted under standard conditions—when explained events were not obviously valenced (positive or negative), when the events were real rather than imagined, when no delay between event and explanation had occurred, and when no (false) information about the base rate of the agent's behavior was provided. As Table 2 shows, under these standard conditions, the effect sizes actually dropped to below zero, ranging from $\bar{d} = -0.09$ to $\bar{d} = 0.007$. In this subset of studies, standard deviations were slightly lower, heterogeneity was still very high (only 20%–30% of variability was due to chance alone), and the distributions showed no significant skewness or

[3] Pairs of numbers in parentheses refer to a parameter's lower and upper bounds, respectively, of the 95% confidence interval around the mean.

kurtosis. Thus, without the nonstandard studies, the pool of effect sizes was somewhat more homogeneous, but the actor–observer asymmetry was zero.

Publication bias. Another possibility is that the present sample of published studies, although displaying a small, above-zero effect, is restricted by publication bias and actually reflects a population value of $d = 0$. It is commonly assumed that studies that contradict an established hypothesis or support it only weakly and without significance are less likely to be published than studies that support the established hypothesis (Begg & Berlin, 1988; Coursol & Wagner, 1986; Greenwald, 1975; Hedges, 1984). Inspecting a funnel graph of meta-analyzed data (Light & Pillemer, 1984), which plots effect sizes against sample size or a measure of precision, is a minimal step to examine the potential for publication bias. Figure 1 displays a funnel graph of the present data (effect size against $1/s_e$) and illustrates the typical pattern of greater variability of effect sizes in smaller samples (those with lower $1/s_e$ values). However, the graph also suggests the presence of publication bias, indicated by an asymmetric distribution of data points around the estimated mean (the random-effects $\bar{d}$). On the positive side of the distribution (above $\bar{d}$), more studies are outlying in the extreme range than on the negative side of the distribution, and there appears to be one pocket of studies missing on the negative side of the distribution between $d = -0.8$ and -0.35. These asymmetric areas are marked with ovals in Figure 1.

Various formal procedures to identify publication bias have been proposed. I applied the recent trim-and-fill procedure (Duval & Tweedie, 2000) because it both identifies and corrects for potential bias. The procedure rests on the assumptions that unbiased samples of studies are symmetrically distributed around the true mean effect size and that bias manifests as the scarcity of extreme negative (hypothesis-disconfirming) effect size values. To the extent that more studies lie in the extreme positive range of the distribution of mean-deviated scores than in the extreme negative range, publication bias may be present (although any similar form of data censoring can also contribute to the omission of studies). Applying the formal procedure described by Duval and Tweedie (2000) confirmed the possibility that a greater proportion of extreme effect sizes on the positive side entered the pool of studies. Specifically, the algorithm suggested that 25 studies on the negative side may have been omitted during the publication process (which would correspond to about one a year over the 30-year history of the hypothesis). Estimating the mean effect size after filling back in these presumably omitted studies yielded $\bar{d} = -0.011$ [−0.077, 0.054], indistinguishable from 0.

Heterogeneity due to sampling error. If no actor–observer asymmetry exists in the population, then the present study pool may contain only random variation around this zero population value. However, the variance of effect sizes in the present study sample exceeded the variance expected from sampling error alone.

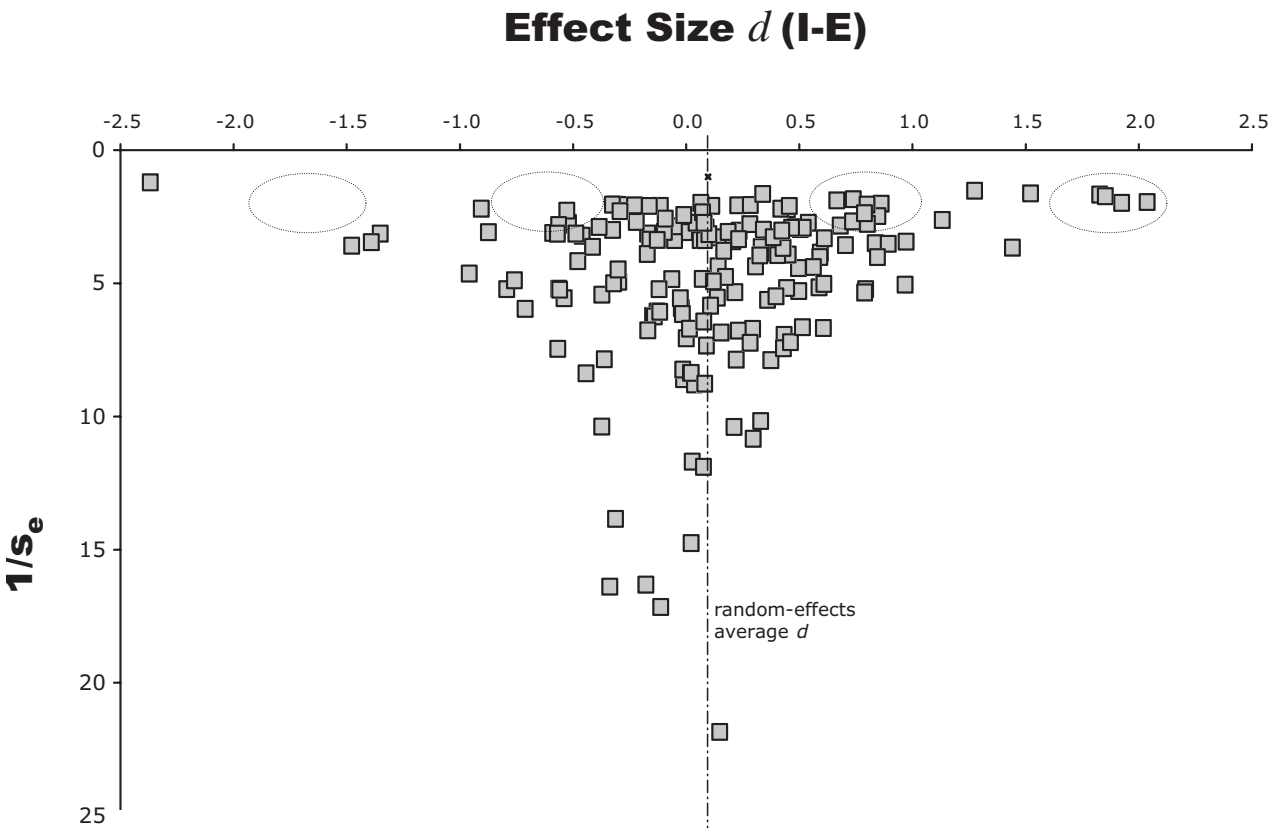

Figure 1. Funnel plot between each raw effect size (I-E) and its standard error ($N = 173$ studies). I-E = internal–external difference score.

The portion of observed variance due to chance, expressed as I^2 (Higgins, Thompson, Deeks, & Altman, 2003), is 21% for I-E and 22% and 23% for I and E, respectively. These values are far lower than the suggested minimal value of 75% that is required for considering an effect size distribution homogeneous (Hunter & Schmidt, 1990). The trim-and-fill analysis also did not account for much of the between-study variability. Heterogeneity remained substantial, with an I^2 value of 19%, $Q(197) = 1{,}054.8$, $p < .001$. An approach that accounts for this heterogeneity is therefore still needed.

The most differentiated analysis of between-study variability in the present data pool is a moderator analysis. It can test hypotheses about the specific conditions that facilitate or hinder the actor–observer asymmetry even if the overall effect size average is indistinguishable from zero.

Moderator Analysis

Valence as moderator. The first moderator of interest is the valence of the explained event, and it constitutes H_2, the claim of a self-serving bias. As a first step, I selected those 25 studies in which positive and negative conditions (primarily success vs. failure outcomes) were directly compared within subjects. The results show that there is an actor–observer asymmetry when people explain negative events, $\bar{d} = 0.231$ [0.062, 0.400], but no asymmetry when these same people explain positive events, $\bar{d} = 0.026$ [−0.102, 0.154]. However, the asymmetry for negative events did not replicate in the fixed-effects analysis, $\bar{d} = 0.031$ [−0.022, 0.084], suggesting that the asymmetry emerges only when the substantial between-study heterogeneity (88%) is taken into account.

Two concerns apply to treating valence as a within-subject factor. First, within-subject designs may not be representative of real-life judgments, as people rarely explain both a failure and a success at the same time. Indeed, almost half of the within-subject studies asked people to explain hypothetical events, compared with 9.5% among the remaining articles. Second, there is strong evidence for a pervasive negativity bias in social judgment and self-regulation (Skowronski & Carlston, 1989; Taylor, 1991), which leads people to attend to and be more influenced by negative stimuli than positive stimuli. Attributions may thus be unduly amplified for the negative case.

Perhaps a better test of the valence hypothesis could be conducted across studies, contrasting those studies (or between-subjects conditions) that examined explanations for positive events with those that examined explanations for negative events. All studies were classified for valence on the basis of the study authors' own categorizations or, where not available, on the basis of two coders' classifications (described earlier). Sixty-one studies had examined negative events (e.g., failures, delinquent behavior), 45 had examined positive events (e.g., success, moral behavior), and the remaining 67 were of neutral, mixed, or unknown valence. These three groups of studies differed substantially in their I-E scores, $Q(2, 170) = 20.7$, $p < .001$. In particular, attributions for negative events yielded the expected actor–observer asymmetry, $\bar{d} = 0.243$ [0.135, 0.350], whereas positive events showed the reverse pattern, $\bar{d} = -.149$ [−0.280, −0.019]. (See Table 3.)

Figure 2 shows the distinct distributions of positive and negative event explanations. The graph also serves as a funnel plot, and it appears that both distributions are asymmetric. Applying the trim-and-fill method to the pool of studies featuring negative events suggested that 15 studies might be missing on the left side of the distribution (negative effect sizes), and the adjusted estimate after fill-in would be $\bar{d} = 0.110$. For positive events, the procedure suggested that 8 studies might be missing on the right side of the distribution (strongly positive effect sizes), and the adjusted estimate after fill-in would be $\bar{d} = 0.01$. However, the assumption that these strongly confirmatory data points were excluded from the published literature suggests an exclusion process opposite to that operating in most instances of data censoring.

Several additional points are worth noting. First, as Table 3 illustrates, the asymmetry for negative events held for internal attributions ($\bar{d} = 0.304$), but not for external attributions ($\bar{d} = -0.014$), whereas the (reverse) asymmetry for positive events was similar (and small) for internal attributions ($\bar{d} = -0.106$) and for external attributions ($\bar{d} = -0.129$).

Second, the valence-dependent asymmetries became even more marked when I excluded three studies with East Asian participants who all showed a reversal of the Western self-serving bias (Hol-

Table 3
Effect Sizes (d) of Actor–Observer Differences in Studies With Clearly Defined Valence of Event Explained

Events explained	σ^2-weighted random effects	95% CI	σ^2-weighted fixed effects	Q	N (studies)
Positive					
I-E	−0.158*	−0.315, −0.002	−0.148*	183.4**	45
I	−0.140	−0.327, 0.046	−0.071	109.5**	29
E	−0.134	−0.380, 0.111	−0.142*	149.6**	27
Negative					
I-E	0.241*	0.135, 0.347	0.106*	244.7**	61
I	0.311*	0.169, 0.469	0.166*	152.3**	35
E	−0.020	−0.164, 0.123	−0.071*	115.8**	32

Note. CI = confidence interval; I-E = internal minus external attributions; I = internal attributions; E = external attributions.
* Different from 0 at $p < .05$. ** Different from 0 at $p < .001$.

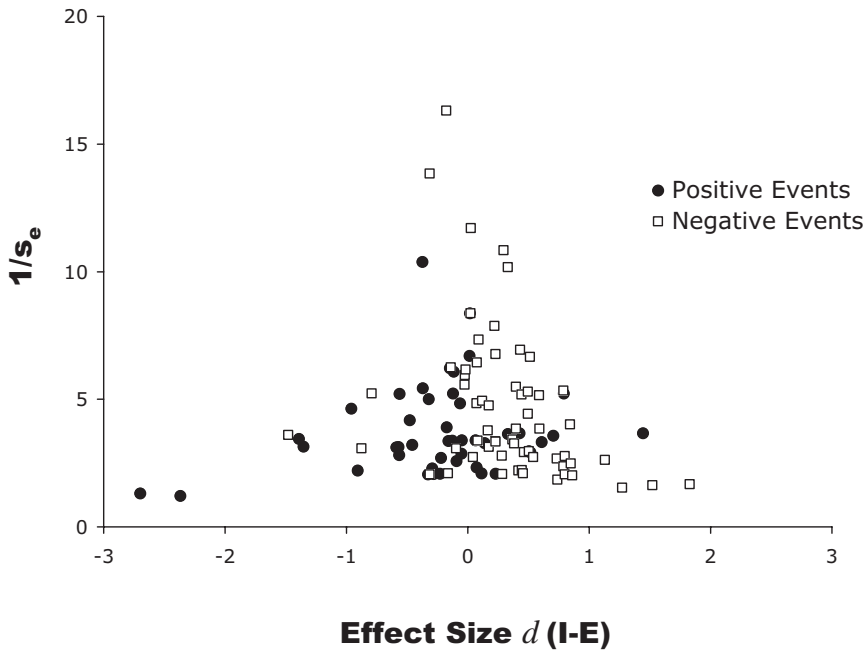

Figure 2. Relationship between effect size (I-E) and inverse standard error separately for explanations of positive and negative events. I-E = internal–external difference score.

loway, Kashiwagi, Hess, & Azuuma, 1986; Yamauchi, 1988). Among Western participants only, the average asymmetry for negative events was $\bar{d} = 0.270$ [0.168, 0.372], and the average reverse asymmetry for positive events was $\bar{d} = -0.193$ [−0.338, −0.048].

Third and perhaps most striking, the valence effect held for the 124 studies in which the actor–observer variable was manipulated between subjects—for negative events, $\bar{d} = 0.364$; for positive events, $\bar{d} = -0.240$ ($\eta^2 = 20\%$)—but it did not hold at all for the 50 studies in which the actor–observer factor was manipulated within subjects ($Q = 1.6$, $\eta^2 = 2\%$).

Combining these qualifications, I conclude that the classic pattern of actor–observer differences (observers' I-E is greater than actors' I-E) may hold for Western participants when the actor–observer variable is manipulated between-subjects and internal attributions of negative events are examined. The potential of considerable publication bias limits the reliability of this conclusion.

