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# Two Meta-analyses Exploring the Relationship between Teacher Clarity and Student Learning

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*This article reports the findings of two meta-analyses that explored the relationship between teacher clarity and student learning. Combined, the results suggest that teacher clarity has a larger effect for student affective learning than for cognitive learning. However, neither the effects for cognitive learning nor affective learning were homogeneous. Heterogeneous effects were observed for several additional subsets of the datasets. The first meta-analysis reviews the findings of 144 reported effects (*N = *73,281) examining the relationship between teacher clarity and student learning outcomes. The cumulative evidence indicates that teacher clarity accounts for approximately 13% of the variance in student learning. The second meta-analysis reports a random-effects meta-analysis of 46 studies (*N = *13,501). Moderators were examined and revealed that study design (i.e., survey versus experiment) moderated the impact of instructor clarity on affective learning. No significant moderators were found for cognitive learning. The cumulative results confirm that teacher clarity has a moderate effect on student affective and cognitive learning; however, persistent heterogeneity among the samples implies the presence of one or more moderating variables. Theoretical, practical, and methodological implications are discussed. Recommendations are made for future clarity researchers including a shift back to using low-inference behavioral measurements instead of high-inference perceptual measurements.*

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*Editor's note: In a rather unusual situation, I received two meta-analyses of the relationship between teacher clarity and student learning within a few weeks of each other. While the two studies had remarkable similarities, they also had some interesting differences in both method and results. With the support of the authors, I suggested presenting the two separate studies within a single article. This format allows the reader to compare and contrast the methods and results. I hope the reader finds this article illuminating for both what these studies tell us about teacher clarity and also for considering methodological choices in meta-analysis. I express my appreciation to the authors for collaborating to present their results in this innovative format.*



Contemporary interest in teacher clarity is generally traced to the initial research efforts of Rosenshine and Furst (Rosenshine, 1971; Rosenshine & Furst, 1971) who summarized the results of over 50 teacher-effects studies and concluded that, among the 11 categories of teacher behaviors related to student learning, teacher clarity had the strongest connection. Stemming from their conclusion, scholars in communication studies and educational psychology have explored a diverse array of questions surrounding the relationship between clarity, student affect, and student learning. Results of these efforts have demonstrated through both observation and controlled experiments that Rosenshine and Furst's evaluation of clarity had merit; research has consistently shown that clarity results in greater student learning.

Despite continued interest in the topic, research on clarity has seemingly become mired in issues of how to define and operationalize clarity, which have dominated entire programs of research on the topic and, as noted by Civikly (1992), have done little to increase precision. While a single definition of clarity might be elusive, research across fields has consistently shown positive relationships between teacher clarity behaviors and student learning. This article presents the results of two meta-analyses that examine the relationship between teachers' clarity behaviors and students' cognitive and affective learning. To provide a conceptual orientation to the topic of teacher clarity, research on the topic is synthesized before turning to the approach used for the meta-analyses.

## Theoretical Foundations of Teacher Clarity

Teacher clarity emerged from a series of early teacher-effects studies in educational psychology. Following a conventional variable analytic tradition, these studies attempted to establish relationships between observed instructor behaviors and student learning (see Brophy, 1986) and, in many cases, did not explicitly attribute observed relationships to broad theoretical positions. That said, scholars in both educational psychology and communication studies implicitly draw on two sorts of theoretical foundations: *information processing* and what loosely could be called *adaptive instruction*.

Information-processing theory dominated educational psychology research conducted in the 1960s and 1970s (Mayer, 1996). Succinctly, this theory views teachers as dispensers of information and learners as information processors. Learners take information that is input into short-term memory by teachers and apply mental operations to the information; through this process, information is added to long-term memory. As teachers enact clarity behaviors, students are better able to attend to, process, store, and retrieve information.

Complementary to the information processing perspective, some clarity researchers assumed that teachers must adapt their clarity behaviors to students through communication (i.e., adaptive instruction). For example, both Civikly (1992) and Simonds (1997) argued that clarity occurs as teachers and students negotiate meaning within the instructional setting. Through ongoing classroom communication, teachers plan and present information; students react through questions, comments, and performance on formal and informal assessment opportunities, and teachers respond as necessary to enhance understanding. This perspective is complementary to information processing because it expands the programming metaphor to be iterative rather than linear. That is, clarity does not happen in one message, but rather, clarity is a process of communication where meanings are negotiated.

