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ORIGINAL ARTICLE

Understanding the Third-Person Perception: Evidence From a Meta-Analysis

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The third-person effect hypothesis has generated a vibrant research area. The expansion of this literature poses need for a systematic synthesis of the empirical evidence and assessment of the viability of various theoretical accounts. For this purpose, a meta-analysis is conducted on all published empirical studies of the perceptual component. This analysis covers 60 papers, 106 studies, and 372 effect sizes. Results from a series of multilevel models show that the third-person perception is robust and not influenced by variations in research procedures. Desirability of presumed message influence, vulnerability of referent others, referent others depicted as being similar to self, and others being likely audience of the media content in question are significant moderators. A vote counting analysis is conducted on 124 self–other comparisons in 29 studies from 13 additional papers that do not have the necessary statistical information. Further analyses are conducted to detect and estimate potential publication bias. Based on the empirical synthesis, the paper evaluates several explanatory factors and offers suggestions for future research.

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The third-person effect (TPE) hypothesis (Davison, 1983) depicts a two-part phenomenon: People tend to believe that others are more influenced by media messages than themselves and then act on the basis of such perceptions. The perceptual component, called the third-person perception (TPP), captures the self–other asymmetry in individuals' perceptions about media influences. It shares similar features with other self–other asymmetries in social perception documented in the social psychology literature, such as judgments about traits (Alicke, Klotz, Breitenbecher, Yurak, & Vredenburg, 1995), moral behaviors (Epley & Dunning, 2000), event frequencies (Chambers, Windschitl, & Suls, 2003), and susceptibility to bias (Pronin, Lin, & Ross, 2001), to name a few. TPE, therefore, is one of the theoretical formulations of individuals' perceptual fallacies in their personal and social life (Pronin, Gilovich, & Ross, 2004).

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TPE has a particular appeal to communication scholars also because it represents a unique approach to understanding media effects. It is not about how media as a change agent directly influence people's cognitions or behaviors (Perloff, 2002) but rather how people react to perceived media influences and, consequently, alter various aspects of their social realities. Media messages serve as a context for individuals to imagine the relational presence of others (Gunther, Perloff, & Tsfati, 2007), conjure up how others may think, and devise their own actions.

In recent years, TPE has become "one of the most popular theories" in communication research (Bryant & Miron, 2004). Empirical studies have examined the hypothesis in a wide range of media contexts and about a variety of referent others (see Perloff, 2002) and explored how such self–other asymmetries may occur (e.g., Eveland, Nathanson, Detenber, & McLeod, 1999; Paek, Pan, Sun, Abisaid, & Houden, 2005; Reid & Hogg, 2005). The growing volume of TPE studies has generated a series of reviews (Gunther et al., 2007; Perloff, 1993, 1999, 2002) that provide systematic and insightful readings of the literature. Narrative reviews, however, do not have a formal mechanism for selecting studies, weighing findings, and aggregating them into overall quantitative assessments. A meta-analysis has strengths in these aspects (Hunter & Schmidt, 2004).

A meta-analysis on TPP conducted by Paul, Salwen, and Dupagne (2000) analyzed 121 effect sizes extracted from 32 studies reported in 14 papers. It reported an average effect size of $r = .50$. The study also uncovered three moderators (sampling, respondent, and message) of TPP. Contrary to the unanimous observation in narrative reviews, the study found that desirability of presumed message influence was not a moderator of TPP. As the first available meta-analysis on TPP, Paul et al.'s study has been widely cited. But we see need for a new meta-analysis on TPP. First, there have been many more studies on TPP since the cutoff point of 1998 in Paul et al.'s analysis. Second, as we will discuss in details later, their findings are inaccurate because they overlooked critical differences between within- and between-subjects designs when deriving effect size estimates. Third, more importantly, with a large body of primary studies, we can examine potential moderators to advance our theoretical knowledge (Pfau, 2007).

Theoretical explanations and hypotheses

The first task of our meta-analysis is to synthesize empirical findings by first estimating the magnitude of the self–other perceptual gap and establishing its statistical significance and second, examining the potential effects of "methodological artifacts" (Hunter & Schmidt, 2004), namely, effects of methodological features of the primary studies on effect size estimates. Then, taking meta-analysis as a theory-building tool for assessing theoretical explanations with cumulative data (Hox, 2002), we will test hypotheses of moderator effects derived from the theoretical accounts proposed in the TPP literature.

Various theoretical formulations in the literature can be grouped into two general categories: a motivational account and a cognitive account. Those in the motivational account converge on recognizing TPP as a "self-serving bias" (Gunther & Mundy, 1993) or "self-enhancement bias" (Perloff, 2002). According to this account, individuals are disposed to projecting (inwardly or outwardly) a superior self-image. To defend or enhance one's ego, individuals would deny or downplay their own susceptibility to messages with undesirable influences but acknowledge receptivity to messages designed to impart positive social values. As depicted in the "negative influence corollary" (Gunther & Storey, 2003), TPP typically occurs for messages with presumed undesirable social influences. Such self–other disparity will be reversed in direction for desirable messages, leading to "the first-person perception" (FPP; e.g., Hoorens & Ruiter, 1996). We assess the negative influence corollary by testing the following set of hypotheses:

H1: Self–other perceptual gap varies with the desirability of presumed influence such that (H1a) TPP is significant for media content that is widely recognized as having undesirable or ambiguous influences, (H1b) TPP effect size is larger for media content with undesirable influences than for media content with ambiguous influences, and (H1c) FPP is significant for media content designed to impart desirable social influences.

The need for self-enhancement may also incline people to perceive greater distance with those whom they regard as weak or inferior in certain aspects. Seeing these others as being susceptible to media influences provides an opportunity for individuals to act on their "paternalistic" tendency of protecting the weak (e.g., McLeod, Detenber, & Eveland, 2001). Therefore, when the referent others have certain sociodemographic or psychological attributes that fit the prototype of a "vulnerable" audience, either to media in general or to a specific type of media content, individuals may indicate a larger self–other difference in perceived message effects. This possibility leads to our second hypothesis. Because the direction of the perceptual gap has been predicted to be dependent upon message desirability, the effects predicted in this and all the subsequent hypotheses are above and beyond that of message desirability.