Independent of these qualifications, the overall valence effect undermines the validity of the earlier test of the actor–observer hypothesis on central tendency measures across all studies. The classic hypothesis predicts an actor–observer difference across valence (a main effect), but computing an appropriate meta-analytic average across studies that vary in valence requires that there is an equal number of studies in which people explain negative and positive events. This was not the case in the present pool of studies. Among those for which I had reliable valence information, 61 studies could be clearly identified as examining negative events and 45 as examining positive events, thus favoring negative valence studies in the meta-analytic average. When I corrected for this preponderance of negative studies by means of an unweighted average across the 106 studies, the actor–observer difference amounted to $\bar{d} = 0.042$.

Visual perspective as moderator. According to Jones and Nisbett (1971), the actor–observer asymmetry in attribution is largely due to differences in visual perspective. The observer's visual attention is directed to the actor, whereas the actor's visual attention is direction to the situation (see also Heider, 1958). Switching actors' and observers' visual perspective should therefore eliminate the actor–observer asymmetry. Such a switch can be achieved by showing actors a videotape of themselves acting in the setting and/or showing the observer a videotape filmed from the actor's perspective (e.g., with a camera positioned right behind the actor's shoulder). Six studies (in five articles) tested the hypothesis of perspective switching as a moderator of the actor–observer asymmetry (Arkin & Duval, 1975; Martin & Huang, 1984; Sherrod & Farber, 1975; Storms, 1973; Uleman, Miller, Henken, Riley, & Tsemberis, 1981). In the initial test of this hypothesis, Storms (1973) found both the standard effect ($d = 0.482$) and the predicted reversal in the perspective-switched condition ($d = -0.636$). However, five subsequent studies between 1975 and 1984 averaged a reverse asymmetry in the control condition (raw $\bar{d} = -0.229$) and a very similar value after perspective switching (raw $\bar{d} = -0.289$). In a between-conditions analysis of all studies (a total of six control and six switched conditions), I found a slightly reversed asymmetry in the control condition, $\bar{d} = -0.077$ [−0.385, 0.065], and a reversal in the perspective-switched condition, $\bar{d} = -0.280$ [−0.509, −0.051]. Given that no asymmetry occurred in the control condition, it remains unclear what the switched perspective achieved in this experimental setting. One possibility is that the actors' unusual experience of seeing themselves on video created a demand to respond differently in some way and there was more room to change internal attributions than external attributions. Another possibility is that perspective switching per se is inert and merely creates a time delay between the event and the attributions. This account is supported by one study

that found no difference between participants who watched a video that actually switched their perspective and participants who merely watched an irrelevant music video, with both conditions causing equal time delay (Martin & Huang, 1984). So, time delay was the next moderator variable to consider.

Time delay as moderator. Six studies (in five articles) compared directly whether a time delay between the event and people's attributions made a difference (Burger, 1986; Funder & Van Ness, 1983; Martin & Huang, 1984; D. T. Miller & Porter, 1980; Moore, Sherrod, Liu, & Underwood, 1979). In a between-conditions analysis, the average effect size at Time 1 was $\bar{d} = 0.064$ [−0.2347, 0.3618] and at Time 2 was $\bar{d} = -0.007$ [−0.287, 0.289], with no significant difference between the two ($p > .50$). Thus, in studies that contained commensurate conditions, there was no sign of an actor–observer asymmetry, and time delay did not change this fact. A second analysis examined time delay in the entire pool—comparing studies in which participants provided explanations immediately after the behavior or event ($N = 101$) with studies in which they provided explanations after some delay ($N = 45$). Again, no reliable difference emerged ($p > .50$), with $\bar{d} = 0.068$ for immediate attributions and $\bar{d} = 0.101$ for delayed attributions.

Observer involvement as moderator. Another potential moderator discussed by Jones and Nisbett (1971) is that of involved or active observers. In the present study sample, 12 studies (in seven articles) permitted a comparison of actor–observer asymmetries for these two types of observers (Chen & Yates, 1990; Chen, Yates, & McGinnies, 1988; McAllister, 1996; D. T. Miller & Norman, 1975; Nesdale & Moore, 1984; M. R. Wolfson & Salancik, 1977; Wright, 1980). Across these studies, there was no difference between standard observers, $\bar{d} = 0.034$ [−0.350, 0.418], and involved observers, $\bar{d} = 0.103$ [−0.259, 0.465].

I should mention one pattern that is itself highly tenuous but does suggest follow-up research. Within each valence group (positive, negative), standard observers showed a strong asymmetry that was neutralized within active observers. That is, for negative events (six studies total), standard observers showed a substantial asymmetry ($\bar{d} = 0.713$), whereas involved observers showed much less of it ($\bar{d} = 0.259$). For positive events (four studies total), standard observers showed a substantial reverse asymmetry ($\bar{d} = -0.990$), whereas involved observers showed no such asymmetry ($\bar{d} = 0.012$). One should have limited confidence in this pattern because it was almost exclusively driven by extreme effect sizes (−2.70 to 1.83) in two studies (Chen & Yates, 1990; Chen et al., 1988). Moreover, some studies created involved observers who had reason to be more charitable toward the actor (because they were soon to take the actor's place), whereas other studies created involved observers who had reason to be more critical of the actor (because the actor's performance reflected on them). So, the above neutralizing effect may have been the result of an averaging across importantly distinct conditions. (With the small number of studies, no analysis breaking down these groups was possible.) In light of the suggestive data, however, future studies should examine behavior or outcome explanations in designs that cross valence with the passive/involved observer condition and with the observer's motivational stakes.

Manipulated base rates as moderator. Eight studies (in three articles) presented participants with (fictitious) base-rate information to create the impression that the actor differed substantially from other people within the same situation (Hansen & Stonner, 1978; W. G. Stephan, 1975; G. L. Wells et al., 1977). For example, in one study, both actors and observers were told that the amount of a so-called neutralizing solution the actor had drunk was much larger (actual $M = 88$ ml) than the amount that 15 previous participants had drunk (alleged $M = 12$ ml). Manipulations like this resulted in a strong actor–observer asymmetry in these eight studies, $\bar{d} = 0.797$ [0.387, 1.207], compared with all other studies, $\bar{d} = 0.078$ [0.039, 0.146], $Q(1, 171) = 11.5$, $p < .01$. However, three extreme values (*d*s of 1.925, 2.037, and 1.907) drove this mean, and they all stemmed from one article (Hansen & Stonner, 1978). The other four studies averaged an effect size of only $\bar{d} = 0.219$. So, the moderating effect of manipulated base rates appears to be real, but its actual size is difficult to determine with so few and highly variable data points.

Intimacy as moderator. Jones and Nisbett (1971) and other authors postulated intimacy to be a moderator of actor–observer differences such that intimates should show a smaller actor–observer asymmetry than strangers. To test this hypothesis, coders examined all studies' method sections to classify the relationship between actors and observers as either intimate (parent–child, close friends, romantic partners; $N = 29$) or nonintimate (strangers, acquaintances; $N = 143$). Against predictions, intimates showed a stronger actor–observer asymmetry, $\bar{d} = 0.245$ [0.098, 0.392], than nonintimates did, $\bar{d} = 0.072$ [−0.014, 0.128], $Q(1, 169) = 5.1$, $p < .05$. It is interesting to note that this effect held only for external attributions ($\bar{d} = 0.212$ vs. -0.014), not for internal attributions ($\bar{d} = 0.091$ vs. 0.056).

Age as moderator. A first analysis compared three age groups: the large group of undergraduate students (aged around 19 years), younger participants, and older participants. Nineteen studies reported results from participants younger than college age, ranging from 5 to 17 years, and their average actor–observer difference was $\bar{d} = 0.171$. An additional 37 studies reported results from participants older than college age, ranging from 20 to 45 years. The average effect of this adult group was $\bar{d} = 0.146$. These averages did not differ reliably from the college group's average ($\bar{d} = 0.079$, $p > .50$). An analysis of age as a continuous variable also did not reveal a relationship.

Attribution ratings versus codings. Attributions were assessed either with rating scales (152 studies) or by means of content coding of verbal explanations (20 studies). Whereas an actor–observer difference appeared with the coding approach, $\bar{d} = 0.318$ [0.137, 0.500], none did with the rating approach, $\bar{d} = 0.062$ [−0.004, 0.129], $Q(1, 170) = 6.7$, $p < .01$. Supporting this average difference, one researcher (Burger, 1986) used both rating and coding methods in the same study and found a greater effect size for the coded verbal explanations ($d = 0.198$) than for the ratings ($d = -0.265$). (No qualifications of this pattern emerged in a finer grained analysis of different types of rating scales, such as unipolar vs. bipolar or single items vs. multiple items assessing internal–external attributions.)

Realism. In 149 studies, people explained real behaviors or outcomes; in 24 studies, they were to imagine behaviors or outcomes and explain these hypothetical events. Studies with hypothetical events yielded a noteworthy effect size on I-E, $\bar{d} = 0.280$ [0.122, 0.438], whereas studies with real events did not, $\bar{d} = 0.059$ [−0.097, 0.129], $Q(171) = 6.3$, $p < .01$. Intriguingly, the difference between real and hypothetical events interacted with valence.

When explaining hypothetical events, people showed a consistent asymmetry across event valence ($\bar{d}$s = 0.214 and 0.175), but when explaining real events, people showed an actor–observer asymmetry for negative events ($\bar{d}$ = 0.250) and a reverse asymmetry for positive events ($\bar{d}$ = −0.171). Thus, the unusual cell (compared with the pattern in the entire pool) is the one in which people explain hypothetical positive events.

Dispositions versus other person factors. The actor–observer hypothesis is sometimes formulated as a contrast between dispositional (trait) and situation attributions, sometimes as one between person and situation attributions. Dispositions are just one type of person cause, so the person–situation formulation is somewhat broader in scope. In the present study pool, these two versions of the hypothesis appeared to be interchangeable, however, as the degree of dispositionality of internal attributions showed no reliable relationship with effect sizes: For I-E, $r(172) = .050$; for I, $r(113) = .066$; and for E, $r(106) = -.042$. A correlation test may have been inappropriate, however, because dispositionality was unevenly distributed: Ninety-nine studies measured internal attributions as an even mix between traits and nontraits; in 12 studies, nontraits dominated; and in the remaining 61 studies, traits dominated. These three groups, however, did not differ from each other. For nontrait dominance, $\bar{d}$ = −0.054 [−0.275, 0.168]; for an even mix, $\bar{d}$ = 0.127 [0.043, 0.211]; and for trait dominance, $\bar{d}$ = 0.098 [−0.011, 0.207].

A related test of the difference between dispositional and nondispositional internal causes was possible for studies that relied on Heider's (1958) and Frieze and Weiner's (1971) framework of outcome attribution, which asks people to attribute outcomes to ability, effort, task characteristics, and chance. In this framework, ability refers to a stable (dispositional) internal cause, whereas effort refers to an unstable (nondispositional) internal cause. Here, the asymmetry for ability was slightly larger ($\bar{d}$ = 0.159) than that for effort ($\bar{d}$ = 0.042), $z = 2.06$, $p < .05$. So, whereas dispositions in general do not appear to strengthen the actor–observer asymmetry, the ability dimension in outcome attributions does amplify it to a small extent.

Inferences needed for effect size extraction. A technical variable in the present analysis tracked the varying number of inferences (0–3) that had to be made when extracting or reconstructing each study's effect size. There were no differences in average effect size between these levels of inference ($p > .40$). The vast majority of studies ($N = 146$) required zero or one inference; their combined effect size was $\bar{d}$ = 0.092 [0.024, 0.161]. The 27 studies with two or three inferences did not differ from this average, $\bar{d}$ = 0.114 [−0.058, 0.287], $p > .50$.

Design. The design variable (actor–observer perspective assessed within subject or between subjects) by itself produced a trend of a moderator effect. For I scores, between-subjects designs yielded significantly larger asymmetries, $\bar{d}$ = 0.129 [0.031, 0.227], than within-subject designs did, $\bar{d}$ = −0.076 [−0.217, 0.065]. A similar pattern existed for the I-E parameter ($\bar{d}$ = 0.129 vs. 0.038), but it was not as reliable ($p = .18$). These are the main effects for design; the far more powerful involvement of design lies in its interaction with valence. As mentioned earlier, valence effects held only when the actor–observer variable was manipulated between subjects ($\eta^2 = 20\%$), not when it was assessed within subjects ($\eta^2 = 2\%$). Design did not interact with other moderators in this way.

Identical event explained. A final methodological variable captures whether the event being explained is identical for actor and observer. Actor and observer can either explain literally the same event—the actor's particular outcome or behavior that both attend to—or they can explain different events. Perhaps counterintuitively, when actors and observers explained the identical event, there was a modest but significant asymmetry on I-E, $\bar{d}$ = 0.188 [0.048, 0.281]; when they explained different events, there was no asymmetry, $\bar{d}$ = 0.018 [−0.068, 0.103], $Q(1, 171) = 7.0$, $p < .01$. The same patterns held for I and E separately as well.

Assessment context (questionnaire vs. interaction), outcome manipulation (yes vs. no), intentionality, and event type (outcome vs. behavior/internal state) showed no moderator effects. The last variables to consider were the year and outlet of publication.

Publication outlet as moderator. An interesting question is why the actor–observer asymmetry has been widely accepted despite, as one can see now, very limited empirical support. One possibility is that the scientific community has been exposed to a biased sample of research results. For example, if more widely circulated and frequently cited journals happen to publish more hypothesis-confirming studies, then the research community may be more likely to infer that a hypothesis is generally supported. To test this hypothesis, I garnered impact factors for each journal that published an actor–observer asymmetry study in the present pool (source: *Journal Citation Reports* from the Institute of Scientific Information/Thomson Scientific). This factor indicates the citation rate of an average article from a given journal. I assigned each journal-level impact factor to all articles published in the corresponding journal and, where applicable, to the multiple studies or subsamples contained in the article. If possible, I selected the impact factor assessed in the year in which the pertinent article was published, but in many cases, no impact factors older than 1995 could be located. Six journals did not appear to have an impact factor and showed very low citation rates in the Social Science Citation Index (now Web of Science). These journals (*Acta Psychologica Taiwanica, Compartement, Journal of Sport Behavior, Replications in Social Psychology, Representative Research in Social Psychology,* and *Revista de Psicologia General y Aplicada*) were assigned an impact factor of 0.100. (Excluding them did not alter the results.)