These broad theoretical foundations have led to more specific, theory-driven explanations of how clarity influences learning. Mayer (1977), for instance, developed assimilation-to-schema theory to draw distinctions between rote and meaningful learning. Meaningful learning occurs when students receive information, can integrate new information to existing schema, and can then activate appropriate schema to accomplish tasks. Conversely, rote learning simply involves the reception of information. Mayer theorized that advance organizers, or statements explaining what students will learn from a lesson (see Kibler, Cegala, Barker, & Miles, 1974), assist in creating schema, or knowledge structures, to which new information can be assimilated. Thus, as teachers present information and students expand their structures of understanding, their ability to both remember and accurately use information is enhanced. The information processing and adaptive instruction perspectives have provided the foundation for much of our knowledge of teacher clarity.

## Conceptualizing and Operationalizing Teacher Clarity

After Rosenshine and Furst's (1971) observation that clarity was a promising process-product variable, various researchers undertook the task of conceptually and operationally defining the construct. Two groups of scholars, one from educational psychology and the other from communication studies, attempted to devise measurement scales that could be used to assess clarity within classroom situations.

*The Ohio State Low-Inference Studies*

Through the mid-1970s, research exploring teacher clarity suffered from a common problem: the conceptual and operational definition of teacher clarity lacked precision. As explained by Bush, Kennedy, and Cruickshank (1977), "considering the most commonly used definition of teacher clarity, 'being clear and easy to understand,' the difficulties can be readily appreciated. Not only is the common definition circular but, as stated, clarity cannot be directly observed or easily measured" (p. 53). In response to such imprecision, a team of researchers from Ohio State University undertook a large-scale effort to empirically derive a precise operational definition of teacher clarity.

Throughout the teacher-effects studies (Rosenshine, 1971; Rosenshine & Furst, 1971), teacher clarity had been defined and operationalized at a high-inference level. That is, clear teaching was defined as an observer's broad impression that a teacher was either clear or not clear in a given situation (see Rosenshine, 1971). Substantial problems stem from such an approach. As noted by Bush et al. (1977), high-inference variables are ambiguous, difficult to study, and "notoriously weak" when used for training purposes. By way of contrast, low-inference variables are easily observable, consistent in how they are interpreted (i.e., the specific behavior is either present or absent), easily tallied, and trainable. In response to criticisms of high-inference teacher clarity studies, the Ohio State group sought to define teacher clarity in terms of low-inference, observable behaviors and to then relate those behaviors to students' learning. Therefore, *high-inference* clarity behaviors (e.g., being clear) are often vaguely defined and open to subjectivity. *Intermediate-inference* behaviors are less vague and can include clarity dimensions such as "organization" and "explanation." The intermediate-inference dimension of "organization" can be readily observed and understood through a *low-inference* teacher behavior (e.g., clearly previewing the main points of a lecture), which can be clearly observed and objectively quantified.

Initial work by the Ohio State researchers involved asking over 1,000 students in the Columbus Public School system to list five behaviors performed by their clearest teacher (see Bush et al., 1977). Responses to their open-ended survey were used to create items and measurement scales, which were administered to over 1,500 junior high students in Cleveland to determine whether factor structures were present. Examining separate factor analyses for various versions of the scales, two factors were found to be consistent. The first factor was a relatively general dimension involving explaining concepts and directions in an "understandable manner and appropriate pace" (p. 57). Examples of low-inference items associated with the first factor were "Takes time when explaining" and "Gives explanations that the student understands." The second factor dealt specifically with how teachers use examples and illustrations when presenting information. Examples of low-inference items for the second factor included "Gives an example on the board of how to do something" and "Gives students an example and then lets them try to do it."

Given the highly specific sample used in the 1977 study, Kennedy and colleagues (Kennedy, Cruickshank, Bush, & Myers, 1978) attempted to cross-validate the previous findings using a more diverse sample of students from Ohio ($N$ = 425), Tennessee ($N$ = 307), and Australia ($N$ = 531). Results of their study revealed 29 prime discriminators (i.e., items) classified into four dimensions: assesses student learning (e.g., "tries to find out if we don't understand and then repeats things"), provides time to think (e.g., "gives us a chance to think about what's being taught"), uses examples (e.g., "works examples and explains them"), and reviews and organizes (e.g., "prepares us for what we will be doing next").