H2: TPP effect size is larger when the referent others are perceived to be particularly vulnerable to media influence in general or the media content in question.

Cognitive explanations trace the self–other asymmetry in perception or judgment to some cognitive roots, attributing it to pitfalls in reasoning, deficiencies in knowledge structure, egocentric mode of cognitive processing, and so on (see Chambers & Windschitl, 2004; Pronin et al., 2004). One idea is based on the attribution theory (e.g., Gunther, 1991). It suggests that individuals tend to attribute message effect on self to the presumed persuasive intent of the message or media power in general but place greater emphasis on dispositional weakness (e.g., gullibility) when evaluating the media effect on others.

The second idea is that perceptions of media or message effect are a particular type of comparative judgments rendered under different degrees of uncertainty or lack of information (Paek et al., 2005). A basic premise for this line of arguments is that individuals have (or feel to have) more privileged knowledge about self than about others. Such knowledge differentials lead individuals to extract information from the descriptor of the referent others about how they are different from or similar to self. When a descriptor signals greater difference from self, individuals will perceive a larger self–other disparity in message effects. The empirical manifestation of this cognitive mechanism is depicted in the "social distance corollary" (Cohen, Mutz, Price, & Gunther, 1988), which states that the more "distant" a referent other is, along either geographic or sociodemographic dimension, the greater the self–other perceptual gap.

The third idea in the cognitive account is along the same line but focuses more on the presumptions about the referent other's exposure to the media message in question. It suggests that individuals, when estimating message effects on various others, consider the likelihood or the group-based normative levels of exposure to the given message. Greater effect of a message is allotted to a referent other presumed to have a higher likelihood of exposure (e.g., Eveland et al., 1999) or belong to a group "known" to have a higher level of exposure (e.g., Reid & Hogg, 2005) to the message.

These ideas, though not incompatible with one another, emphasize different aspects of the cognitive process involved in estimating effect. In this meta-analysis, based on the systematic coding of the descriptors of referent others used in primary studies, we examine the viability of these cognitive ideas by testing H3 through H5.

H3: As the referent others are depicted in terms of increasing geographic distance, there is an increase in effect size.

H4: Effect size is larger when referent others are depicted as from a sociodemographic category different from self than when they are depicted as from the same sociodemographic category as self.

H5: Effect size is larger when referent others are perceived as more likely audience of a particular message type.

Method

Article identification and selection

We started with a systematic search of the following databases: Communication and Mass Media Complete, PsycINFO, ProQuest Research Library, PsycArticles, and Communication Abstract using the following search terms: "third-person perception," "third-person effect," "first-person perception," and "first-person effect."[1] The cutoff point was the end of 2005. The search yielded 126 nonredundant entries. The 13 review articles and book chapters in the list were excluded because they did not report primary research findings. Also excluded were three papers in foreign languages (two in German and one in Spanish), six dissertation abstracts (five of which report quantitative analysis), and six papers from the 2005 Conference of the International Communication Association. After the initial screening, 98 research papers were retained.[2]

These papers were further screened by applying three selection criteria. First, to ensure conceptual coherence (see Hunter & Schmidt, 2004), we excluded nine papers that were built upon the logic of the TPE but did not measure TPP as defined in the classic formulation. Second, a study was selected only if it provided usable statistical information on the difference between effect estimates on self and others. Some studies did not gather self–other mean comparison data (e.g., only the percentage of people claiming greater effects on others than on self was available), did not report data on perceived message effects (e.g., focusing on the behavioral component only, with the data on the perceptual component reported elsewhere), or reported data that were judged problematic (and our repeated queries to the researcher were not answered). Twelve papers that fell under one of these conditions were removed. Finally, some papers reported the same primary data relevant to our analysis. For such cases, we retained only the paper that was judged to best represent the data. Four papers were excluded with this criterion.

The resulting sample has 73 papers (59 more than in Paul et al.'s [2000] study) reporting 135 studies. Based on the notion of statistical independence in meta-analysis (Hedges, 1994; Hunter & Schmidt, 2004), a "study" is defined as a research undertaking that measures the outcomes from an independent sample. If a research project implemented outcome measures on several nonoverlapping groups of participants, then each group is counted as a study.

Unit of analysis

A key assumption in meta-analysis is statistical independence among effect sizes. Typically, a study is treated as the unit of analysis, each contributing one effect size estimate. When multiple effect estimates are reported in a study, the recommendation is to either select only one into the analysis or use an aggregated value (see Cooper, 1989; Hunter & Schmidt, 2004). Both approaches assume that multiple effect size estimates represent complete *conceptual replications* of one another, and thus, such selection or aggregation would not blur potentially important theoretical distinctions.

Such an assumption does not hold in the TPE literature, where multiple tests of self–other disparities are more a rule than exception. Multiple comparisons are often included in a study to capture variations in (a) message contexts/characteristics (e.g., public service announcements vs. commercial ads), (b) characteristics of referent others (e.g., others in one's neighborhood vs. others in the state or country), and/or (c) evaluation domains of perceived effect (e.g., the effect of a message on one's attitude vs. behavior). Such conceptual differentiations within a study are important to preserve for exploring theoretically meaningful moderators. Therefore, the unit of analysis for the present study is each *conceptually distinct* self–other comparison in perceived effect estimates. When multiple comparisons reported in a study can be clearly differentiated in one of the above three aspects, they were recorded as separate entries in the data. For multiple comparisons that were conceptual replications, we aggregated them to yield a single effect size estimate.[3] Following these decision rules, we identified 496 effect size estimates in our sample.