Aggregating studies within journals and correlating effect size per journal with citation impact led to a nonsignificant correlation of $r(49) = 0.08$, *ns*. Likewise, aggregating studies within articles led to a correlation of $r(113) = 0.10$, *ns.* In a random-effects regression of each study's effect size on its journal's impact factor, there was a marginal but small effect. The regression weight of $b = .05$ ($p = .08$) indicated that with every full unit of citation impact (present range: 0.10–3.10), effect size increased by d = 0.05. Moreover, this small effect was driven by the data from the *Journal of Personality and Social Psychology,* $\bar{d}$ = 0.214 [0.456, 0.383], and this journal's elevated effect size relied itself on three extreme data points from one article that manipulated base-rate information (Hansen & Stonner, 1978). After removal of this article, the regression weight for impact predicting effect size dropped further to $b = 0.04$, $p < .20$. Thus, there does not seem

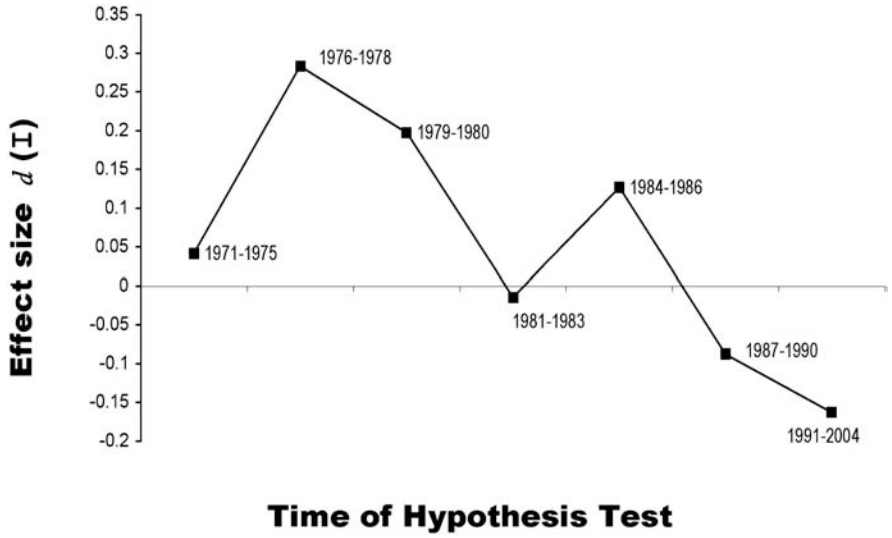

Figure 3. The actor–observer asymmetry on d(I) as a function of time of hypothesis test. Shown are seven roughly equal-sized subsets of studies (Ns = 15–19), accumulated over the years. I = internal cause score.

to be a relationship between citation impact and effect size, but if there is one, it is very small.

Year of publication as moderator. A historical trend may also explain the continued acceptance of the actor–observer hypothesis despite evidence to the contrary. Perhaps the hypothesis was supported by initial studies and later failed to gain support, but the field formed its collective opinion on the basis of the early studies. Overall, year of publication showed a small correlation with values of I-E, $r(173) = -.13$, $p = .09$; a somewhat larger one with values of I, $r(113) = -.20$, $p < .05$; and a somewhat smaller one with values of E, $r(107) = -.09$, $p > .30$. This slight trend can be described as a decrease in I-E effect sizes from the first half of research studies (1971–1981), $\bar{d} = 0.155$ [0.065, 0.244], to the second half (1982–2004), $\bar{d} = 0.037$ [−0.052, 0.126], $Q(1, 171) = 3.3$, $p < .07$. The effect showed more strongly in I scores, which dropped from 0.156 to −0.026, $Q(1, 111) = 4.8$, $p < .03$, whereas E scores dropped from 0.085 to −0.037. Figure 3 displays I scores as a function of time of hypothesis test and shows the decrease of effect sizes from about 1981 on. (More fine-grained analyses that broke the year variable into three, five, or seven groups showed the same pattern but with increasing variability.)

When correlating time of hypothesis test with other variables, one can get a sense of what changed over the years that may have contributed to the slight decline in effect size. Noteworthy changes include a lesser use of base-rate manipulations ($r = -0.18$, $p < .05$) and fewer settings in which actors and observers explained literally the same event ($r = -0.18$, $p < .05$). Controlling for these variables, however, did not eliminate the modest correlation between I scores and year of hypothesis test ($r = -0.19$, $p < .06$). So, it remains a viable hypothesis that with the passage of time and as a result of author decisions as well as reviewer responses, more disconfirming studies found their way into the journals.

Simultaneous regression analyses. To examine the relationship among multiple moderators, I followed Glass et al.'s (1981) recommendation to simultaneously regress effect sizes on moderator variables. Specifically, I conducted a multiple regression with precision-weighted random-effects estimation that corrected standard errors for the random-effects variance component. An interaction term for Design × Valence was added to the main effects for valence and design because the valence-dependent actor–observer asymmetry had emerged only for between-subject designs.

The initial R for predicting I-E scores from 20 moderators (N = 163) was 0.52. Four predictors stood out with significant regression weights: Valence × Design (in between-subjects designs, negative events have larger effect sizes), manipulated base-rate information (when present, effect size increases), realism (hypothetical events show larger effect sizes), and the publication outlet's citation impact (larger effects with higher impact factor). After iterated backward elimination, the final R was 0.51 (N = 171, $p < .001$). As Table 4 shows, the three main predictors were Valence × Design (b = –.24), manipulated base-rate information (b = .71), and realism (b = –.29). In addition, four predictors emerged with somewhat weaker contributions: coding versus rating assessment (coded explanations had larger effect sizes, b = .15), intimacy (the asymmetry held only for close others, b = .14),[4] event identity (effect size was larger when actors and observers explained the identical event, b = .12), and citation impact (effect size was larger with increasing impact factor, b = .07). Compared with the first three predictors, these last four were somewhat less stable when other predictors were entered into or removed from the model.

To assess the influence of extreme values (positive or negative) on the regression result, I first removed the two studies with d values of less than −1.5 and reran the analysis using the seven

[4] Assessment and intimacy shared considerable variance and competed with one another in the multiple regression. When one of the two was left out, the other became stronger and significant; when both were included, their shared variance increased R, but they each slipped just below traditional significance.

Table 4
Predictors Remaining After Backward Elimination in Multiple Regression of I-E Score on All Moderator Variables (N = 171)

Predictor	*b*	95% CI	*Z*
Valence × Between-Subjects Design	−.24	−.34, −.15	−5.42*
Base-rate manipulation	.71	.31, 1.11	3.94*
Real event	−.29	−.46, −.13	−2.99*
High familiarity	.14	−.03, .30	1.66
Coding vs. rating assessment	.12	−.04, .34	1.56
Identical event explained	.12	.00, .24	2.02*
Citation impact	.07	.01, .13	2.30*

Note. CI = confidence interval.
* $p < .05$.

substantial predictor variables. The multiple regression results were unchanged. I then removed the five studies with *d* values greater than 1.5 and reran the analysis. This time, there was one notable change: The base-rate effect dropped to $b = 0.20$, $p > .40$. This drop was entirely due to the removal of one article with three studies that had effects of $d = 1.85$, 1.92, and 2.04 in the manipulated base-rate condition; without this article, manipulated base rate lost a good deal of its predictive power. In this truncated analysis, citation impact also lost some predictive power ($b = 0.05$, $p = .08$), whereas all other moderator variables slightly increased their *b* coefficients.

The simultaneous analysis of moderator variables should be treated with caution because some moderators cannot be completely crossed (e.g., explanations of identical events cannot be achieved in within-subject designs) and some of the predictors are conceptually or empirically related to each other (explanations among intimates were more often subjected to verbal coding procedures). By and large, however, the multivariate regression confirmed the results of individual analyses. Although there is no reliable actor–observer asymmetry across events (in the regression analysis, $\bar{d} = 0.087$), under certain conditions, a moderate actor–observer asymmetry can be found: when people explain negative events (but only in between-subjects designs), when they are presented with (fictitious) base-rate information about the actor's behavior, or when the explained events are hypothetical. Additional conditions that produced small increases in the asymmetry include the coding of people's verbal explanations, intimacy among actors and observers, actors and observers explaining the identical event, and a higher impact factor of publication outlet.

Discussion

This meta-analysis has examined the classic actor–observer asymmetry in attribution first formulated by Jones and Nisbett (1971). The analyses were guided by four hypotheses that are widely cited in the psychological literature. H_1, featuring I-E scores, is the most direct test of the actor–observer asymmetry: Observers are expected to show relatively more internal versus external attributions than actors do. Across 173 studies, the average asymmetry was $\bar{d} = 0.095$ in a random-effects model and $\bar{d} = 0.032$ in a fixed-effects model. These averages were significantly different from zero, but 68 studies had effect sizes at or below $d = 0.0$, and 97 studies failed to reach the threshold of small effects: $d = 0.20$ (Cohen, 1988).

H_{1A} and H_{1B} test the classic actor–observer asymmetry separately for the component scores of internal attributions (I) and external attributions (E). The asymmetry on I scores was $\bar{d} = 0.062$ in a random-effects model and $\bar{d} = -0.016$ in a fixed-effects model; the asymmetry on E scores was $\bar{d} = 0.023$ in a random-effects model and $\bar{d} = 0.016$ in a fixed-effects model. None of these effect averages were statistically different from zero.

H_2 proposes a self-serving bias in attributions according to which, for negative events, observers offer a greater preponderance of internal over external attributions than actors do, but for positive events, it is actors who show a greater internal–external preponderance than observers do. This hypothesis received support, albeit with small effects. For negative events, effect sizes for I-E amounted to $\bar{d} = 0.241$ in a random-effects model and $\bar{d} = 0.106$ in a fixed-effects model, whereas for positive events, effect sizes amounted to $\bar{d} = -0.158$ in a random-effects model and $\bar{d} = -0.148$ in a fixed-effects model.

The actor–observer hypothesis, if it is actually distinct from the self-serving hypothesis, must hold across valence. In light of valence-dependent actor–observer differences and a preponderance of studies with explanations of negative events, adjusted mean effect sizes must be computed that correct for the influence of valence. These adjusted averages for the actor–observer asymmetry were $\bar{d} = 0.042$ and $\bar{d} = -0.021$ in a random-effects model and a fixed-effects model, respectively.

I must conclude that after 35 years of research and more than 170 studies, the classic actor–observer hypothesis is—counter to what textbook descriptions and commonly held beliefs suggest—neither firmly established nor robust and general and that evidence for it is neither pervasive nor plentiful (Aronson, 2002; Fiske & Taylor, 1991; Jones, 1976; Watson, 1982). The actor–observer hypothesis appears to be a widely held yet false belief. Nonetheless, there are a few conditions under which the effect (or its opposite) seems to hold.

Moderators

Even though the effect sizes for the actor–observer asymmetry were close to zero, variation around the average was greater than mere chance would suggest. This pattern required an exploration of moderator variables to reveal some of the psychological processes that make the asymmetry vary in one direction or another. Out of 20 variables, 3 showed strong moderating effects, and 4 additional ones showed smaller effects.

Valence. The most important moderator is valence, both because it replicated across a large number of studies and because it suggests that one cannot speak of a general actor–observer asymmetry, only of a tendency toward self-servingness.

However, the interpretation of this valence-dependent pattern as indicating self-servingness may be problematic. What is known is that when actors explain failures, mishaps, and socially undesirable behaviors, they are less willing than observers to cite internal causes (there is no difference for external causes; see Table 3). Actors, more so than observers, avoid attributing relapses in drinking, aggressive impulses, failures on tests, and problems in school to their own stable dispositions or to their intentions, thoughts, and desires. This pattern might indicate self-servingness, harsh observer judgments, or simply responses to an unusual situation. The literature contains valuable discussions of the first two accounts

(e.g., Ackermann & DeRubeis, 1991); I explore here the third, perhaps more controversial idea—that actors who appear to give self-serving attributions may actually respond in normatively defensible ways.

Many studies in which people explained negative events confronted participants with an unusual event, such as an extramarital affair, a relapse in one's rehabilitation, giving in to an opportunity to cheat, or (in almost half of the cases) a failure experience created by means of false feedback. Unusual events are more likely to contradict an actor's knowledge base than an observer's. If Audrey generally does well on creativity tests but learns that she did worse than most of her peers, it is not self-serving but, rather, normative to go with the base rates and assume that this particular outcome was a fluke caused by local, temporary factors (cf. Swann & Read, 1981). If the feedback in an experiment is sufficiently negative so that more than half of the people normally do better than the feedback indicates, the average shift of actors' attribution ratings toward the situational end need not imply self-servingness. Observers, by contrast, have no base-rate knowledge that would contradict the (false) information they receive. They have an *N* of 1 (Dawes, 1990) that indicates the actor did badly, and when they are pressed for an explanation, observers may be justified in attributing the event at least partially to internal factors.

For positive outcomes, the argument from normativeness applies as well if people in general have positive outcomes and therefore see the experimental outcome as confirming this past trend. Observers, by contrast, may be less willing to use their *N* of 1 when dealing with positive outcomes because such outcomes are generally considered less diagnostic of the actor's true characteristics (Reeder & Brewer, 1979; Skowronski & Carlston, 1989). The result is a reverse actor–observer asymmetry.

Another important fact is that the opposite actor–observer differences for positive versus negative events emerged only for between-subjects manipulations of perspective (one person assigned as actor, the other as observer), not for within-subject assessments (one person judging both self and other). The precise psychological mechanism at work here is unknown. Between-subjects designs may profit from the observer's scrutiny of a single person's outcome or behavior—a situation in which the greater diagnosticity of negative over positive outcomes may be amplified. Within-subject designs, by contrast, may invite perspective taking and activate norms of modesty and fairness. Moreover, the observer in a within-subject design need not rely merely on an *N* of 1 but has a trustworthy data point available: his or her own case (in the actor role). If the person considers relevant base rates when explaining the event from the actor perspective, these base rates are available for explaining the same behavior from the observer perspective as well.

Manipulated base rates. The second influential moderator is the manipulation of base-rate information—that is, the experimenter's demand to compare an actor's behavior or outcome with a reference group's (fictitious) behavior or outcome, whereby these base rates make the actor's behavior look unusual and distinct. One article (Hansen & Stonner, 1978) yielded the single highest effect size of $d = 2.037$, but none of the other three base-rate articles approached this extreme value, averaging $\bar{d} = 0.263$. Without the studies in the former article, the base-rate effect is no longer reliable, in both univariate and multivariate analyses.

Nonetheless, there is an apparent potential for a strong actor–observer asymmetry, so how it operates needs to be understood. To this end, it is instructive to look at one study in more detail. Hansen and Stonner (1978, Study 1) asked actor subjects to perform a particular behavior (drinking as much of a liquid as they liked) and then (incorrectly) informed both actor and observer subjects that the actor differed from the previous 15 subjects in the amount he or she drank (less or more) by a factor of 3 to 4. Actors and observers then explained the amount the actor drank by indicating to what extent the drinking may have been due to the actor's thirst (internal attribution) or due to the solution's taste (external attribution). Faced with these two options, one could expect actors and observers to follow very different lines of thinking. For actors, the thirst explanation was likely to be dismissed on experiential grounds unless they happened to be particularly thirsty at the time. What was left for them as a plausible explanation was some stimulus characteristic. Observers, by contrast, had no countervailing evidence to dismiss the thirst explanation but had reason to dismiss the situation explanation because, after all, the 15 other participants in the experiment tasted the same solution and showed a very different response from the target person. Given the information provided and given participants' background knowledge, the differential responses seem quite understandable.