Although the Ohio State instruments did not result in a legacy of use by other scholars, this program of research undertook important steps in operationalizing and defining a previously opaque construct through identifying low and intermediate-inference dimensions of clarity. The Ohio State studies also provided initial evidence of a broad relationship between teacher clarity, student achievement, and student satisfaction. For instance, Hines, Cruickshank, and Kennedy (1985) studied preservice teachers ($N$ = 202) in the Ohio State University College of Education who were engaging in reflexive teaching activities. Results of that study, coupled with canonical correlations reported in the 1977 study, showed that teacher clarity was positively related to both students' achievement and satisfaction. These findings served as a rationale for continued exploration of the construct.



### *Instructional Communication Studies*

Similar to the Ohio State team, various communication researchers attempted to devise scales tapping students' perceptions of instructor clarity. For instance, Powell and Harville (1990), who defined clarity as "the fidelity of instructional messages" (p. 372), developed a 15-item Teacher Clarity Scale (TCS) based on categories of clarity behaviors found in an unpublished manuscript by Book and McCaleb; no example items were provided in their manuscript. Their 15-item scale was factor analyzed, and it was concluded that a one-factor solution was most appropriate.

Using the TCS's focus on oral clarity as a foundation, Sidelinger and McCroskey (1997) created an expanded 22-item scale that included 10 items from the TCS—that assessed oral clarity—and 12 new items that assessed students' perceptions of oral and written teacher clarity. Although factor analyses showed that the expanded TCS was still one-dimensional, Sidelinger and McCroskey chose to interpret the scale as two-dimensional (oral and written clarity) "for exploratory purposes" (p. 4). Chesebro and McCroskey (1998) later revised the TCS into a shortened version to be more commensurate in length with other measures. The Teacher Clarity Short Inventory (TCSI) contained 10-items that were found to load on a single factor. Example items include "My teacher is straightforward in her or his lecture" and "In general, I understand my teacher."

The TCSI became the *de-facto* option for much of the correlational research on clarity conducted in the communication discipline. For instance, in using the TCSI, Avtgis (2001) found clarity to be correlated positively with students' attributional confidence, and Chesebro and McCroskey (2001) observed that clarity had significant positive relationships with instructor immediacy, student motivation, student affect for the instructor and affect for the course, as well as significant negative correlations with student state receiver apprehension and perceived learning loss. Faylor, Beebe, Houser, and Mottet (2008) surveyed adult learners and found that trainer clarity was the only significant predictor of affective learning in a training environment. In an examination of ninth-grade classrooms, Mottet and colleagues (2008) found that teacher clarity was related positively to teacher immediacy, perceived relevance of information, use of study strategies, and students' affect toward learning, and negatively related to perceived disconfirmation by the instructor.

Guided by Civikly's (1992) treatment of clarity, Simonds (1997) argued for an additional expansion of the clarity construct. Her work resulted in a 20-item, two-dimensional instrument assessing content clarity (e.g., "My instructor is clear when presenting content") and process clarity (e.g., "Asks if we know what to do and how to do it"); however, a factor analysis of the scale revealed only a single dimension of clarity. While communication researchers provided conceptually distinct alternatives to the Ohio State instruments, those alternatives have resulted in a less precise analysis of clarity behaviors because they essentially tap a broad, one-dimensional view of the construct.

## Effects of Teacher Clarity

Although questions of how to define clarity have remained dominant in literature, an impressive program of research examining the effects of clarity on classroom outcomes and processes has emerged. Indeed, multiple outcome variables are important in classroom contexts; however, most studies attempted to determine relationships between clarity behaviors and students' affective and cognitive learning. Such relationships were uncovered throughout a series of studies that emphasized particular linguistic facets of teacher clarity.

### *Smith and Land Language Studies*

In a period from 1979 to 1985, Lyle Smith and Michael Land published over 15 studies exploring specific linguistic dimensions of instructor clarity (e.g., Land & Smith, 1979a; Smith, 1984, 1985a; Smith & Land, 1980). Whereas the Ohio State studies attempted to operationalize clarity as a multidimensional, low-inference variable, Smith and Land isolated specific, low-inference aspects of clarity to serve as independent variables in a series of experiments. Smith and Land not only departed from the Ohio State group through the use of experiments but also addressed the positive bias of the Ohio State studies by exploring what happens when teachers use imprecise language, confusing terms, and other unclear language choices.