Coding

To examine the conditions under which TPP varies, each unit of analysis was coded on three sets of moderators: research practice characteristics, message characteristics, and referent characteristics. The development of the coding scheme was guided by the following principles: First, we wanted to capture the characteristics of research procedures that may affect effect size estimates, some of which may constitute "methodological artifacts" (Hunter & Schmidt, 2004). For example, respondents rendering perceived effect estimates in natural settings might be more prone to various distractions than those in controlled laboratory settings. Effect size estimates based on single-item measures might be attenuated more by measurement unreliability than those based on multiple-item measures. Second, we coded the variables that would enable us to examine theoretical accounts of TPP in the extant literature. More specifically, our coding should yield data to test the hypotheses set up in the previous section. Third, we also coded other variables for exploratory purposes. The extant literature, for example, has provided little for us to predict how TPP may vary across informational and entertainment categories of media content, persuasive and nonpersuasive media messages, messages in different topical domains, or various effect domains (i.e., cognitive, attitudinal, and behavioral). Incorporating these variables would enable us to garner empirical clues for further theoretical advances in this area.

Table 1 presents all the coded moderator variables and their categories and the intercoder reliability coefficients computed via the "concord" package in the open-source statistics software *R*. To ensure that the coding scheme can be reliably implemented (Krippendorff, 2004), two of the authors independently coded 15 randomly selected papers, which covered 20 studies and 53 effect sizes. After they discussed the discrepancies, the coding scheme was revised and instructions were further clarified. The same two coders then coded 105 effect sizes in 23 studies from another 17 randomly selected papers. Intercoder reliability of each variable was assessed at this stage. For the 16 variables coded, Krippendorff's alpha ranged from .73 to 1.00, all above the acceptable level (.667; Hayes & Krippendorff, 2007; Krippendorff, 2004). The two coders resolved remaining discrepancies through discussion. The lead author then applied the final operational rule concerning each variable and coded the rest of the studies.

Table 1 Moderator Variables and Intercoder Reliabilities (Krippendorff's Alpha)

Variables	Operational Definitions of Categories	α
Research characteristics		
Study setting	Natural versus lab setting	1.00
Data collection method	Self versus interviewer administered	.73
Design	Between-subjects or within-subjects design	1.00
Study population	General public, college or high school students	1.00
Sampling method	Random or nonrandom sampling	.79
Measure for perceived effect	Single- versus multiple-item measures	.88
Domain of perceived effects	Cognitive, attitudinal, behavioral, or unspecified	.94
Message characteristics		
Desirability of message	Undesirable, desirable, or ambiguous	.90
Message topic domain	Health, politics, commercials, entertainment, or general	1.00
Persuasive intent	Persuasive, nonpersuasive, or general media	.93
Functional focus	Informational, entertainment, or no particular focus	.94
Referent characteristics		
Referent other descriptor	Singular versus plural form	1.00
Geographic distance	Neighborhood/community, county, state, nation/general public	.97
Sociodemographic distance	Similar, dissimilar, or no indication	.81
Vulnerable audience	Vulnerable or not vulnerable to the message	1.00
Likely audience	Likely or unlikely audience for the message	1.00

Extracting effect sizes

The basic formulae

The common metric for effect size used in this study is d, which expresses group mean difference in standardized score, as shown in Equation 1 (Cohen, 1988; denoted as g in Hedges & Olkin, 1985):

$$d = \frac{M_O - M_S}{S_{\text{pooled}}}, \tag{1}$$

where M_{O} and M_{S} are group means concerning the referent other and self, respectively, and S_{pooled} is the pooled standard deviation from two groups.

For within-subjects designs, as is the case for most TPP studies, the denominator is still the pooled standard deviation instead of the standard deviation of the mean difference (Dunlap, Cortina, Vaslow, & Burke, 1996; see also Morris, 2000; Morris & DeShon, 2002). Conceptually, this maintains the same metric for the effect size estimates across between- and within-subjects designs (Morris & DeShon, 2002). Statistically, using the standard deviation of the mean difference would lead to overestimating effect size, as it is smaller than pooled standard deviation due to the subtracted correlation between the two repeated measures (i.e., $s_{O-S} = \sqrt{s_O^2 + s_S^2 - 2r_{O,S}s_O s_S}$; see Dunlap et al., 1996).

Effect size can also be derived based on F or t statistics. For the same reason given above, converting the F or t statistics from within-subjects designs without correcting for the correlation between repeated measures would yield an inflated effect size (Dunlap et al., 1996). Equation 2 (from Dunlap et al., 1996) should be used as the appropriate formula for t-to-d conversion for within-subjects designs. A negative sign is assigned to d if the estimate on self is larger than that on others.

$$d = t \times \sqrt{\frac{2(1 - r_{O,S})}{n}} \tag{2}$$

As has been noted by a number of scholars (Hedges & Olkin, 1985; Hunter & Schmidt, 2004; Morris, 2000), d is a biased estimator of the population effect size δ. The second step is thus to correct the bias in d. Applying the "bias function" equations provided in Morris (2000) and Morris and DeShon (2002), we obtained the unbiased estimates of d for subsequent analyses.

Finally, for each effect size, the sampling error variance was computed using Equation 9 in Morris (2000) for within-subjects designs and Equation 22 in Morris and DeShon (2002, p. 13) for between-subjects designs.

Procedure

Not all the papers have the primary statistical information for extracting effect size. Additional information was solicited from their authors when needed. For 320 effect sizes reported in 88 studies from 47 papers, Equation 1 or 2 was used to compute effect sizes. For 52 effect sizes reported in 18 studies from 13 papers that had provided t values only, we obtained an imputed correlation coefficient ($r_{O,S}$) between perceived effects on self and other. To get a reasonable and defendable imputed value, we performed a meta-analysis on the 234 correlation coefficients available in our data, following the bare-bones meta-analysis approach for common metric r outlined by Hunter and Schmidt (2004). The weighted average coefficient was .37, with the 95% confidence interval delimited by 0.35 and 0.39. Then, for each test in this category, one of these three values was randomly selected as the imputed $r_{O,S}$ and plugged into Equation 2 to compute the effect size. This strategy is theoretically justifiable and practically acceptable (Malle, 2006; Morris & DeShon, 2002). To test whether such imputation would have any impact on effect size estimate, we created an additional moderator (i.e., using imputed r or not). Subsequent analysis showed that using the imputed r made no difference.