Less clear, however, is whether this pattern has much to do with actor–observer differences. What appears to have been driving the effect is the particular information presented or available to actors and observers. Suppose it had been different. Suppose the actors explained a similarly unusual imbibing from many years ago but did not recall the details of the event. In this case, the explanation that they were thirsty at the time (internal attribution) would be quite plausible to them. Conversely, if observers had evidence suggesting that the actor was not particularly thirsty (e.g., the person had refused a glass of water earlier) or that the liquid this particular actor drank was from a new batch (making it plausibly different from the one that all the other participants had drunk), chances are observers would favor a stimulus attribution. Thus, having or not having certain information predictably influences people's causal attributions (Kelley, 1967), but this information influence is orthogonal to the explainers' actor versus observer role.

By distributing information selectively to explainers, the researcher can push them toward either an internal or an external attribution regardless of their perspective. Base-rate studies distributed information to actors and observers in such a way that actors favored external attributions and observers favored internal attributions; this sort of information distribution helps confirm the actor–observer hypothesis. However, one could design base-rate studies that would be likely to contradict the hypothesis. Thus, base-rate studies do not seem to tell much about the actor–observer asymmetry proper—unless the particular information distribution created in the extant base-rate studies is actually representative of reality.

Assume for a moment that real life does, like the base-rate studies, provide actors with intimate information about their personal states and history that often leads them to reject internal attributions in favor of external attributions. Then, one would predict that as an observer gets more intimate with and knowledgeable about the actor, the observer too would reject such person attributions and favor situation attributions. This is Jones and

Nisbett's (1971) information account of the actor–observer asymmetry, one of two major accounts featured in textbooks of psychology and social psychology. Yet the meta-analysis does not support this prediction. One actually sees a stronger asymmetry among intimates ($\bar{d} = 0.245$) than among strangers ($\bar{d} = 0.072$), particularly in their avoidance of external attributions. So, whatever privileged information actors generally have, this information does not seem to favor situation attributions because, in all meta-analytic averages, actors did not differ from observers on external attributions (see Table 2).

To conclude, the moderator effect that base-rate information exerts on actor–observer asymmetries appears to rely on strong experimental demands (selective information distributions) and does not seem to represent genuine differences between actor and observer perspectives. It is methodologically useful to know that a substantial actor–observer asymmetry can be produced when the actor is portrayed as unusual and distinct, but such an asymmetry does not appear to generalize to the world outside the laboratory.

Realism. One surprising moderator variable is the realism of the event explained: Explanations for actual events ($N = 149$) did not yield a noteworthy actor–observer asymmetry ($\bar{d} = 0.059$), whereas explanations for hypothetical events ($N = 24$) did yield a significant asymmetry ($\bar{d} = 0.280$). In ordinary life, people typically do not explain hypothetical behavior, so the external validity of studies with hypothetical explanations is in question. Hypothetical explanations may be meaningful if they are understood as expressions of plans or predictions (Malle & Tate, 2006). Further research is needed to clarify whether there is indeed an actor–observer asymmetry for predictions of behavior.

Citation impact. The moderating effect of a journal's citation impact was small and inconsistent. Univariate analyses showed a weak trend toward larger effect sizes in studies from high-impact journals; multivariate analyses marked this pattern more reliably. Even so, in the multivariate analysis, the effect size *d* increased by only 0.07 units with one unit gain in citation impact. In addition, both univariate and multivariate analyses were driven by the data from the *Journal of Personality and Social Psychology,* which has by far the highest impact factor among the journals in the pool (3.1). Its influence in turn relied heavily on three extreme effect sizes reported in one article that manipulated base rates (Hansen & Stonner, 1978). In both univariate and multivariate analyses, removal of this article further reduced the already small effect of citation impact and rendered it nonsignificant.

Event identity. When Carl explains Lana's success on a task and Lana explains her own success, one has the identical event being explained by actor and observer. By contrast, when Lana explains both her own success and Carl's success on the task, one has two events (of the same type) being explained by one person who occupies both the actor role and the observer role. One might expect that actors and observers would be more likely to differ in their explanations when they do not explain the very same event because construal differences may contribute to explanation differences. Counter to expectations, however, there was no asymmetry when actors and observers explained nonidentical events ($\bar{d} = 0.018$), whereas there was a slight asymmetry when they explained the very same event ($\bar{d} = 0.188$). This finding, which also held up in the multivariate analysis in the presence of other moderators, is difficult to explain psychologically; it may well be a proxy for some other processes. In particular, studies that asked actors and observers to explain the same event always treated perspective as a between-subjects variable (which is a design that leads to higher effect sizes on average), and the identical event in these studies tended to be one whose valence had been experimentally manipulated. If design and valence manipulations largely account for the effect of event identity, then adding them into the simultaneous multiple regression of I-E on all moderators should weaken or eliminate the effect of identity. Indeed, in the presence of these two variables, event identity no longer predicted effect sizes significantly ($b = .08$, $p > .30$).

Intimacy. The moderator effect of intimacy is surprising because its direction was opposite to what has been assumed in the literature. Jones and Nisbett (1971) suggested that observers who know the actor well would decrease their internal attributions, and many textbooks have described the observer's lack of intimacy (or knowledge) as one of the major reasons for the actor–observer asymmetry (e.g., Franzoi, 2006; Myers, 2004; Taylor et al., 2006). The meta-analytic data suggest, however, that when actors and observers are intimates, they actually show a larger asymmetry than when they are strangers. There is some doubt, however, about the unique contribution of this variable (see Table 4). Only 29 studies involved intimates, and among them, 10 studies assessed explanations by means of coding verbal responses. This subgroup of studies produced an effect size of $\bar{d} = 0.42$, whereas the other 19 studies (which used rating scales) produced an effect size of $\bar{d} = 0.15$, which was barely higher than the value ($\bar{d} = 0.09$) produced by 142 studies of strangers (across assessment method). I offer a possible account of this pattern after discussing the coding variable itself.

Coding. The classic asymmetry emerged in content-coded verbal explanations ($\bar{d} = 0.318$) but was virtually absent on fixed rating scales ($\bar{d} = 0.062$). This moderator would normally be considered a methodological variable, but I propose that it may be of considerable theoretical interest. Why would natural text codings be more sensitive than fixed rating scales in detecting actor–observer differences? Neither traditional attribution theories nor Jones and Nisbett's (1971) model of the actor–observer asymmetry provide an explanation for the boundary between ratings and codings, so one has to look elsewhere to account for it. The central question is whether these text codings are more sensitive to real psychological differences or to mere linguistic differences in the way actors and observers formulate their explanations. To answer this question, I briefly introduce a theoretical framework that allows the identification of the specific explanation parameters to which traditional person–situation (internal–external) codings are most sensitive.

Over the past decade, my colleagues and I have developed a model of attribution that we call the *folk-conceptual theory of explanation* (Malle, 1999, 2004, in press; Malle, Knobe, O'Laughlin, Pearce, & Nelson, 2000; O'Laughlin & Malle, 2002). It arose from the assumption that natural language is a more valid indicator of behavior explanations than are theoretically constrained (internal–external) rating scales (Antaki, 1988; Fletcher, 1983). Indeed, if one examines naturally occurring behaviors, it becomes apparent that ordinary people do not think about human behavior in simple person-versus-situation terms; rather, their explanations are embedded in a complex folk-conceptual framework

(Malle, 1999, 2005a). A core element of this framework is people's sharp distinction between unintentional and intentional behavior (Buss, 1978; Malle & Knobe, 1997a; White, 1991). As a result of this distinction, people explain intentional behaviors very differently from unintentional behaviors and apply a variety of explanation modes to intentional action that are not captured by the classic person–situation dichotomy (Malle et al., 2000). Detailed coding of naturally occurring explanations can identify these explanation modes and reveal the cognitive and social functions they serve (Malle, 2004).

One corollary of the folk-conceptual theory is that the person–situation dichotomy is too simple to capture significant variance in the way people explain behavior, and that is why one sees no actor–observer asymmetries on person–situation rating scales. There are, however, real differences between actors' and observers' explanations of behavior—differences that come into clear view when verbal explanations are coded into appropriate theoretical categories (Knobe & Malle, 2002; Malle, 1999, 2002, 2005b). For example, actors and observers differ in the types of events they explain (e.g., actions vs. experiences; Malle & Knobe, 1997b), in the modes of explanation they prefer, and in the specific linguistic tools they use to express those explanations (Malle, Knobe, & Nelson, 2006). A key claim here is that these real actor–observer differences have nothing to do with a person–situation distinction per se. One tends to see an actor–observer asymmetry on person–situation coded verbal explanations only because some of the real actor–observer differences can bleed through to the person–situation classifications and make it appear as if actors and observers differ on these classifications.

Let me illustrate how real actor–observer differences in formulating explanations can produce spurious person–situation patterns when a coding approach is used. It has been observed that person–situation codings are highly sensitive to linguistic differences in the ways people express explanations (Antaki, 1994; Monson & Snyder, 1977; L. Ross, 1977). In addition, our research has shown that actors and observers differ in the way they express explanations (Malle, 2002; Malle, Knobe, & Nelson, 2006). As a result, person–situation codings may reflect linguistic actor–observer differences that need not have anything to do with genuine person–situation causes. For example, actors often explain their actions by mentioning a reason for which they acted (typically, a belief or desire; Malle, 1999), but in expressing those reasons, they often refer only to the content of the reasons and omit the verb marker of the reason's status as a mental state. Whereas an observer might say, "She watered the orchid because she thought the soil was dry" (explicitly marking the belief reason with the mental state verb "she thought"), an actor might say, "I watered the orchid because the soil was dry" (omitting the mental state verb and thereby leaving the reason unmarked). When such explanations are coded with a person–situation scheme, reasons that are marked (e.g., "because she thought. . . .") are taken to refer to the agent and therefore end up being coded as person attributions, but when the same reasons are left unmarked—that is, expressed without the mental state verb ("because the soil was dry")—they end up being coded as situation attributions. (For supportive evidence, see Malle, 1999; Malle et al., 2000.) From the perspective of the folk-conceptual theory of explanation, using or omitting mental state markers can carry powerful social functions (Malle et al., 2000), but from the perspective of classic attribution theory, such linguistic variations are meaningless because they do not reflect actual ascriptions of person or situation causes (L. Ross, 1977).

If this reasoning is correct, then the increased actor–observer differences in person–situation attributions among coded studies are likely to be spurious. A person–situation asymmetry seemingly emerges in coded explanations not because of any true actor–observer asymmetry in person–situation causes but because of an interesting difference in the way actors and observers use verbs to formulate some of their behavior explanations (Malle, 1999; Malle et al., 2000.)

This proposal also applies to the puzzling pattern of results according to which intimate actors' and observers' explanations, if content coded, yielded a considerable effect size ($\bar{d} = 0.42$). This pattern, I suggest, is also a symptom of differences in the use of mental state markers. To the extent that intimate observers actually know what their friends or partners think, want, and like, they may express this knowledge to an audience (in this case, the experimenter) by offering explanations with mental state markers: "Why did she choose psychology?" "I guess because she wants to help people" or ". . . because she thinks it's interesting." The targets themselves can easily leave out these markers (indicated below by [—]) without marring the meaning of their explanations: "Why did I choose psychology? I guess [—] to help people" or ". . . because [—] it's interesting." Indeed, our own studies have shown that actors use systematically fewer mental state markers than observers do (Malle, 2005b; Malle, Knobe, & Nelson, 2006), and because a good number of reasons have situation content, actor's explanations appear to refer to external causes. This linguistic pattern may be responsible for the considerable effect size from studies in which intimates explained behavior that was later content coded.

Lessons for Theory: Explanations of the Asymmetry

Given the lack of support for a genuine (valence-independent) actor–observer asymmetry in person–situation attributions, the question about what explains the asymmetry may seem superfluous. However, some of the psychological forces assumed to underlie the person–situation asymmetry are so compelling that one needs to reconcile their plausibility with the equally compelling lack of evidence for the asymmetry they were designed to explain. Three such forces have been postulated: information differences (knowledge), attention differences (visual perspective), and motivational differences (sense of freedom).

Information differences. Information or knowledge differences represent one major textbook explanation of the actor–observer effect (e.g., Bernstein, Clarke-Stewart, Roy, & Wickens, 1997; Franzoi, 2006; Gray, 2002; Myers, 2004; Taylor et al., 2006). Actors are expected to offer more situational attributions (and fewer person attributions) because they have more information than observers about such things as their feelings, intentions, and personal history with respect to the behavior in question. Furthermore, when observers gain access to such information (when they know the actor well), the person–situation asymmetry should disappear. However, the pertinent studies have shown the opposite pattern. According to the meta-analytic results, an actor–observer asymmetry holds only when actor and observer are intimates (know each other well), whereas no asymmetry emerges when actor and observer are strangers. I suggested earlier that this finding may reflect a linguistic difference in the way actors and

observers formulate their explanations. Yet this does not rule out that there is something real about the impact of knowledge differences on behavior. This impact may just not be reliably demonstrated within a person–situation framework of attribution. By contrast, the folk-conceptual theory captures information differences in several explanation parameters. For example, actors normally have more information about the reasons for their actions (e.g., beliefs, desires, goals) because they have decided to act for those very reasons, whereas observers must frequently infer the actor's reasons and at times simply lack this information. This is especially true for belief reasons, which are generally harder to infer than desire reasons (Bruner, 1990; Malle, 1999; Wellman & Woolley, 1990). As a result, observers offer fewer belief reasons on average than actors do, and the average effect size across six studies exceeded $d = 0.50$ (Malle, 2002, 2004; Malle, Knobe, & Nelson, 2006). Thus, reliable actor–observer asymmetries that may reflect information differences are beginning to be found, but only if explainers are allowed to go beyond ratings of person causes and situation causes.