Smith and Land's research program extensively explored five variables including vagueness terms, mazes, utterances, bluffing, and uncertainty. *Vagueness terms* are often used by teachers who do not have a sufficient understanding of the material required for effective communication. Vagueness terms include unclear sets of words that mark a recovery point in a lecture (*bluffing*) or reveal a teacher's lack of assurance (*uncertainty*). *Mazes* include false starts and the use of redundant words, while *utterances* are vocalized pauses (e.g., "uh," "ah," "um") that detract from a teacher's level of verbal fluency. Smith and Land designed studies that took various approaches to manipulating these variables. One approach was to isolate individual variables (e.g., word mazes or vagueness terms) and treat them as separate factors in experiments. The "by variable" approach, of which there were seven studies, yielded impressive outcomes. Land and Smith (1979a) observed an effect size of $r$ = .31 in a study manipulating the use of vagueness terms. The other approach was used by Land in five of the studies and involved clustering variables together into "clear" and "unclear" conditions. For example, Land (1981b) combined vagueness terms and mazes, and observed an effect size similar to that observed in the study that only examined vagueness terms ($r$ = .29).

Smith and Land's program of research complemented the Ohio State studies in two important ways. First, their use of experimental designs allowed them to more directly test causal relationships between low-inference clarity variables and student learning using video-taped lectures. Second, their program highlighted a set of low-inference variables, specifically the use of clear and unclear language, not prominently revealed in the Ohio State studies. However, their research was not without limitations. Variables identified by Smith and Land appear to conflate with each other. For example, vagueness terms, mazes, utterances, and uncertainty terms appear on face to be essentially the same variable. That is, the statement, "This lesson might get you to understand a little more about some things we, ah, usually call number patterns," could be coded to contain all of the imprecise variables in their program. Perhaps this observation is what led Land to cluster these variables in several of the studies.

### *Interactive Effects of Teacher Clarity*

Whereas previous sections described what are essentially "main effects" of clarity, scholars have also explored potential interaction effects. One such avenue involved potential interactions between clarity and immediacy. Researchers hypothesized two interaction patterns: the *delivery distraction hypothesis* and the *additivity hypothesis*. As explained by Titsworth (2001b), the delivery distraction hypothesis assumes that highly immediate delivery by the instructor could distract students from attending to and processing details. Conversely, the additivity hypothesis assumes that the positive main effects of immediacy and clarity will combine to create an ideal learning situation for students.

Compelling evidence points in favor of the additivity hypothesis. The additivity hypothesis received support in experiments conducted by Comadena et al. (2007) and Chesebro and McCroskey (1998). In each study, patterns of mean scores show that students benefit from higher levels of immediacy and clarity. Although negligible interaction effects were observed in some of the studies, those effects point to an ordered pattern of mean scores where the high immediacy/high clarity condition was highest and the low immediacy/low clarity condition was lowest in terms of cognitive learning. Although Titsworth (2004) found that high instructor immediacy led students to record fewer details in their notes, another study (Titsworth, 2001a) found that delayed retention was greatest when lectures contained both immediacy and clarity. Thus, while there may be a short-term distraction, in the longer term students benefit from immediate and clear teachers. Consequently, the conclusion that immediacy and clarity are both beneficial and largely work independently of one another seems warranted at this time.

In addition to exploring how clarity interacts with immediacy, Schonwetter, Struthers, and Perry (1995) explored how clarity interacted with students' test anxiety. They manipulated lecture organization (high vs. low) and assessed whether students were high, medium, or low in test anxiety. They found that while low and moderate test-anxious students benefited from organized lectures, high test-anxious students did not. The implication of this finding is that clarity may work differently for students who enter a class with different orientations toward learning, at least in terms of text anxiety.

Finally, several scholars have operated under the assumption that multiple low-inference clarity behaviors work in combination to improve learning (e.g., Chesebro & McCroskey's Profile of the Clear Teacher, 2001). The assumption of these scholars is basically similar to the additivity hypothesis described above in that there is a linear positive relationship between the number of clarity behaviors enacted by an instructor and student learning. Additional research is needed to understand whether there are conditions in which separate clarity behaviors might interact to differentially influence learning.