The remaining 29 studies from 13 papers reported only the means of perceived effects for a total of 124 self–other comparisons. Without any information on variance, it is impossible to compute individual effect sizes. For these studies, we applied the vote counting procedure developed by Hedges and Olkin (1980) to produce one overall effect size estimate based on the proportion of hypothesis-consistent results.

A multilevel modeling approach to meta-analysis

As many scholars have noted, meta-analysis data have an embedded hierarchical structure (e.g., Hox, 2002; Raudenbush & Bryk, 2002), with the individuals who supplied the data for primary studies constituting the units at the first level. In practice, the data available for meta-analysis are at the effect size level, represented by d and the corresponding sampling error variance σ_e^2. This feature of the meta-analytical data creates a special case for the family of multilevel models with Level 1 variance known (Raudenbush & Bryk, 2002).

As explained earlier, multiple effect sizes can be extracted from a study, creating a situation where effect sizes are nested in studies. One important consequence of this nested structure is that effect sizes in the same study are not statistically independent. The violation of the independence assumption would result in underestimation of the sampling error and consequently erroneous statistical inferences about not only the significance of the overall effect size but also the moderator effects (Hunter & Schmidt, 2004).[4] Multilevel modeling, by specifying effect sizes as being clustered under the higher level unit, is designed to address this thorny problem (Hox, 2002). Further, given that studies vary in number of effect sizes, the multilevel modeling approach will correct such imbalance among studies, so that studies contributing a large number of effect sizes will not be overweighted.

In addition, multilevel modeling incorporates the fixed- and random-effects models into the same analytical framework, as shown in Equation 3:

$$d_{jk} = \delta + \mu_k + \mu_{jk} + e_{jk}, \tag{3}$$

where, $j = 1, 2, 3 \ldots J$ effect sizes, $k = 1, 2, 3 \ldots K$ studies.

This is an "intercept-only" model in that no moderator is included as a predictor. It estimates the average effect size δ, the deviation of the average effect size of a study from the grand mean (μ_k), and the deviation of each effect size in the kth study from the grand mean (μ_{jk}). The latter two terms have the variance of σ_k^2 and σ_{jk}^2, respectively. The error term e_{jk} is the known sampling error at the individual level and supplied as data input.

If the two random-effects variances (σ_k^2 and σ_{jk}^2) do not differ from 0, the observed variation among effect sizes is then entirely due to the sampling error. If so, we would end the analysis with evidence for a fixed-effects model—that is, a single estimate of the population effect size (δ). If one or both of the random-effects variances are significant, then there are systematic variations, or heterogeneity, among effect sizes. More explanatory variables (moderators) are to be added to the model (Hox, 2002; Raudenbush & Bryk, 2002).

Analyses were carried out via a series of mixed linear regression models by using the GLLAMM module in Stata (Rabe-Hesketh, Skrondal, & Pickles, 2004). First, we estimated the "intercept-only model." Second, message desirability was added to Equation 3 to form "the desirability model." Third, we added other moderators and estimated "the full model."

Results

The overall effect size: The intercept-only model

The results from this model are presented in the top panel of Table 2. The average effect size for TPP is significant: $d = 0.646$ ($z = 14.85$, $p < .001$), with the 95% confidence interval ranging from 0.56 to 0.73 (expressed in common metric r, this effect size is .307). Falling between the "medium" ($d = 0.50$) and the "large" ($d = 0.80$) effect as conventionally characterized (Cohen, 1988), this mean estimate renders strong support for the TPP hypothesis.

Compared with the result from the previous meta-analysis (Paul et al., 2000), $r = .50$ (or $d = 1.15$), our analysis yields a much smaller estimate. Part of the "shrinkage" may have come from including more studies testing the FPP in our sample. Another major reason is that Paul et al. used the formula based on between-subjects designs, which is inappropriate, to compute all effect sizes. Using the appropriate formula (i.e., Equation 1 or 2, depending on a study's design), we reanalyzed a subset of 35 effect sizes included in their analysis for which necessary information was provided in the primary studies. We replicated Paul et al.'s results using the equations they applied, yielding $\bar{r} = .43$. Using the right formulae led to $\bar{r} = .29$. Their overestimation was substantial and significant ($\Delta\bar{r} = .14$, $z = 3.06$, $p < .001$).

Results from the intercept-only model also showed evidence of heterogeneity among effect sizes. The random-effects variance is .339 and significant ($z = 12.11$, $p < .001$) across effect sizes, and .065 and significant ($z = 3.25$, $p < .001$) across studies. Clearly, there are systematic differences unexplained by this model.

The moderating effect of desirability: The desirability model

The middle panel of Table 2 presents the results of estimating the model with desirability of presumed influences added. Effect sizes clearly differed across the three message categories: desirable ($d = -0.168$), ambiguous ($d = 0.645$), and undesirable messages ($d = 0.862$). Compared with that of ambiguous messages, the average effect size was significantly smaller ($\beta = -.81$, $p < .001$) for desirable messages and significantly larger ($\beta = .22$, $p < .01$) for undesirable messages.

For the ambiguous and undesirable categories, the average effect size was statistically significant ($p < .001$ in both cases). The average effect size for desirable messages flipped to the other direction, revealing an FPP. However, this estimate fell short of statistical significance ($z = -1.89$, *ns*) with the 95% confidence interval including 0 (from −0.34 to 0.01). The results, therefore, support both H1a and H1b but not H1c.

The bottom panel of Table 2 shows that including message desirability in the model reduced the random variance by .077 at the effect size level and by .032 at the study level. This corresponds to, following Raudenbush and Bryk's (2002, Equation 4.12) interpretation approach, 22.7% reduction of the variance-to-be-explained at the effect size level and 49.2% at the study level. The likelihood ratio test showed that these reductions together were significant ($\chi^2 = 102.18$, $df = 2$, $p < .001$).