Attention differences. Heider (1958) spoke of the actor engulfing the observer's field, being figure against the situational background, and Jones and Nisbett (1971) contrasted observers' attentional focus with that of actors who are "directed outward" (p. 85) because they cannot easily perceive their own behavior. In Nisbett and Ross's (1980) formulation, "the situation normally will be figural for the actor" (p. 123). One would therefore predict that changing the actor's (and the observer's) visual perspective on the behaviors in question would eliminate the actor–observer asymmetry. Two studies confirmed this prediction, but four disconfirmed it, and so did the average effect across all available studies. In a recent experiment in our laboratory, my colleagues and I also disconfirmed the hypothesis of an impact of visual perspective on person–situation attributions (Malle, Heim, & Knorek, 2006). At the same time, we showed a powerful impact of perspective on one parameter of explanations not considered by classic attribution theory: the use of mental state markers. As mentioned above, actors normally explain their actions using few such markers; when they see themselves on a video recording, however, they appear to take a more distant view of themselves and mark more explicitly the mental states of "that person over there."

Motivational differences. Nisbett, Caputo, Legant, and Marecek (1973) suggested that actors avoid using internal causes to explain their own behavior because they do not like to lose their sense of freedom. Very few studies manipulated or assessed sense of freedom and its effect on the actor–observer asymmetry. Among the few, D. T. Miller and Norman (1975) actually found the opposite pattern from what was predicted. Surprisingly, the authors explained their contrary findings in terms of the same sense of freedom: Actors, more than observers, attribute their behavior to internal factors because they want to avoid freedom-threatening situation attributions (see also D. T. Miller & Porter, 1980). Once again, I would like to suggest that a sense of freedom may well influence people's explanations of behavior, but this influence cannot be reliably demonstrated with a person–situation approach to explanations. By contrast, in our studies (Malle, Knobe, & Nelson, 2006), my colleagues and I found that actors, compared with observers, prefer to explain their intentional actions with reference to the reasons for which they acted—the mode of explanation that emphasizes the agent's control, deliberation, and free choice.

To conclude, despite the lack of support for the traditional actor–observer asymmetry, one should not yet abandon the intuition that actors and observers differ along a variety of psychological processes (information, attention, and motivation) that can in turn affect behavior explanations. The person–situation approach, however, is not the way to capture and document these effects. Explanations are a sense-making activity embedded in a complex conceptualization of human action and experience, and this conceptualization generates a manifold of modes and forms of explanation (Malle, 2004). Powerful actor–observer asymmetries appear to exist with respect to these modes and forms of explanation (Knobe & Malle, 2002; Malle, 2002; Malle, Knobe, & Nelson, 2006), but these asymmetries do not come into sight when studied within a person–situation framework.

Another Lesson for Theory: The Fundamental Attribution Error

A good number of sources in social and general psychology have equated the actor–observer asymmetry with the FAE or correspondence bias (Gilbert & Malone, 1995; Jones & Harris, 1967; L. Ross, 1977). For example, C. G. Morris and Maisto (in press) described the FAE as "part of the actor–observer bias" (p. 450), whereas Hockenbury and Hockenbury (2006) described the actor–observer effect as an exception to the FAE, echoing Baron, Byrne, and Branscombe (2006), who claimed that the FAE applies mainly to others (p. 102). Underlying all these views is the assumption that the FAE drives the observer part of the actor–observer asymmetry: Because observers (erroneously) overattribute behavior to the actor's dispositions, whereas actors attribute behavior more to the situation, the actor–observer asymmetry emerges. It turns out now that actors and observers do not really differ in their attributions to dispositions versus situations. Does one have to conclude that the FAE does not exist either? The answer to this question depends on what is meant by the FAE.

To the extent that the FAE is formulated as a statement about behavior explanations, it is seriously called into question by the present results. For example, L. Ross and Nisbett (1991) stated that "people are inclined to offer dispositional explanations for behavior instead of situational ones" (p. 125) and that "actors tend to give fewer dispositional explanations for their behavior than observers do" (p. 140). The present data do not support their contentions. To begin, people just do not use dispositions very much in their behavior explanations. Estimated from about 10,000 verbal explanations I have collected over the past 10 years, only 5%–10% of all explanations refer to personality traits, and an additional 5% refer to stable beliefs and preferences (Malle, 2004). Furthermore, as the present data show, actors and observers do not notably differ in their person and situation explanations whether one bases the comparison on traits or on all available internal (person) factors.

However, the FAE need not be equated with a pattern of behavior explanation. Again, L. Ross and Nisbett (1991) wrote that "people infer dispositions from behavior that is manifestly situationally produced" (p. 126) and people "assume a person has traits corresponding directly to the type of behavior that was exhibited" (p. 88). The context in which the FAE, thus defined, can occur is quite circumscribed: when a perceiver observes a behavior

that may be diagnostic of an underlying trait (e.g., personality, attitude) but that has, in fact, been strongly pressured or enticed by the situation. When the perceiver makes a trait inference in this situation—an inference about how the actor stably differs from other people—an error may be present. Yet the social perceiver's concern here is not with explaining the behavior but with inferring whether this behavior is characteristic of the person, whether he or she would show the behavior again, whether he or she has, in a word, the stable disposition to do this sort of thing. When defined this way, the FAE qua trait inference from nondiagnostic behavior is not challenged by the present results on behavior explanations. One might have a variety of concerns about the claim that people routinely commit the FAE. For example, there is no evidence that people do it routinely (about 95% of all data on the FAE have emerged from tightly controlled lab experiments, not surveys, observations, or archival studies), the question of what constitutes an error is a thorny one (Funder, 1987; Krueger & Funder, 2004), and it is questionable to consider something fundamental if other important processes, such as behavior explanations, neither show the tendency toward trait (ab)use nor document any notable difference between actor and observer explanations. However, whatever misgivings one might have about the evidence on fundamental, erroneous, or just plain trait inferences, one cannot conclude that just because there is no actor–observer asymmetry in attributions, there is no FAE.

Threats to Validity

Generalizations from meta-analyses are only as valid as the data on which they are based—which include the study sample, extraction and statistical analysis of effect sizes, and construct representation. Below, I discuss potential threats to the validity of the present conclusions, following the outline by Shadish, Cook, and Campbell (2002).

Sample of primary studies. Despite intensive search procedures scanning the published literature, the present sample of studies may have missed some empirical data on the actor–observer asymmetry. For example, book chapters have not been included in literature databases until recently (and many still do not include them); likewise, some foreign-language journals may not be listed in the mainstream databases. However, the number of studies in the present sample is large (173, compared with an average of 71.5 studies in other social psychological meta-analyses; Richard, Bond, & Stokes-Zoota, 2003), rendering the overall results very robust against the potential of omitted studies. For example, if there were 50 studies somewhere that averaged an effect size of $\bar{d} = 0.40$ or a sample of 100 studies with $\bar{d} = 0.30$, the combined sample of studies still would have an effect size of only $\bar{d} = 0.16$ or 0.17, respectively.

The literature is unlikely to contain such a large number of published studies without leaving a trace in the searchable databases. However, researchers' file drawers may contain a substantial number of unpublished studies whose results may deviate from those of the published pool.

File drawer and publication bias. Meta-analysts often estimate how many disconfirming but left-in-the-file-drawer studies it would take to make the supportive effect size become indistinguishable from zero (Rosenthal, 1979). In the present analysis, a converse estimation is in order. How many confirming studies would have to be hidden in file drawers to increase the obtained near-zero effect to a more substantial size? It would take 177 additional studies with effect sizes of $d = 0.50$ to arrive at an overall effect size of $\bar{d} = 0.30$, and it would take 233 studies with effect sizes of $d = 0.80$ to arrive at an overall effect size of $\bar{d} = 0.50$.

It is difficult to imagine so many studies with such strong positive results remaining unpublished in researchers' file drawers. To go beyond imagination and get some sense of the number and results of unpublished studies, I examined the Dissertation Abstracts database. Findings from dissertations can be expected to have minimal publication bias because disconfirming findings are just as likely to be written up as are confirming findings, as few doctoral students are willing to start over with a new research program or forfeit their degree because of unexpected results. A search of the 460,000 theses in psychology or education that were recorded between 1971 and 2004 yielded an initial set of 180 candidate studies, with the keyword combination *actor* or *observer* or *self–other* and *attribution.* According to their abstracts, only 10 had measured genuine actor–observer asymmetries in causal attribution (one additional thesis was published as Eisen, 1979). Of the five dissertations whose abstracts provided information about the findings (or whose results I knew), two appeared to confirm the actor–observer hypothesis, and three appeared to contradict it.

Thus, the present result seems quite robust against the possibility that the true effect size is (substantially) larger. What about the opposite? Might the true effect size be even smaller? This is possible given that the pool of unpublished studies in researchers' file drawers is assumed to contain a substantial number of nonsignificant, null, or disconfirming findings (Begg & Berlin, 1988; Coursol & Wagner, 1986; Hedges, 1984). It would take only 23 disconfirming studies with an average effect size of $\bar{d} = -0.30$ to make the overall mean effect size go below $\bar{d} = 0.05$, and 35 studies (one disconfirming study per year since 1971) with −0.40 would bring $\bar{d}$ to 0.01. A more systematic estimation of the possible impact of unpublished disconfirming studies can be based on Greenwald's (1975) survey of reviewers and journal authors on the issue of publication decisions. From his data, one can estimate the likelihood of a study's eventual publication given that it yielded confirming results as $p(\text{published} \mid \text{confirming}) = 0.50$ and the likelihood of publication given that it yielded disconfirming results as $p(\text{published} \mid \text{disconfirming}) = 0.30$. When one designates findings with $d \geq 0.050$ as confirming and those below 0.050 as disconfirming, the present pool includes 75 disconfirming studies (30% of all disconfirming studies) and 98 confirming studies (50% of all confirming studies). Thus, 275 studies should be expected in file drawers somewhere, with 98 of them confirming and 177 disconfirming. Assuming, finally, a d of 0.408 for confirming studies and a d of −0.292 for disconfirming studies (averages from the present pool), the inclusion of potential file drawer studies in the pool would yield an overall effect size of 0.010. Notably, this value is identical to the one produced by the trim-and-fill correction procedure for publication bias reported earlier, thus showing excellent convergent validity.

Nonindependence. Effect sizes that enter a meta-analysis are assumed to be independent, and considerable dependence among data points can threaten the validity of meta-analytic findings. There was some amount of dependence in the present analysis

because, even though only one effect size was extracted from each study (condition or sample), studies were sometimes nested within articles. Effect sizes from samples collected in the same setting and by the same researchers will on average be correlated and may therefore inflate effect size averages. To estimate the possible inflation in the present data set, I computed a per-article effect size average ($N = 113$). Using a random-effects model, this value ($\bar{d} = 0.092$) was virtually identical to the average based on the 173 individual studies/samples ($\bar{d} = 0.095$), so there is no reason to assume effect size inflation from nonindependence.

Study quality. Several features of study quality and reporting quality can threaten the validity of a meta-analysis (Matt & Cook, 1994). Uncertainty in inferences of effect size is one such feature, and it was a problem with some of the present studies, primarily because of limited data reporting. However, no relationship between inference uncertainty and actual effect size was apparent.

If one assumes that journals with higher citation impact tend to be more rigorous and that the studies they publish are of higher quality, then effect size should covary with citation impact if study quality is a concern. Here, too, effect sizes showed a very small and inconsistent relationship with citation impact.

Reliability of dependent measures is another quality feature. The present analysis did not take this feature into account because the reliability of attribution measures was largely unknown. Only one study in the present pool (Herzberger & Clore, 1979) reported a test–retest reliability, which was $r_{tt} = 0.26$ for the four-item attribution measure that was developed by Storms (1973) and used in at least six other studies. However, such low retest reliabilities may be ecologically valid because measures for explanations of context-specific events are not expected to be temporally stable; all that is required is internal consistency of items. Ender and Bohart (1974) reported consistency coefficients of 0.77 to 0.88 for Weiner's scales (ability, effort, task, luck) across sets of 10 items. When using the Storms measure (person–situation ratings for having been talkative, friendly, dominant, nervous) in our lab, my colleagues and I found lower reliabilities (Malle & Pearce, 2001). Cronbach alpha coefficients among the four actor attributions were 0.78 for I-E, 0.53 for I, and 0.73 for E, but there was less consistency for observer attributions, with Cronbach alphas of 0.29 for I-E, 0.41 for I, and 0.34 for E. The source of unreliability appears to have been a two-dimensionality of observer ratings: Attributions of friendly and talkative (two desirable behaviors) showed $\alpha = .89$ for I-E, whereas attributions for nervous and dominant (two undesirable behaviors) showed $\alpha = .65$, and the two dimensions did not correlate. This divergence of attributions for what appear to be positive and negative events raises the possibility that observer attributions for valence-mixed behavioral stimuli (e.g., the range of behaviors during a 5-minute interaction) may be a misleading aggregate of distinct positive-valence and negative-valence attributions. Thus, the present meta-analytic results might be more reliable for studies examining events with clearly positive or negative valence than for studies examining events with neutral, mixed, or unknown valence.

Unreliability in the coding of moderator variables can be a concern as well. Intercoder agreement in the present analysis was satisfactory, but traitedness of attribution scores and event valence showed only moderate agreement. Additional analyses of ability versus effort attributions provided a clean test of the traitedness hypothesis without reliance on coding, and it showed results similar to the ones based on coding. To examine the possible impact of unreliability in event valence coding, I excluded those studies for which there had been disagreement on the valence code, and the results were indistinguishable from the original ones displayed in Table 3.

Older publications sometimes report results in less detail, which can make effect size calculation difficult or impossible and lead to a disproportional exclusion of studies. In the present study-selection process, I encountered only four studies that met all inclusion criteria but failed to report vital information for effect size calculation (all were missing the relevant means). Of these excluded studies, three reported their findings verbally: One was consistent with the actor–observer hypothesis, and two were inconsistent with it.

In sum, like every other meta-analysis, the present one examined a sample of studies, not the entire population, and conclusions are constrained by this sample. However, none of the threats to validity of the present study sample appear to be serious, and many of the common concerns about selection and publication bias may actually strengthen the present findings because the file drawer part of the population tends to contain more null results (and perhaps more hypothesis-disconfirming results) than the published record.

Limitations

Even though the present conclusion holds up well under scrutiny of validity concerns, it does have some remaining limitations. First, the data pool did not contain unpublished studies. All estimations of their potential impact and the examination of unpublished dissertations support the reached conclusions, but it would further increase confidence if one had access to a comprehensive sample of unpublished data.

Second, a large number of studies did not report correlations among dependent measures (e.g., internal with external, effort with ability, positive with negative). On the basis of studies that did report correlations, I computed a best estimate, but some uncertainty remains. In the case of internal and external scores, the results do not seem to be affected by a biased estimate because the I and E scores examined separately lead to the same conclusions as the I-E scores with an assumed I*E correlation of −0.1. The impact of estimating ability and effort correlations (and task and chance correlations) is more difficult to assess, but no apparent differences emerged when separating results that were based on these component items and those that were based on direct I and E measures.