### *Teacher Clarity and Culture*

Undoubtedly, culture plays a vital role in the communication process as cultural groups often share styles of language, symbols, and beliefs. Working from this assumption, Powell and Harville (1990) explored the relationship between instructor clarity, instructor immediacy, and instructional outcomes for students of white, Latino, and Asian-American ethnic groups. Their results indicated that the positive effects of clarity remained relatively consistent across cultures, suggesting that culture may have little, if any, moderating effect on the clarity–learning relationship.

Extending instructor clarity research from United States classrooms to Chinese classrooms, Zhang and Zhang (2005) used a translated version of the Teacher Clarity Inventory and other pertinent scales to explore the association between instructor clarity, classroom communication apprehension, student motivation, and affective and cognitive learning. Results revealed a significant positive correlation between instructor clarity and student affective learning ($r$ = .44). These findings are consistent with conclusions generated from United States classrooms and suggest that, regardless of culture, clear teaching appears to influence student affect.

Citing a need to explore a possible mediated effect in the clarity–learning relationship, Zhang and Huang (2008) examined the impact of teacher clarity on student learning in United States, Chinese, German, and Japanese classrooms. They argued that motivation functioned as a link between affective and cognitive learning and subsequently hypothesized that affective learning and motivation mediate the effect of teacher clarity on student cognitive learning. Structural equation modeling revealed that when all variables (clarity, motivation, affective learning, and cognitive learning) were included in the model, the effect of clarity on cognitive learning was nonsignificant, which suggested mediation for affective learning and motivation in the clarity–learning relationship. Zhang and Huang's (2008) findings suggest mediation for affective learning and motivation in the immediacy–learning relationship (Zhang & Oetzel, 2006; Zhang, Oetzel, Gao, Wilcox, & Takai, 2007). They concluded that cultural effects might reduce the validity of instructional communication models across cultures. Zhang and Huang (2008) argued for cross-cultural validity testing to better determine the impact of teacher communication behavior on student learning.

### *Negotiation of Clarity*

Much of the teacher clarity research has been situated within the process-product paradigm where teacher clarity is correlated with student outcomes. Put simply, this paradigm centers on the belief that clarity on the part of teachers leads to learning on the part of students. Civikly (1992) reviewed over 50 teacher clarity articles and argued for an expansion of the construct "to include (a) the clarity of the message or content, and (b) the role of the student as clarifier" (p. 138). She argued for a more constructivist view of clarity by including the learner in the teaching, clarity, and learning relationship, emphasizing that clarity highlights the central role of communication in the teaching and learning process.

Stemming from the relational perspective, a series of studies have highlighted the importance of student clarification tactics in the classroom. Darling (1989) studied college classrooms to understand how students signal comprehension problems. Following a series of weekly observations, she found that students commonly utilized three strategies to indicate their lack of comprehension: focused and directive (i.e., there is a problem and the student proposes how to proceed); focused and nondirective (i.e., there is a problem but the student is unclear how to proceed); and personally qualified (i.e., there is a problem and the student focuses on why he or she has the problem). Kendrick and Darling (1990) sought to explore the tactics students use to cope with potential misunderstandings in the classroom. After analyzing students' responses to open-ended survey questions, they determined that students' use of clarifying tactics often vary based on the size of the class, teaching method, and the type of problem experienced. Students tended to request elaboration from the teacher, express their confusion, or ask for examples.

Other scholars have also explored student question asking in the classroom as part of the clarity process. Pearson and West (1991) found that, as a class, students tended to ask an average of three questions of clarification and procedure in each hour of instruction. Students who typically asked questions were found to be independent and self-confident learners. In a later study, West and Pearson (1994) examined the types of questions students ask in classrooms and what instructors say before and after each type of student question. An analysis of classroom audiotapes and transcriptions revealed six categories of student questions: classroom procedures, general inquiry (content), clarification, confirmation, general inquiry (teacher), other. With respect to what instructors say, West and Pearson found that an instructor's question most often served as the antecedent to a student's question.

### Summary

Although multiple outcome variables are important in classroom contexts, many of the clarity studies have attempted to discern main effects for clarity, either through manipulated experimental conditions or through correlational designs, on students' cognitive and affective learning. The findings related to students' affective learning are consistently positive: higher levels of instructor clarity are associated with higher levels of student affect (e.g., Chesebro, 2003; Comadena, Hunt, & Simonds, 2007; Titsworth, 2001a), the state of psychological and emotional arousal toward the teacher, subject matter, instructional, and approach (Bloom, Englehart, Furst, Hill, & Krathwohl, 1956).