Table 2 Average Effect Size Estimates (Multilevel Regression With Maximum Likelihood Estimation)

	K	d	r	SE_d	95% CI_d	z	$\sum N_i$	σ_u^2 at Effect Size Level	σ_u^2 at Study Level	Deviance
Overall estimate	372	0.646	.307	.043	0.56–0.73	14.85***	87,058	.339 (.028)	.065 (.020)	715.28
Estimates by message desirability										
Undesirable	171	0.862	.396	.051	0.76–0.96	16.77***	47,568			
Ambiguous	147	0.645	.307	.053	0.54–0.75	12.24***	28,589			
Desirable	54	−0.168	−.084	.088	−0.34–0.01	−1.89	10,901			
Model improvement with message desirability								.262 (.022)	.033 (.014)	613.10
Reduction of random-effects variance at effect size level								.077		
Reduction of random-effects variance at study level									.032	
Likelihood ratio test ($df = 2$)										102.18***

$^{***}p < .001$.

Though a powerful moderator, message desirability did not explain all the variance among effect sizes. The remaining random-effects variance was .262 ($z = 11.90$, $p < .001$) at the effect size level and .033 ($z = 2.36$, $p < .001$) at the study level, both significant. The evidence of unexplained heterogeneity warranted further exploration of other moderators.

Additional moderators included: The "full" model

To accommodate both the explanatory and the exploratory goals of the moderator analysis in this study, we first examined each moderator separately by adding them one at a time to the desirability model. This step was to assess each variable for its potential moderating role after message desirability was controlled for. The statistical criteria that were considered in selecting a potential moderator included a significant regression coefficient and a significant reduction in the random variance at one or both levels, as determined by the likelihood ratio test. A moderator was selected for the final model if (a) it had been deemed important theoretically in the hypotheses section or (b) it met the statistical criteria, making it empirically relevant.

This procedure led to the selection of nine variables, in addition to message desirability, to be included in the final model. Of these nine, except for the number of effect sizes per study, which was a continuous variable, all the other variables were represented by one or more dummy codes in the model. Depending on the relationship among the subcategories of a given variable, we specified a particular contrast in order to facilitate interpretation. The results of this model are presented in Table 3.

In the category of research practice characteristics, the number of effect sizes in a study had a negative association with the magnitude of effect size estimates ($\beta = -.022$, $p < .001$), indicating that making more self–other comparisons in one study would lead to decreased estimates of self–other differences. For the effect domain variable, a contrast was set up in a sequential order so that each category was compared to the previous one. The result showed no difference in effect size whether effect domain was unspecified or specified in terms of attitudinal or cognitive domains. But when perceived effect was specified as on behaviors, the perceptual gap was significantly larger compared to that on attitude ($\beta = .387$, $p < .001$).

In the category of message characteristics, estimating the effects of entertainment content resulted in a significantly smaller self–other perceptual gap ($\beta = -.458$, $p < .05$) than when no content specification is made (i.e., media in general). The effect of the other message variable, informational versus entertainment focus, was reduced to nonsignificance in this multivariate model.

In the category of referent characteristics, five variables were included as moderators. First, when the referent other had characteristics indicative of gullibility to message influence, the self–other perceptual gap was significantly larger ($\beta = .274$, $p < .01$). H2 is supported. Second, the geographic distance of the referent other, specified as the contrast between the two most distant geographic units, did not turn out to be a significant moderator of effect sizes. H3 is not supported. Third,

Table 3 Effects of Other Moderators on Effect Size Estimates

Predictors	β	*SE*	*z* Value
Intercept	.780	.240	3.191***
Ambiguous versus desirable message	.599	.144	4.152***
Undesirable versus ambiguous message	.275	.067	4.103***
Number of effect size per study	−.022	.004	−4.926***
Effect domain			
Cognitive versus unspecified/mixed	.041	.077	.535
Attitudinal versus cognitive	−.061	.115	−.535
Behavioral versus attitudinal	.387	.120	3.213***
Message topic domain (compared to media in general)			
Health	−.257	.160	−1.606
Politics	−.185	.097	−1.918
Commercials	−.036	.107	−.340
Entertainment	−.458	.218	−2.095*
Information versus entertainment as focus	−.348	.206	−1.689
Similar referent versus no similarity reference	−.280	.072	−3.876***
Geographic distance	.068	.104	.653
Plural versus singular descriptor of others	.001	.072	.016
Vulnerable audience	.274	.087	3.163**
Likely audience	.610	.102	6.006***
σ_u^2 at the effect size level	.220	.017	
σ_u^2 at the study level	.000	.000	
Deviance			517.42
Compared to the model with only desirability as the predictor			
σ_u^2 reduction at the effect size level			.042
σ_u^2 reduction at the study level			.033
Likelihood ratio test ($df = 16$)			95.88***

*$p < .05$. **$p < .01$. ***$p < .001$.

explicit cues of sociodemographic similarity in the descriptor of the referent other significantly reduced the self–other perceptual gap ($\beta = -.280$, $p < .001$). H4 is supported. Fourth, when the referent other was judged to have a higher likelihood and/or more exposure to the type of media content in question, the perceptual gap was significantly enlarged ($\beta = .610$, $p < .001$). H5 is supported. Finally, whether the singular or plural descriptor was used to designate the referent other showed no significant impact.

Including these additional moderators decreased the random-effects variance to .22 at the effect size level and to 0 at the study level. Compared to the desirability model, the variance reduction is 16% at the effect size level and 100% at the study level. The likelihood ratio test showed that the combined reduction in random variances at the two levels, when compared with the model with only desirability as the predictor, was significant ($\chi^2 = 95.88$, $df = 16$, $p < .001$).

Vote counting analysis

The vote counting procedure was used to estimate the effect size for the studies that provided no information on variance. It involved counting the number of "positive" results, defined as being in the hypothesized direction (Hedges & Olkin, 1980). We first applied the procedure on all the 124 tests included in this category and then repeated the analysis within each of the desirability subgroups. Across the 124 tests, the estimate of population effect size $\hat{\delta}$ was .193. It was statistically significant, with the 95% confidence interval ranging from 0.128 to 0.256. The subgroup analysis showed that TPP was significant for undesirable ($\hat{\delta}$ = .212, SE_{δ} = .088, $p < .01$) as well as ambiguous ($\hat{\delta}$ = .289, SE_{δ} = .059, $p < .001$) messages. For desirable messages, the mean effect size was in the form of FPP, but again, it did not reach statistical significance (the 95% confidence interval ranging from −0.245 to 0.069). Overall, the findings from the vote counting analyses were consistent with those reported in Table 2.