Third, the moderator analyses were not as successful in accounting for between-study variance as one might like. An overall prediction of $R = 0.51$ is decent, but the small number of studies within some of the moderator categories created somewhat unstable results. The analyses brought to light two strong and reliable moderators (valence and realism of event), one strong but variable moderator (base-rate information) that was based on very few studies, and four additional moderators that were smaller in contribution. Of the latter, some may not make a unique contribution once other variables are controlled for. However, it should also be noted that several of the moderators that have been featured in the literature (e.g., visual perspective switch, time delay, involved observers) were tested but failed to predict effect size variance. The pool of actor–observer studies may contain substantial

between-study variance that is nonsystematic, reflecting the influence of instruction, context, sample, and the interpretational ambiguity of dichotomous person–situation scales.

Lessons for the Science of Psychology

One of the intriguing questions raised by the present results is how a hypothesis could be presented in textbooks for decades as supported by research even though the cumulative evidence showed little support for it. One plausible reason for the reluctance to abandon the actor–observer hypothesis is its compelling simplicity: an elegant 2 (actors and observers) × 2 (person and situation) interaction, tested by means of two rating scales. It is understandable that researchers were initially hesitant to give up such an attractive and easily researched hypothesis in the absence of an equally attractive theoretical alternative. At the same time, the widespread acceptance of the hypothesis curtailed the motivation to consider a theoretical alternative. Another contributor to the actor–observer hypothesis's longevity in the face of unsupportive evidence may have been the considerable influence of valence on attributions. A total of 61 studies had actors and observers explain negative events, and for those, the classic asymmetry pattern was obtained on average. Contrary to that, in 45 studies, actors and observers explained positive events and displayed a reverse asymmetry. This situation presents a stochastic advantage for readers finding the hypothesis confirmed rather than disconfirmed. In addition, several authors concluded that the actor–observer asymmetry is weakened for positive events rather than falsified (e.g., Fiske & Taylor 1991, pp. 74–75), and many studies under the label of self-servingness may have been falsely counted as confirming the actor–observer hypothesis.

A final plausible reason for the actor–observer hypothesis's longevity may be that early studies published in high-impact journals supported the hypothesis. In light of this early success, the hypothesis may have been assumed to be true by default (for exceptions, see Buss, 1978; Sabini, 1995; Weary, Stanley, & Harvey, 1989), preventing the needed reconciliation with contradictory data. Interestingly, even the early success was limited. By 1973, the cumulative d was 0.16, but by 1976, it had dropped to 0.06. Then, it increased steadily and stayed around 0.18 until a well-cited review of the actor–observer hypothesis (Watson, 1982) concluded that the hypothesis was strongly supported—a conclusion that was possible only because many published studies (and many falsifying studies) had not entered this qualitative review.

Whereas qualitative reviews can lack rigor in study selection and data integration, meta-analysis holds great promise as a tool for systematically examining core scientific hypotheses and developing new theory (N. Miller & Pollack, 1994). Indeed, over the past decades, meta-analyses have begun to supplant qualitative reviews in the behavioral sciences, and many of these analyses have offered compelling, or at least provocative, evidence either for previously unpopular hypotheses (e.g., Bem & Honorton, 1994) or against previously popular hypotheses (e.g., Allen & Burrell, 1996; Frank, Augustyn, Grant Knight, Pell, & Zuckerman, 2001).

Summary

A meta-analysis of 113 articles and 173 studies showed scarce support for an actor–observer asymmetry in attribution, with overall averages ranging from $\bar{d} = -0.015$ to $\bar{d} = 0.095$, depending on statistical models and specific attribution scores. Corrections for the preponderance of negative valence studies as well as for possible publication bias made the average effect size converge to 0. Under particular conditions, this value can increase, primarily when negative events are explained in between-subjects designs, when the experimenter provides base-rate information that portrays the actor as idiosyncratic, when hypothetical events are explained, or when explanations are verbally expressed and later coded. However, even these conditions do not provide compelling support for the classic actor–observer hypothesis. A sole focus on negative events is not compatible with the actor–observer hypothesis because the hypothesis requires a main effect across valence. Base-rate studies appear to speak not to an actor–observer asymmetry per se but rather to the effects of creating specific information differences between actors and observers—differences that can easily be reversed and are not representative of the information actors and observers have outside the laboratory. Similarly, an asymmetry for hypothetical events is of limited force given that outside the laboratory, people predominantly explain real behaviors and outcomes. Finally, codings of verbal explanations into person and situation categories appear to be oversensitive to linguistic surface patterns and therefore support an actor–observer asymmetry only in formulating explanations, not in ascribing causes.

The disconfirmation of an actor–observer asymmetry along person and situation attributions should not be mistaken for the disconfirmation of actor–observer asymmetries in general. Recent evidence suggests that actors and observers differ reliably in multiple (and psychologically significant) features of explanation, but these features are not captured by the classic person–situation distinction. Thus, the present meta-analysis both resolves a long-standing empirical question and sets the stage for a new approach to studying how people explain human behavior.

References

References marked with an asterisk indicate studies included in the meta-analysis.

*Abramovitch, R., & Freedman, J. L. (1981). Actor–observer differences in children's attributions. *Merrill-Palmer Quarterly, 27,* 53–59.

Ackermann, R., & DeRubeis, R. J. (1991). Is depressive realism real? *Clinical Psychology Review, 11,* 565–584.

Allen, M., & Burrell, N. (1996). Comparing the impact of homosexual and heterosexual parents on children: Meta-analysis of existing research. *Journal of Homosexuality, 32,* 19–35.

*Ames, R., Ames, C. A., & Garrison, W. M. (1977). Children's causal ascriptions for positive and negative interpersonal outcomes. *Psychological Reports, 41,* 595–602.

*Anderson, C. A. (1985). Actor and observer attributions for different types of situations: Causal-structure effects, individual differences, and the dimensionality of causes. *Social Cognition, 3,* 323–340.

Antaki, C. (Ed.). (1988). *Analysing everyday explanation: A casebook of methods.* Newbury Park, CA: Sage.

Antaki, C. (1994). *Explaining and arguing: The social organization of accounts.* Thousand Oaks, CA: Sage.

*Arkin, R. M., & Duval, S. (1975). Focus of attention and causal attributions of actors and observers. *Journal of Experimental Social Psychology, 11,* 427–438.

Aronson, E. (2002). *The social animal* (8th ed.). New York: Worth Publishers.

*Ashkanasy, N. M. (1997). Attributions for the performance of self and other: It matters who the "other" is. *Australian Journal of Psychology, 49,* 14–20.

*Ashkanasy, N. M., & Gallois, C. (1987). Locus of control and attributions for academic performance of self and others. *Australian Journal of Psychology, 39,* 293–305.

Baron, R. A., Byrne, D., & Branscombe, N. R. (2006). *Social psychology* (11th ed.). Boston: Pearson.

*Bar-Tal, D., & Frieze, I. H. (1976). Attributions of success and failure for actors and observers. *Journal of Research in Personality, 10,* 256–265.

*Beckman, L. J. (1979). Beliefs about the causes of alcohol-related problems among alcoholic and nonalcoholic women. *Journal of Clinical Psychology, 35,* 663–670.

Begg, C. B., & Berlin, J. A. (1988). Publication bias: A problem in interpreting medical data. *Journal of the Royal Statistical Society: Series A, 151,* 419–463.

Bem, D. J., & Honorton, C. (1994). Does psi exist? Replicable evidence for an anomalous process of information transfer. *Psychological Bulletin, 115,* 4–18.

Bernstein, D. A., Clarke-Stewart, A., Roy, E. J., & Wickens, C. D. (1997). *Psychology* (4th ed.). Boston: Houghton Mifflin.

*Böhm, G. (1992). Perspektive oder Bewertung: Was beeinflußt Kausalattributionen? [Perspective or valence: What influences causal attributions?]. *Zeitschrift für Sozialpsychologie, 23,* 206–216.

*Boski, P. (1983). Egotism and evaluation in self and other attributions for achievement related outcomes. *European Journal of Social Psychology, 13,* 287–304.

Bradley, G. W. (1978). Self-serving biases in the attribution process; A reexamination of the fact or fiction question. *Journal of Personality and Social Psychology, 36,* 56–71.

*Brockner, J., & Eckenrode, J. (1979). Self-monitoring and the actor–observer bias. *Representative Research in Social Psychology, 9,* 81–88.

Bruner, J. (1990). *Acts of meaning.* Cambridge, MA: Harvard University Press.

*Burger, J. M. (1986). Temporal effects on attributions: Actor and observer differences. *Social Cognition, 4,* 377–387.

Buss, A. R. (1978). Causes and reasons in attribution theory: A conceptual critique. *Journal of Personality and Social Psychology, 36,* 1311–1321.

*Butler, R. (1986). The role of generalised expectancies in determining causal attributions for success and failure in two social classes. *British Journal of Educational Psychology, 56,* 51–63.

*Buunk, B. (1984). Jealousy as related to attributions for the partner's behavior. *Social Psychology Quarterly, 47,* 107–112.

*Buunk, B. (1987). Conditions that promote breakups as a consequence of extradyadic involvements. *Journal of Social and Clinical Psychology, 5,* 271–284.

*Chen, H., & Yates, B. T. (1990). Attributions and information processing as a function of observers' involvement level: A re-examination of actor–observer attribution differences. *Annals of the New York Academy of Sciences, 602,* 133–155.

*Chen, H., Yates, B. T., & McGinnies, E. (1988). Effects of involvement on observers' estimates of consensus, distinctiveness, and consistency. *Personality and Social Psychology Bulletin, 14,* 468–478.

Cohen, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). Hillsdale, NJ: Erlbaum.

*Compas, B. E., Adelman, H. S., Freundl, P. C., Nelson, P., & Taylor, L. (1982). Parent and child causal attributions during clinical interviews. *Journal of Abnormal Child Psychology, 10,* 77–84.

*Compas, B. E., Friedland-Bandes, R., Bastien, R., & Adelman, H. S. (1981). Parent and child causal attributions related to the child's clinical problem. *Journal of Abnormal Child Psychology, 9,* 389–397.

Cooper, H. M., & Hedges, L. V. (Eds.). (1994). *The handbook of research synthesis.* New York: Russell Sage Foundation.

Coursol, A., & Wagner, E. E. (1986). Effect of positive findings on submission and acceptance rates: A note on meta-analysis bias. *Professional Psychology, 17,* 136–137.

*Cunningham, J. D., Starr, P. A., & Kanouse, D. E. (1979). Self as actor, active observer, and passive observer: Implications for causal attributions. *Journal of Personality and Social Psychology, 37,* 1146–1152.

Dawes, R. M. (1990). The potential nonfalsity of the false consensus effect. In R. M. Hogarth (Ed.), *Insights in decision making: A tribute to Hillel J. Einhorn* (pp. 179–199). Chicago: University of Chicago Press.

Deaux, K., & Wrightsman, L. S. (1988). *Social psychology* (5th ed.). Pacific Grove, CA: Brooks/Cole.

DeCoster, J. (2004). *Meta-analysis notes.* Unpublished manuscript, University of Alabama.

DeCoster, J. (2005). Meta-analysis. In K. Kempf-Leonard (Ed.), *The encyclopedia of social measurement*. San Diego, CA: Academic Press.

DeVader, C. L., Bateson, A. G., & Lord, R. G. (1986). Attribution theory: A meta-analysis of attributional hypotheses. In E. A. Locke (Ed.), *Generalizing from laboratory to field settings* (pp. 63–79). Lexington, MA: Heath.

Dunlap, W. P., Cortina, J. M., Vaslow, J. B., & Burke, M. J. (1996). Meta-analysis of experiments with matched groups or repeated measures designs. *Psychological Methods, 1,* 170–177.

Duval, S., & Tweedie, R. (2000). A nonparametric "trim and fill" method of accounting for publication bias in meta-analysis. *Journal of the American Statistical Association, 95,* 89–98.

*Eisen, S. V. (1979). Actor–observer differences in information inference and causal attribution. *Journal of Personality and Social Psychology, 37,* 261–272.

*Elliott, G. C. (1989). Self-serving attributions in the face of reality: The effect of task outcome and potential causes on self–other attributions. *Human Relations, 42,* 1015–1032.

*Ender, P. B., & Bohart, A. C. (1974). Attributions of success and failure. *Psychological Reports, 35,* 275–278.

*Feather, N. T., & Simon, J. G. (1971). Attribution of responsibility and valence of outcome in relation to initial confidence and success and failure of self and other. *Journal of Personality and Social Psychology, 18,* 173–188.

*Felson, R. B. (1981). The attributions of actors and observers concerning performance in a football game. *Journal of Social Psychology, 115,* 15–23.

*Fincham, F. D., Beach, S. R., & Baucom, D. H. (1987). Attribution processes in distressed and nondistressed couples: 4. Self–partner attribution differences. *Journal of Personality and Social Psychology, 52,* 739–748.

*Finney, P. D., & Helm, B. (1982). The actor–observer relationship: The effect of actors' and observers' responsibility and emotion attributions. *Journal of Social Psychology, 117,* 219–225.

Fiske, S. T. (2004). *Social beings: A core motives approach to social psychology.* Hoboken, NJ: Wiley.

Fiske, S. T., & Taylor, S. E. (1991). *Social cognition* (2nd ed.). New York: McGraw-Hill.

Fletcher, G. J. O. (1983). The analysis of verbal explanations for marital separation: Implications for attribution theory. *Journal of Applied Social Psychology, 13,* 245–258.

*Forsyth, D. R., Pope, W. R., & McMillan, J. H. (1985). Students' reactions after cheating: An attributional analysis. *Contemporary Educational Psychology, 10,* 72–82.

*Forsyth, D. R., & Scott, W. L. (1984). Attributions and moral judgments: Kohlberg's stage theory as a taxonomy of moral attributions. *Bulletin of the Psychonomic Society, 22,* 321–323.

Frank, D. A., Augustyn, M., Grant Knight, W., Pell, T., & Zuckerman, B. (2001). Growth, development, and behavior in early childhood following prenatal cocaine exposure. *JAMA, 285,* 1613–1625.

Franzoi, S. L. (2006). *Social psychology* (4th ed.). Boston: McGraw-Hill.

*Franzoi, S. L., & Sweeney, P. D. (1986). Another look at the relation

between private self-consciousness and self-attribution. *Journal of Research in Personality, 20,* 187–206.

Frieze, I., & Weiner, B. (1971). Cue utilization and attributional judgments for success and failure. *Journal of Personality, 39,* 591–605.

Funder, D. C. (1987). Errors and mistakes: Evaluating the accuracy of social judgment. *Psychological Bulletin, 101,* 75–90.

Funder, D. C., & Ozer, D. J. (1983). Behavior as a function of the situation. *Journal of Personality and Social Psychology, 44,* 107–112.

*Funder, D. C., & Van Ness, M. J. (1983). On the nature and accuracy of attributions that change over time. *Journal of Personality, 51,* 17–33.

*Garrido, M. E., & de la Torre Garcia, A. (1981). Actor–observador. Una hipotesis no confirmada en la theoria de la atribucion [Actor–observer: An unconfirmed hypothesis in attribution theory]. *Revista de Psicologia General y Aplicada, 36,* 697–718.

Gilbert, D. T., & Malone, P. S. (1995). The correspondence bias. *Psychological Bulletin, 117,* 21–38.

*Gioia, D. A., & Sims, H. P. (1985). Self-serving bias and actor–observer differences in organizations: An empirical analysis. *Journal of Applied Social Psychology, 15,* 547–563.

Glass, G. V., McGaw, B., & Smith, M. L. (1981). *Meta-analysis in social research.* Beverly Hills, CA: Sage.

Gray, P. O. (2002). *Psychology* (4th ed.). New York: Worth Publishers.

Greenwald, A. G. (1975). Consequences of prejudice against the null hypothesis. *Psychological Bulletin, 82,* 1–12.

Greenwald, A. G. (1980). The totalitarian ego: Fabrication and revision of personal history. *American Psychologist, 35,* 603–618.

Griggs, R. A. (2006). *Psychology: A concise introduction.* New York: Worth Publishers.

Hamilton, D. L. (1998). Dispositional and attributional inferences in person perception. In J. M. Darley & J. Cooper (Eds.), *Attribution and social interaction: The legacy of Edward E. Jones* (pp. 99–114). Washington, DC: American Psychological Association.

*Hansen, R. D., & Lowe, C. A. (1976). Distinctiveness and consensus: The influence of behavioral information on actors' and observers' attributions. *Journal of Personality and Social Psychology, 34,* 425–433.

*Hansen, R. D., & Stonner, D. M. (1978). Attributes and attributions: Inferring stimulus properties, actors' dispositions, and causes. *Journal of Personality and Social Psychology, 36,* 657–667.

*Harré, N., Brandt, T., & Houkamau, C. (2004). An examination of the actor–observer effect in young drivers' attributions for their own and their friends' risky driving. *Journal of Applied Social Psychology, 34,* 806–824.

*Harvey, J. H., Arkin, R. M., Gleason, J. M., & Johnston, S. (1974). Effect of expected and observed outcome of an action on the differential causal attributions of actor and observer. *Journal of Personality, 42,* 62–77.

*Heath, C. (1999). On the social psychology of agency relationships: Lay theories of motivation overemphasize extrinsic incentives. *Organizational Behavior and Human Decision Processes, 78,* 25–62.

Hedges, L. V. (1984). Estimation of effect size under nonrandom sampling: The effects of censoring studies yielding statistically insignificant mean differences. *Journal of Educational Studies, 9,* 61–85.

Hedges, L. V., & Olkin, I. (1985). *Statistical methods for meta-analysis.* San Diego, CA: Academic Press.

Hedges, L. V., & Vevea, J. L. (1998). Fixed- and random-effects models in meta-analysis. *Psychological Methods, 3,* 486–504.

Heider, F. (1958). *The psychology of interpersonal relations.* New York: Wiley.

*Herzberger, S. D., & Clore, G. L. (1979). Actor and observer attributions in a multitrait–multimethod matrix. *Journal of Research in Personality, 13,* 1–15.

*Herzog, T. A. (1994). Automobile driving as seen by the actor, the active observer, and the passive observer. *Journal of Applied Social Psychology, 24,* 2057–2074.

Higgins, J. P., Thompson, S. G., Deeks, J. J., & Altman, D. G. (2003). Measuring inconsistency in meta-analyses. *British Medical Journal, 327,* 557–560.

Hilton, D. J., Smith, R. H., & Kin, S. H. (1995). Processes of causal explanation and dispositional attribution. *Journal of Personality and Social Psychology, 68,* 377–387.

Hockenbury, D. H., & Hockenbury, S. E. (2006). *Psychology* (4th ed.). New York: Worth Publishers.

*Holloway, S. D., & Hess, R. D. (1982). Causal explanations for school performance: Contrasts between mothers and children. *Journal of Applied Developmental Psychology, 3,* 319–327.

*Holloway, S. D., Kashiwagi, K., Hess, R. D., & Azuuma, H. (1986). Causal attributions by Japanese and American mothers and children about performance in mathematics. *International Journal of Psychology, 21,* 269–286.

Howard, J. A. (1987). The conceptualization and measurement of attributions. *Journal of Experimental Social Psychology, 23,* 32–58.

*Huber, V. L., Podsakoff, P. M., & Todor, W. D. (1985). A dimensional analysis of supervisor and subordinate attributions of success and failure. *Journal of Occupational Behaviour, 6,* 131–142.

*Huber, V. L., Podsakoff, P. M., & Todor, W. D. (1986). An investigation of biasing factors in the attributions of subordinates and their supervisors. *Journal of Business Research, 14,* 83–97.

Hunter, J. E., & Schmidt, F. L. (1990). *Methods of meta-analysis: Correcting error and bias in research findings.* Newbury Park, CA: Sage.

*Janoff-Bulman, R., Lang, L., & Johnston, D. (1979). Participant–observer differences in attributions for an ambiguous victimization. *Personality and Social Psychology Bulletin, 5,* 335–339.

Johnson, J. T., Jemmott, J. B., III, & Pettigrew, T. F. (1984). Causal attribution and dispositional inference: Evidence of inconsistent judgments. *Journal of Experimental Social Psychology, 20,* 567–585.

Jones, E. E. (1976). How do people perceive the causes of behavior? *American Scientist, 64,* 300–305.

Jones, E. E., & Davis, K. E. (1965). From acts to dispositions: The attribution process in person perception. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 2, pp. 219–266). New York: Academic Press.

Jones, E. E., & Harris, V. A. (1967). The attribution of attitudes. *Journal of Experimental Social Psychology, 3,* 1–24.

Jones, E. E., Kanouse, D., Kelley, H. H., Nisbett, R. E., Valins, S., & Weiner, B. (Eds.). (1972). *Attribution: Perceiving the causes of behavior.* Morristown, NJ: General Learning Press.

Jones, E. E., & Nisbett, R. E. (1971). *The actor and the observer: Divergent perceptions of the causes of behavior.* Morristown, NJ: General Learning Press.

*Karasawa, K. (1995). An attributional analysis of reactions to negative emotions. *Personality and Social Psychology Bulletin, 21,* 456–467.

Kelley, H. H. (1967). Attribution theory in social psychology. In D. Levine (Ed.), *Nebraska Symposium on Motivation* (Vol. 15, pp. 192–240). Lincoln: University of Nebraska Press.

Kelley, H. H., & Michela, J. L. (1980). Attribution theory and research. *Annual Review of Psychology, 31,* 457–501.

Kenrick, D. T., Neuberg, S. L., Cialdini, R. B. (2005). *Social psychology: Unraveling the mystery* (3rd ed.). Boston: Allyn & Bacon.

*Kenworthy, J. B. (2003). Explaining the belief in God for self, in-group, and out-group targets. *Journal for the Scientific Study of Religion, 42,* 137–146.

*Kenworthy, J. B., & Miller, N. (2002). Attributional biases about the origins of attitudes: Externality, emotionality, and rationality. *Journal of Personality and Social Psychology, 82,* 693–707.

Knobe, J., & Malle, B. F. (2002). Self and other in the explanation of behavior: 30 years later. *Psychologica Belgica, 42,* 113–130.

*Korevaar, G. G., & Bergen, T. C. (1992). Experienced teachers' attributions and their reactions in problematic classroom situations. *Teaching and Teacher Education, 8,* 509–521.

Kowalski, R. M., & Westen, D. (2005). *Psychology* (4th ed.). Hoboken, NJ: Wiley.

Krueger, J. I., & Funder, D. C. (2004). Towards a balanced social psychology: Causes, consequences, and cures for the problem-seeking approach to social behavior and cognition. *Behavioral and Brain Sciences, 27,* 313–327.

*Leary, M. R. (1979). Actors' and observers' causal attributions: Effects of anticipated discussion, performance feedback, and sex. *Representative Research in Social Psychology, 10,* 77–86.

*Lewis, P. T. (1995). A naturalistic test of two fundamental propositions: Correspondence bias and the actor–observer hypothesis. *Journal of Personality, 63,* 87–111.

Light, R. J., & Pillemer, D. B. (1984). *Summing up: The science of reviewing research.* Cambridge, MA: Harvard University Press.

Locke, K. D. (2002). Are descriptions of the self more complex than descriptions of others? *Personality and Social Psychology Bulletin, 28,* 1094–1105.

*Lowe, C. A., & Hansen, R. D. (1976). Motivational influence of behavioral desirability on actors' and observers' attributions. *Social Behavior and Personality, 4,* 17–25.

*Lowe, C. A., & Medway, F. J. (1976). Effects of achievement motive on divergent self–other attributions for success and failure. *Perceptual and Motor Skills, 42,* 127–132.

*MacDonald, L. M., & Davies, M. F. (1983). Effects of being observed by a friend or stranger on felt embarrassment and attributions of embarrassment. *Journal of Psychology Interdisciplinary and Applied, 113,* 171–174.

Malle, B. F. (1999). How people explain behavior: A new theoretical framework. *Personality and Social Psychology Review, 3,* 23–48.

Malle, B. F. (2002). The social self and the social other: Actor–observer asymmetries in making sense of behavior. In J. P. Forgas & K. D. Williams (Eds.), *The social self: Cognitive, interpersonal, and intergroup perspectives* (pp. 189–204). Philadelphia: Psychology Press.

Malle, B. F. (2004). *How the mind explains behavior: Folk explanations, meaning, and social interaction.* Cambridge, MA: MIT Press.

Malle, B. F. (2005a). Folk theory of mind: Conceptual foundations of human social cognition. In R. R. Hassin, J. S. Uleman, & J. A. Bargh (Eds.), *The new unconscious* (pp. 225–255). New York: Oxford University Press.

Malle, B. F. (2005b). Self–other asymmetries in behavior explanations: Myth or reality? In M. D. Alicke, D. Dunning, & J. I. Krueger, *The self in social perception* (pp. 157–180). New York: Psychology Press.

Malle, B. F. (in press). Attributions as behavior explanations: Toward a new theory. In D. Chadee & J. Hunter (Eds.), *Current themes and perspectives in social psychology.* St. Augustine, Trinidad: University of the West Indies, School of Continuing Studies.

Malle, B. F., Heim, K., & Knorek, J. (2006). *Storms revisited: Visual perspective and the actor–observer asymmetry in attribution.* Manuscript in preparation.

Malle, B. F., & Knobe, J. (1997a). The folk concept of intentionality. *Journal of Experimental Social Psychology, 33,* 101–121.

Malle, B. F., & Knobe, J. (1997b). Which behaviors do people explain? A basic actor–observer asymmetry. *Journal of Personality and Social Psychology, 72,* 288–304.

Malle, B. F., Knobe, J., & Nelson, S. (2006). *Actor–observer asymmetries in folk explanations of behavior: New answers to an old question.* Manuscript in preparation.

Malle, B. F., Knobe, J., O'Laughlin, M., Pearce, G. E., & Nelson, S. E. (2000). Conceptual structure and social functions of behavior explanations: Beyond person–situation attributions. *Journal of Personality and Social Psychology, 79,* 309–326.

*Malle, B. F., & Pearce, G. E. (2001). Attention to behavioral events during social interaction: Two actor–observer gaps and three attempts to close them. *Journal of Personality and Social Psychology, 81,* 278–294.

Malle, B. F., & Tate, C. (2006). Explaining the past, predicting the future. In L. J. Sanna & E. E. Chang (Eds.), *Judgments over time: The interplay of thoughts, feelings, and behaviors* (pp. 182–209). New York: Oxford University Press.

*Manusov, V., Floyd, K., & Kerssen-Griep, J. (1997). Yours, mine, and ours: Mutual attributions for nonverbal behaviors in couples' interactions. *Communication Research, 24,* 234–260.

*Martin, D. S., & Huang, M. (1984). Effects of time and perceptual orientation on actors' and observers' attributions. *Perceptual and Motor Skills, 58,* 23–30.

Matt, G. E., & Cook, T. D. (1994). Threats to the validity of research synthesis. In H. M. Cooper & L. V. Hedges (Eds.), *The handbook of research synthesis* (pp. 503–520). New York: Russell Sage Foundation.

*McAllister, H. A. (1996). Self-serving bias in the classroom: Who shows it? Who knows it? *Journal of Educational Psychology, 88,* 123–131.

McArthur, L. Z., & Post, D. L. (1977). Figural emphasis and person perception. *Journal of Experimental Social Psychology, 13,* 520–535.

*McGill, A. L. (1989). Context effects in judgments of causation. *Journal of Personality and Social Psychology, 57,* 189–200.

*McKay, J. R., O'Farrell, T. J., Maisto, S. A., Connors, G. J., & Funder, D. C. (1989). Biases in relapse attributions made by alcoholics and their wives. *Addictive Behaviors, 14,* 513–522.

*Medway, F. J. (1979). Developmental aspects of self and other causal attributions. *Psychological Reports, 45,* 155–159.

*Medway, F. J., & Lowe, C. A. (1980). Causal attribution for performance by cross-age tutors and tutees. *American Educational Research Journal, 17,* 377–387.

*Miller, A. G. (1975). Actor and observer perceptions of the learning of a task. *Journal of Experimental Social Psychology, 11,* 95–111.

*Miller, D. T., & Norman, S. A. (1975). Actor–observer differences in perceptions of effective control. *Journal of Personality and Social Psychology, 31,* 503–515.

*Miller, D. T., & Porter, C. A. (1980). Effects of temporal perspective on the attribution process. *Journal of Personality and Social Psychology, 34,* 532–541.

Miller, F. D., Smith, E. R., & Uleman, J. (1981). Measurement and interpretation of situational and dispositional attributions. *Journal of Experimental Social Psychology, 17,* 80–95.

Miller, N., & Pollock, V. E. (1994). Meta-analytic synthesis for theory development. In H. M. Cooper & L. V. Hedges (Eds.), *The handbook of research synthesis* (pp. 457–483). New York: Russell Sage Foundation.

Mischel, W. (1968). *Personality and assessment.* New York: Wiley.

*Mitchell, T. E. (1985). Actor–observer differences in attributions to morality. *Journal of Social Psychology, 125,* 475–477.