Publication bias

To see whether publication bias was present in our results, we employed multiple methods. Because none of the methods has been incorporated in multilevel modeling of clustered data, we randomly selected one effect size from each of the studies that had reported full information. This subsample, with 106 statistically independent effect sizes, was used in the analyses on publication bias. First, a funnel graph was plotted on these effect sizes, with the inverse of sampling error as the measure of precision. The funnel plot (Light & Pillemer, 1984) is a primary visual tool for detecting publication bias, the presence of which would lead to asymmetry in the graph. There was no discernible asymmetry in the plot of our data. Because such visual inspection is rather subjective, we used three methods to obtain quantitative assessments of the possible asymmetry in our data. First, the Begg and Mazumdar's (1994) rank correlation test and Egger's linear regression test (Egger, Davey, Schneider, & Minder, 1997) were performed. Results from both tests were nonsignificant, showing no evidence of publication bias. Then, we employed the trim-and-fill procedure developed by Duval and Tweedie (2000), which can both identify and correct for potential publication bias. The method involves first estimating the number of missing studies based on the theoretically assumed symmetry in a funnel plot and then reestimating the mean effect size after filling in the effect sizes from the possible missing studies. Results of this analysis, based on both the random-effects model and the fixed-effects model, showed that no missing studies could be identified. Based on the 106 effect sizes, the fixed-effects model estimate of the mean effect size is d = .649 (SE = .007) and the random-effects model estimate is also d = .649 (SE = .054), very close to what we obtained based on the 372 effect sizes using the multilevel modeling approach.

Discussion and conclusions

This meta-analysis evaluated 17 years of empirical research on the TPP hypothesis (tracing back to the first empirical study published in 1988). Across 372 effect estimates in 106 studies, we obtained an average effect size estimate of d = 0.646 (r = .307). The effect sizes are not moderated by research setting, population, between- or within-subjects design, mode of data collection, or whether single- or multiple-item measures were used in measurement. The evidence is clear that the self–other discrepancy in perceived message effect is a real and robust phenomenon that cannot be attributed to methodological artifact (David, Liu, & Myser, 2004).[5] The effect size, however, is significantly smaller than that reported in Paul et al.'s (2000) study, which, as we demonstrated, contained serious overestimation because they did not (a) apply the right effect size formula for within-subjects designs nor (b) address the statistical dependency among multiple effect sizes extracted from the same study.

The perceptual discrepancy, however, is directional, depending on the desirability of presumed message influence. Contrary to Paul et al.'s (2000) conclusion, message desirability is found to be the most important moderator. The self–other perceptual gap is toward the direction of TPP for messages with undesirable or ambiguous social influences, whereas it is reversed for messages with presumed desirable influence. Although the evidence for FPP is not robust in part due to a much smaller number of effect sizes in the extant literature, it is clear that "the negative influence corollary" (Gunther & Storey, 2003) specifies theoretical boundaries of TPP.

Our conclusion is bolstered by two sets of supplementary findings. First, for studies with incomplete information, which are routinely excluded from meta-analyses, we conducted a vote counting analysis. Second, for the part of the universe that could not be accessed or identified, we conducted diagnostic and sensitivity tests of publication bias. These analyses yielded no evidence that would undermine the validity of our conclusion based on the 372 effect sizes.

Seeking to advance theories through a meta-analysis (Pfau, 2007), we examined the role of the explanatory variables that could be reliably coded from primary studies. Above and beyond the moderating effect of message desirability (H1), the effect size was found to increase significantly when others were regarded as vulnerable (H2). In combination, these results are consistent with the motivational account. However, the evidence is tangential due to the lack of manipulation or direct measurement of motivation in the primary studies. Regarding the cognitive account, our findings suggest that individuals do take some cognitive cues from an immediate evaluation context when rendering their judgment. The perceptual gap is reduced when sociodemographic similarity is cued (H4). This result may be interpreted as supporting the social distance corollary (Cohen et al., 1988). But our findings show that "social distance" in terms of geographic distance is not a significant moderator (H3) when the other referent characteristics are included in the model. Consistent with the exposure hypothesis (Eveland et al., 1999), the self–other perceptual gap is larger when the referent other is judged to be a likely audience member of the media content in question (H5).

We also examined a number of other variables for exploratory purposes. Due to the absence of conceptual explications of message characteristics in the TPE literature, other than desirability of presumed message influence, we could only rely on the minimal message-related information reported in primary studies to explore their potential effects. A couple of interesting findings are worthy of note. The number of effect size estimates in a study had a significant negative effect on the magnitude of effect sizes. Whereas this finding may be indicative of research subjects' fatigue, it may also have a substantive interpretation. That is, people's initial, and often impressionistic, assessment of media message effect may register a larger self–other discrepancy, and repeated comparisons could invoke self-reflection and adjustment mechanisms, resulting in a tendency to reduce such gaps. Our findings also indicate that the self–other gaps are larger when the message effect is evaluated in the behavioral domain than in the attitudinal domain. If nothing else, these findings suggest that future research needs to consider temporal trajectories of individuals' effect estimation and specify effect domains.

Our meta-analysis suggests a few lessons for future research on TPE. The first lesson is that greater effort in future research needs to be devoted to theory development. Simply replicating self–other disparity in perceived effects with another medium, another message type, or another specification of referent others is not going to carry us much further. Theoretical advances can be made more fruitfully by identifying characteristics of target referent and media message in terms of *theoretically explicated* relationships among individuals, target referents, and message content in question.