*Monson, T. C., & Hesley, J. W. (1982). Causal attributions for behaviors consistent or inconsistent with an actor's personality traits: Differences between those offered by actors and observers. *Journal of Experimental Social Psychology, 18,* 416–432.

Monson, T. C., & Snyder, M. (1977). Actors, observers, and the attribution process: Toward a reconceptualization. *Journal of Experimental Social Psychology, 13,* 89–111.

*Moore, B. S., Sherrod, D. R., Liu, T. J., & Underwood, B. (1979). The dispositional shift in attribution over time. *Journal of Personality and Social Psychology, 15,* 553–569.

Morris, C. G., & Maisto, A. A. (in press). *Understanding psychology* (7th ed.). Upper Saddle River, NJ: Prentice Hall.

Morris, S. B. (2000). Distribution of the standardized mean change effect size for meta-analysis on repeated measures. *British Journal of Mathematical and Statistical Psychology, 53,* 17–29.

Morris, S. B., & DeShon, R. P. (2002). Combining effect size estimates in meta-analysis with repeated measures and independent-groups designs. *Psychological Methods, 7,* 105–125.

Mullen, B., & Riordan, C. A. (1988). Self-serving attributions for perfor-

mance in naturalistic settings: A meta-analytic review. *Journal of Applied Social Psychology, 18,* 3–22.

Myers, D. G. (2004). *Psychology* (7th ed.). New York: Worth Publishers.

*Nesdale, A. R., & Moore, D. (1984). Perspective effects on causal attributions of success and failure. *Australian Journal of Psychology, 36,* 75–83.

*Newman, A. (1978). Actor–observer differences in perception of self-control. *Journal of Social Psychology, 105,* 199–204.

*N'Gbala, A. (1987). L'explication par l'acteur et l'observateur d'une action coherente ou incoherent [How do an actor and an observer explain a consistent or inconsistent behavior?]. *Compartements, 7,* 103–122.

*Nisbett, R. E., Caputo, C., Legant, P., & Marecek, J. (1973). Behavior as seen by the actor and as seen by the observer. *Journal of Personality and Social Psychology, 27,* 154–164.

Nisbett, R. E., & Ross, L. (1980). *Human inference: Strategies and shortcomings of social judgment.* Englewood Cliffs, NJ: Prentice-Hall.

*O'Connor, B. (1988). Actors and observers: Are their descriptions of behavior indications of their causal attributions? *Social Behavior and Personality, 16,* 207–213.

O'Laughlin, M. J., &. Malle, B. F. (2002). How people explain actions performed by groups and individuals. *Journal of Personality and Social Psychology, 82,* 33–48.

*Osberg, T. M., & Shrauger, J. S. (1986). Retrospective versus prospective causal judgments of self and others' behavior. *Journal of Social Psychology, 126,* 169–178.

Ozer, D. J. (1985). Correlation and the coefficient of determination. *Psychological Bulletin, 97,* 307–315.

Rathus, S. A. (2004). *Psychology: Concepts and connections* (7th ed.). Belmont, CA: Thomson/Woodsworth.

Reeder, G. D., & Brewer, M. B. (1979). A schematic model of dispositional attribution in interpersonal perception. *Psychological Review, 86,* 61–79.

*Rejeski, W. J., Rae, T., & McCook, W. (1981). Divergent perceptions of athletic outcomes: A field inquiry into the epistemology of active observer. *Perceptual and Motor Skills, 52,* 139–146.

Richard, F. D., Bond, C. F., Jr., & Stokes-Zoota, J. J. (2003). One hundred years of social psychology quantitatively described. *Review of General Psychology, 7,* 331–363.

*Robins, R. W., Spranca, M. D., & Mendelsohn, G. A. (1996). The actor–observer effect revisited: Effects of individual differences and repeated social interactions on actor and observer attributions. *Journal of Personality and Social Psychology, 71,* 375–389.

Rogers, T. B., Kuiper, N. A., & Kirker, W. S. (1977). Self-reference and the encoding of personal information. *Journal of Personality and Social Psychology, 35,* 677–688.

Rosenthal, R. (1979). The "file drawer problem" and tolerance for null results. *Psychological Bulletin, 86,* 638–641.

Ross, L. (1977). The intuitive psychologist and his shortcomings: Distortions in the attribution process. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 10, pp. 174–221). New York: Academic Press.

Ross, L., & Nisbett, R. E. (1991). *The person and the situation.* New York: McGraw-Hill.

Ross, M., & Fletcher, G. J. O. (1985). Attribution and social perception. In G. Lindsey & E. Aronson (Eds.), *The handbook of social psychology* (Vol. 2, pp. 73–114). New York: Random House.

*Ruble, T. L. (1973). Effects of actor and observer roles on attributions of causality in situations of success and failure. *Journal of Social Psychology, 90,* 41–44.

*Rudy, E. B. (1980). Patients' and spouses' causal explanations of a myocardial infarction. *Nursing Research, 29,* 352–356.

Sabini, J. (1995). *Social psychology* (2nd ed.) New York: Norton.

*Sadava, S. W., & Weithe, H. (1985). Maintenance and attributions about smoking among smokers, nonsmokers, and ex-smokers. *International Journal of the Addictions, 20,* 1533–1544.

*Sagatun, I. J. (1981). Sex differences in attribution: A Norwegian study. *Scandinavian Journal of Psychology, 22,* 51–57.

Sande, G. N., Goethals, G. R., & Radloff, C. E. (1988). Perceiving one's own traits and others': The multifaceted self. *Journal of Personality and Social Psychology, 54,* 13–20.

*Saulnier, K., & Perlman, D. (1981). The actor–observer bias is alive and well in prison: A sequel to Wells. *Personality and Social Psychology Bulletin, 7,* 559–564.

*Schlenker, B. R., Hallam, J. R., & McCown, N. E. (1983). Motives and social evaluation: Actor–observer differences in the delineation of motives for a beneficial act. *Journal of Experimental Social Psychology, 19,* 254–273.

*Schoeneman, T. J., & Rubanowitz, D. E. (1985). Attributions in the advice columns: Actors and observers, causes and reasons. *Personality and Social Psychology Bulletin, 11,* 315–325.

*Seneviratne, H., & Saunders, B. (2000). An investigation of alcohol dependent respondents' attributions for their "own" and "others" relapses. *Addiction Research, 8,* 439–453.

Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). Generalized causal inference: Methods for multiple studies. In W. R. Shadish, T. D. Cook, & D. T. Campbell, *Experimental and quasi-experimental designs for generalized causal inference* (pp. 417–455). New York: Houghton Mifflin.

Shadish, W. R., & Haddock, C. K. (1994). Combining estimates of effect size. In H. Cooper & L. V. Hedges (Eds.), *The handbook of research synthesis* (pp. 261–281). New York: Russell Sage Foundation.

Sheldon, K. M., & Johnson, J. T. (1993). Forms of social awareness: Their frequency and correlates. *Personality and Social Psychology Bulletin, 19,* 320–330.

*Sherrod, D. R., & Farber, J. (1975). The effect of previous actor/observer role experience on attribution of responsibility for failure. *Journal of Personality, 43,* 231–247.

*Sherrod, D. R., & Goodman, C. L. (1978). Effects of sex-of-observer on female actors' causal attributions for success and failure. *Personality and Social Psychology Bulletin, 4,* 277–280.

Skowronski, J. J., & Carlston, D. E. (1989). Negativity and extremity biases in impression formation: A review of explanations. *Psychological Bulletin, 105,* 131–142.

*Small, K. H., & Peterson, J. L. (1981). The divergent perceptions of actors and observers. *Journal of Social Psychology, 113,* 123–132.

*Snyder, M. L., Stephan, W. G., & Rosenfield, D. (1976). Egotism and attribution. *Journal of Personality and Social Psychology, 33,* 435–441.

*Solano, C. H. (1979). Actor–observer differences in attribution: A replication. *Psychological Reports, 44,* 583–586.

Solomon, S. (1978). Measuring dispositional and situational attributions. *Personality and Social Psychology Bulletin, 4,* 589–594.

*Stephan, C., Presser, N. R., Kennedy, J. C., & Aronson, E. (1978). Attributions to success and failure after cooperative or competitive interaction. *European Journal of Social Psychology, 8,* 269–274.

*Stephan, W. G. (1975). Actor vs. observer: Attributions to behavior with positive or negative outcomes and empathy for the other role. *Journal of Experimental Social Psychology, 11,* 205–214.

*Storms, M. D. (1973). Videotape and the attribution process: Reversing actors' and observers' points of view. *Journal of Personality and Social Psychology, 27,* 165–175.

Swann, W. B., & Read, S. J. (1981). Acquiring self-knowledge: The search for feedback that fits. *Journal of Personality and Social Psychology, 41,* 1119–1128.

Taylor, S. E. (1991). Asymmetrical effects of positive and negative events: The mobilization–minimization hypothesis. *Psychological Bulletin, 110,* 67–85.

Taylor, S. E., & Brown, J. D. (1988). Illusion and well-being: A social

psychological perspective on mental health. *Psychological Bulletin, 103,* 193–210.

*Taylor, S. E., & Koivumaki, J. H. (1976). The perception of self and others: Acquaintanceship, affect, and actor–observer differences. *Journal of Personality and Social Psychology, 33,* 403–408.

Taylor, S. E., Peplau, A., & Sears, R. (2006). *Social psychology* (12th ed.). Upper Saddle River: Prentice Hall.

*Teglasi, H., & Fagin, S. S. (1984). Social anxiety and self–other biases in causal attribution. *Journal of Research in Personality, 18,* 64–80.

*Thornton, B., & Ryckman, R. M. (1979). Actor–observer attributions for success and failure in public or private disclosure situations. *Representative Research in Social Psychology, 10,* 87–96.

*Till, A. (1977). Attribution of responsibility in a coin-flipping task. *Social Behavior and Personality, 5,* 311–315.

*Tillman, W. S., & Carver, C. S. (1980). Actors' and observers' attributions for success and failure: A comparative test of predictions from Kelley's cube, self-serving bias, and positivity bias formulations. *Journal of Experimental Social Psychology, 16,* 18–32.

*Uleman, J. S., Miller, F. D., Henken, V., Riley, E., & Tsemberis, S. (1981). Visual perspective or social perspective? Two failures to replicate Storms' rehearsal, and support for Monson and Snyder on actor–observer divergence. *Replications in Social Psychology, 1,* 54–58.

*Van der Pligt, J., & Eiser, J. R. (1983). Actors' and observers' attributions, self-serving bias and positivity bias. *European Journal of Social Psychology, 13,* 95–104.

Watson, D. (1982). The actor and the observer: How are their perceptions of causality divergent? *Psychological Bulletin, 92,* 682–700.

Weary, G., Stanley, M. A., & Harvey, J. H. (1989). *Attribution.* New York: Springer.

Weiner, B., Frieze, I., Kukla, A., Reed, L., Rest, S., & Rosenbaum, R. M. (1972). Perceiving the causes of success and failure. In E. E. Jones, D. Kanouse, H. H. Kelley, R. E. Nisbett, S. Valins, & B. Weiner (Eds.), *Attribution: Perceiving the causes of behavior* (pp. 95–120). Morristown, NJ: General Learning Press.

Wellman, H. M., & Woolley, J. D. (1990). From simple desires to ordinary beliefs: The early development of everyday psychology. *Cognition, 35,* 245–275.

*Wells, G. L., Petty, R. E., Harkins, S. G., & Harvey, J. H. (1977). Anticipated discussion of interpretation eliminates actor–observer differences in the attribution of causality. *Sociometry, 40,* 247–253.

*Wells, K. (1980). Adolescents' attributions for delinquent behavior. *Personality and Social Psychology Bulletin, 6,* 63–67.

*West, S. G., Gunn, S. P., & Chernicky, P. (1975). Ubiquitous Watergate: An attributional analysis. *Journal of Personality and Social Psychology, 32,* 55–65.

White, P. A. (1991). Ambiguity in the internal/external distinction in causal attribution. *Journal of Experimental Social Psychology, 27,* 259–270.

*Whitehead, G. I., Anderson, W. F., & Mitchell, K. D. (1987). Children's causal attributions to self and other as a function of outcome and task. *Journal of Educational Psychology, 79,* 192–194.

Whitley, B. E., Jr., & Frieze, I. H. (1985). Children's causal attributions for success and failure in achievement settings: A meta-analysis. *Journal of Educational Psychology, 77,* 608–616.

Whitley, B. E., Jr., & Frieze, I. H. (1986). Measuring causal attributions for success and failure: A meta-analysis of the effects of question-wording style. *Basic and Applied Social Psychology, 7,* 35–51.

*Wilson, S. R., Levine, K. J., Cruz, M. G., & Rao, N. (1997). Attribution complexity and actor–observer bias. *Journal of Social Behavior and Personality, 12,* 709–726.

*Wolfson, M. R., & Salancik, G. R. (1977). Observer orientation and actor–observer differences in attributions for failure. *Journal of Experimental Social Psychology, 13,* 441–451.

*Wolfson, S. (1997). Actor–observer bias and perceived sensitivity to internal and external factors in competitive swimmers. *Journal of Sport Behavior, 20,* 477–484.

*Wolosin, R. J., & Esser, J. (1975). Effects of justification and vocalization on doctors' and observers' attributions of freedom. *Journal of Personality, 43,* 612–633.

*Wortman, C. B., Costanzo, P. R., & Witt, T. R. (1973). Effect of anticipated performance on the attributions of causality to self and others. *Journal of Personality and Social Psychology, 27,* 372–381.

*Wright, S. J. (1980). Actor–observer differences in attributions for smoking: Introducing ex-actors and attributions for failure to give up. *British Journal of Social and Clinical Psychology, 19,* 49–50.

*Wyatt, S. A., & Medway, F. J. (1984). Causal attributions of students and student-proctors for performance on a university examination. *Contemporary Educational Psychology, 9,* 25–37.

*Yamauchi, H. (1988). Effects of actor's and observer's roles on causal attributions by Japanese subjects for success and failure in competitive situations. *Psychological Reports, 63,* 619–626.

*Yee, D., & Yang, K. (1977). Differences in attributional and coping responses between the actor and the observer under cheating situations. *Acta Psychologica Taiwanica, 19,* 111–124.

*Zaccaro, S. J., & Lowe, C. A. (1985). Effort attributions: Task novelty, perceptual focus, and cue utilization. *Personality and Social Psychology Bulletin, 11,* 489–501.

Zuckerman, M. (1979). Attribution of success and failure revisited: The motivational bias is alive and well in attribution theory. *Journal of Personality, 47,* 245–287.

Received August 8, 2005
Revision received March 17, 2006
Accepted March 18, 2006 ■