Second, future research should go beyond showing congruence between empirical results and theoretical notions such as the attribution theory, self-serving bias, and so on. The validity of these theoretical ideas as explanations of TPP must be built upon critical tests. For example, *direct* evidence for the self-serving account of TPP is still lacking, despite the recognition that this account fares well with extant empirical findings (Perloff, 2002); evidence for the social distance corollary also remains deficient in part due to the vagueness of the social distance construct and its varied measures across studies. To perform critical tests of each theoretical account, experiments that manipulate theoretically prescribed explanatory variables or surveys that employ carefully designed and empirically validated scales of such variables are needed.

Third, future TPE research needs to specify not only media messages about which effects are evaluated but also specific domains of effects. As recorded in our database, nearly 40% of the effect sizes are ascertained in ambiguous and general terms such as "media" and "TV." Such vagueness in the causal agent about which individuals render effect estimates makes theory development very difficult. Reflecting the lack of conceptual work on message characteristics in the TPE research, other than desirability of presumed influences, the extant literature offers practically no theoretical account on characteristics of media messages and how they influence individuals' effect estimates. We do not even have a simple taxonomy of messages to formulate predictions of different effect estimates. Similarly, we have very little theoretical knowledge on how people's estimates of message effects would vary across different domains. Very often, the generic term of "media effect" is used to gloss over variations that are worth theorizing about.

Finally, future studies should adhere to the norm of reporting all basic data, including means, standard deviations, and correlation coefficients between perceived effects on self and on each referent for repeated-measure designs, so that readers can better evaluate and make further use of the findings.

Despite our effort, the findings reported in this paper remain limited. First, as shown in Table 3, a significant portion of variation among effect sizes remains unaccounted for. Second, our models are specified to estimate the effect of each moderator that is assumed to be invariant across studies and effect sizes, an assumption that needs to be examined empirically. Third, as effect sizes are shown to differ substantially across message desirability categories, more detailed subgroup analyses are needed. Handling these analytical issues would require the space that this paper cannot provide.

Two other limitations are present, but their sources and treatments are far beyond the scope of this study. One is that our analyses on perceptual gaps were not conducted in conjunction with the behavioral consequences of such gaps. The reason is that the behavioral component of the TPE is poorly explicated (Perloff, 2002), and research in this area does not yet have sufficient conceptual coherence needed for a meta-analytical synthesis. The desired analysis involving the behavioral component would have to await advances in primary research. The other is still publication bias. The problem is a composite of various selection mechanisms (see Dikersin, 2005) that make the total universe of empirical research indefinable. Searching for "fugitive studies" is not a satisfactory solution, nor are the available methods for estimating the extent of such bias. A more comprehensive approach is needed to register empirical studies and compile research literature archives (Berlin & Ghersi, 2005). Developing such an approach requires improving practices of the entire research community.

Notes

1 Our meta-analysis produced a series of technical reports that cannot be included in a journal article. These include a complete reference list of the papers from the search, the coding scheme, the specification of the multilevel models, a complete list of individual effect size estimates for each paper, a complete report of the vote counting results, and comparisons of our effect size estimates with those by Paul et al. (2000). They can be accessed and downloaded at http://ccr.commarts.wisc.edu/suppmat.htm. They are also available from the lead author upon request.

2 Our procedure yields only an accessible subset—published studies in English—of all possible studies, causing concerns over the problem of "publication bias" facing any meta-analytical study. Researchers may take one or a combination of two approaches. One is to conduct an ad hoc search, trying to cover as broad a territory as possible, in order to identify fugitive studies. In practice, this strategy is not free from some "availability bias." For example, conference papers in more recent years are more available or unpublished papers by researchers within the meta-analyst's social network are more accessible. Another strategy is to approach the issue analytically. It takes the

steps of defining the nature of the problem *in the context* of one's meta-analytical study, estimating the potential bias statistically based on the best available data, and qualifying one's meta-analytical conclusion based on the results. As discussed in the Results section, this is the approach we took in this study.

3 One example is our treatment of the study by Huh, DeLorme, and Reid (2004). The study used 22 items to measure perceived effects of direct-to-consumer advertising. The authors factored them into four distinct dimensions both conceptually and empirically. Therefore, we computed the relevant statistics at the factor level to obtain 4 rather than 22 tests. We also combined the "10 other students" and "1,000 other students" categories in Tewksbury's (2002) Study 2 into a single "other students" category. Our rationale is as follows: Though the group size is shown by the author to have a main effect on self–other disparity in perceived effects, this distinction is not examined in any other study. Further, it produces no interaction effect on self–other difference. These two considerations render it undesirable to treat all the effect estimates in the original study as separate entries in our meta-analysis.

4 According to Muthen (1997, p. 456), the amount of underestimation of sampling error variance is $(c - 1)\ \rho$, where c is the cluster size and ρ is the intraclass correlation. For our data, c (the average number of effect sizes per study) is 3.5 and ρ is .13. $(c - 1)\ \rho = (3.5 - 1) \times .13 = .325$. That means 32.5% of the true sampling variance would have been underestimated if simple random sampling had been wrongly assumed.

5 In our analysis, the study population and sampling method did not turn out to be significant moderators, contrary to Paul et al.'s (2000) results. Our best explanation for this inconsistency is that their moderator analysis is based on inaccurate variance estimates. Converting t to d for data from repeated-measure designs without adjusting for correlation between repeated measures could "create an apparent moderator effect where none in fact actually exists" (Dunlap et al., 1996, p. 174). Statistical dependence between effect sizes, as we pointed out earlier, led to undercorrection of sampling error variance and thus inflated Type I error.

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Comprendre la perception de la tierce personne : la preuve d’une méta-analyse

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Résumé

L’hypothèse de l’effet de la tierce personne a généré un domaine de recherche dynamique. Le développement de cette littérature crée le besoin d’une synthèse systématique de la preuve empirique et une évaluation de la viabilité de diverses explications théoriques. À cette fin, une méta-analyse de toutes les études empiriques de la composante de perception fut menée. Cette analyse couvre 60 articles, 106 études et 372 ampleurs de l’effet. Les résultats d’une suite de modèles multiniveau démontrent que la perception de la tierce personne est robuste et n’est pas influencée par des variations dans les procédures de recherche. Des modérateurs significatifs sont la désirabilité de l’influence présumée du message, la vulnérabilité des référents autres, la description des référents autres comme étant similaires à soi et le fait que les autres soient un auditoire probable du contenu médiatique en question. Une analyse par dépouillement est menée sur 124 comparaisons « soi-autre » dans 29 études de 13 articles supplémentaires qui n’ont pas l’information statistique nécessaire. Des analyses additionnelles sont menées afin de détecter et d’estimer de possibles biais de publication. À partir de la synthèse empirique, l’article évalue plusieurs facteurs explicatifs et offre des suggestions pour la recherche future.

Die Third-Person-Wahrnehmung besser verstehen: Ergebnisse einer Metaanalyse

Die Hypothese des Third-Person-Effekts hat ein dynamisches Forschungsfeld hervorgebracht. Der Umfang dieser Literatur erfordert eine systematische Synthese der empirischen Ergebnisse und die Bewertung der Brauchbarkeit der verschiedenen theoretischen Annahmen. Zu diesem Zweck wurde eine Metaanalyse mit allen publizierten empirischen Studien zur Wahrnehmungskomponente durchgeführt. Diese Analyse umfasst 60 Artikel, 106 Studien und 372 Effektstärken. Die Ergebnisse verschiedener Multilevel-Modelle zeigen, dass die Third-Person-Wahrnehmung robust ist und von Variationen der Forschungsabläufe nicht beeinflusst wird. Als signifikante Moderatoren zeigten sich: die Erwünschtheit des angenommenen Botschaftseinflusses, die Verletzbarkeit des anderen, die Darstellung des anderen als ähnlich zum selbst und die Darstellung des anderen als ähnlich zur potentiellen Zielgruppe des Medieninhalts. Für 124 Selbst-Andere-Vergleiche (29 Studien in 13 zusätzlichen Artikeln), die die notwendigen statistischen Informationen nicht ausgewiesen haben, wurde eine Stimmzählungsanalyse durchgeführt. Weitere Analysen dienten der Erfassung und Bewertung möglicher Publikationstendenzen. Basierend auf dieser empirischen Synthese, werden verschiedene Erklärungsfaktoren evaluiert und Vorschläge für zukünftige Forschung unterbreitet.

Comprendiendo la Percepción de la Tercera Persona: Evidencia de un Meta-Análisis

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Resumen

La hipótesis del efecto de la tercera persona ha generado un área de investigación vibrante. La expansión de esta literatura plantea la necesidad de una síntesis sistemática de la evidencia empírica y una evaluación de la validez de varios informes teóricos. Con este propósito, un meta análisis fue conducido sobre todos los estudios empíricos publicados sobre el componente perceptual. Este análisis cubre 60 artículos, 106 estudios, y 372 medidas de efecto. Los resultados de una serie de modelos de varios niveles demuestran que la percepción de la tercera persona es robusta y no influenciada por las variaciones de los procedimientos de investigación. El atractivo de la influencia del mensaje presunto, la vulnerabilidad de los otros referentes, los otros referentes representados como similares a uno mismo, y otros de ser posiblemente la audiencia del contenido de los medios en cuestión son moderadores significativos. Un análisis del conteo de votos es conducido sobre 124 comparaciones entre uno mismo y otros en 29 estudios de 13 artículos adicionales que no tienen la información estadística necesaria. Más análisis son conducidos para detectar y estimar el prejuicio potencial de publicación. Basado en la síntesis empírica, este artículo evalúa varios factores explicativos y ofrece sugerencias para investigaciones futuras.

理解第三人感知：元分析之证据

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第三人效果的假设已引导出一个充满活力的研究领域。该理论文献的扩张使得我们有必要对实证结果进行系统合成并对不同理论论述的有效性进行评估。有鉴于此，我们对所有发表过的有关第三人感知的实证研究进行了综合分析，涵盖 60 篇论文，106 个研究和 372 个效度。通过一系列的多层面模型分析，我们发现第三人感知的效果是强烈的，不受研究程序中变量的影响。重要的中介因素包括所预设之信息影响的渴望度、对比之他人的脆弱度、被描述成与自己类似的对比之他人，和可能成为所涉及媒介内容之受众的他人。我们还对另 13 篇文章（没有包含必要的统计信息）的 29 个研究中的 124 对自我-他人比较进行了数数分析。此外，通过进一步的分析，我们界定、估测了潜在的刊物的偏见。根据实证性的综合分析，本文评估了几个解释性变量，并为未来的研究提供了建议。

제 3 자개념의 이해: 메타분석으로부터의 증거

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요약

제 3 자효과 가정은 역동적인 연구 영역을 산출하였다. 이에대한 문헌의 확장은 여러 이론적 고려들에 대한 실증적인 증거의 체계적 합성과 생존능력의 평가에 대한 필요를 제안하고 있다. 이러한 목적을 위하여, 개념적 요소에 대한 실증적 연구를 단행한 출판들에 대한 메타분석을 실시하였다. 본 분석은 60 개 논문, 106 개 연구, 그리고 372 개 효과 크기들에 대한 것 들이다. 일련의 다양한 수준의 모델들로부터 나타난 결과들은 제 3 자 개념은 매우 견고하며 연구과정에서의 변수들에 의해 영향을 받지 않았다는 것을 보여주고 있다. 추정된 메시지 영향의 바람직함, 언급된 타자들의 취약함, 자아와 유사한 존재로 묘사된 언급된 타자들, 그리고 미디어 내용의 시청자일 것으로 추정되는 타자들이 주요한 매개변수들이다. 본 논문은 또한 필요한 통계적 정보를 가지지 않은 13 개의 추가적인 논문들에 나타난 29 개 연구에서의 124 자기—타자 비교들에 관한 선거개표분석을 실시하였다. 추가적인 분석들이 잠재적인 출판 편견을 발견하고 추정하기 위해 단행되었다. 실증적인 종합에 근거, 본 논문은 여러 설명적 요소들을 평가하였으며, 미래 연구를 위한 유익한 제안들을 제공하였다.