Findings from studies examining the relationship between teacher clarity and student cognitive learning indicate that higher levels of clarity are associated with higher levels of student learning (e.g., Chesebro, 2003; Chesebro & McCroskey, 2001; Titsworth, 2001a, 2001b). Cognitive learning refers to the extent to which students achieve factual, conceptual, and critical understanding of course content (Bloom et al., 1956). Results of the Smith and Land studies reveal that higher levels of vagueness, mazes, or uncertainty—all low-inference behaviors associated with poor clarity—are associated with lower levels of cognitive learning for students (e.g., Land & Smith, 1979a; Smith, 1984, 1985a; Smith & Land, 1980). Given that nearly all of these studies reported results of controlled experiments, the conclusion that clarity causes higher levels of student learning appears to be practically unquestionable. However, because inconsistencies of measurement and controversies over interpretation can render narrative literature reviews subjective and imprecise, the quantitative objectivity of meta-analysis was employed. Meta-analytical reviews can potentially clarify contradictory findings and reaffirm long-recognized relationships (Hunter & Schmidt, 1990). The remaining sections describe the methodology, findings, and implications of two meta-analyses of teacher clarity research.

## Meta-analysis 1

Scott Titsworth and Joseph P. Mazer

### Method

#### *Literature Search*

Manuscripts were identified using electronic databases including EBSCO and ERIC as well as manual inspection of citations in located articles and books. Studies were included in the analysis if they (a) employed clarity as a quantitative variable and (b) reported sufficient statistical information to determine an effect of clarity on either cognitive or affective learning. Following recommendations of Lipsey and Wilson (2001), articles reporting more than one effect were subdivided by effect. For instance, the Spicer and Bassett (1976) manuscript exploring the effects of organizational cues on learning reported effects for two separate recall tests, a selected response (i.e., multiple choice) test and a free recall (i.e., essay-type) test. For that study, both effects were entered separately into the database. This selection and division process yielded a total of 49 entries into the database; a total of 28 manuscripts are represented from this step in the process.

Meta-analytic datasets are constrained by the relative completeness of the data included. In addition to relying on recent studies contained in national databases, we also explored the possibility of unpublished effects present in dissertations or theses. That search yielded a dissertation reporting a meta-analysis of studies in the field of education spanning a time period from 1970 to 1986 (Fendick, 1990). Inspection of the studies reported in that dissertation yielded 92 additional effects that could be added to the database for the current study.

The first selection criterion, selecting studies that employ clarity as a variable, deserves some additional explanation. As discussed in the literature review, clarity lacks precision at the definitional level (Civikly, 1992). As such, the decision to employ this criterion is potentially challenging. For instance, should articles be included that explore what could be considered clarity behaviors (e.g., use of advance organizers) but do not explicitly reference clarity as a construct? In an attempt to isolate the clarity construct as described in conceptual reviews (e.g., Titsworth & Mazer, 2010), we decided to include articles that either explicitly identified clarity as a variable or were cited in such manuscripts as representative of what constitutes clarity. Functionally, this decision led us to include nine effects reported by Smith and Land (e.g., Land & Denham, 1979; Land & Smith, 1979a; Smith & Land, 1980) because literature has generally considered their program of research as typical of other clarity studies. In addition, effects reported in the Fendick dissertation were included because the dissertation explicitly considered all contained effects as pertaining to teacher clarity. All other effects included in the database derived from manuscripts explicitly identifying clarity as a variable. Excluded from the database are studies exploring very specific behaviors like using advance organizers or use of visual cues like PowerPoint because they do not explicitly link those behaviors to intermediate or high-inference manifestations of clarity. For instance, a study by Cierniak, Scheiter, and Gerjets (2009) explored whether use of complex or simple visual graphics, similar to PowerPoint, could trigger a split-attention effect for learners. That study did not explicitly reference clarity literature, nor did it cite clarity as an underlying mechanism for learning. While issues of clarity are potentially related to the use of graphics, manipulation of clarity was explicitly not part of the study, so it was not included in the present analysis. To include such studies would reasonably expand teacher clarity to include everything, which would damage any attempt at advancing a parsimonious analysis of the construct.

*Analytical Approach*

Once the database of articles was generated, statistical information in the articles was used to calculate an effect size representing the relationship between clarity and cognitive learning and/or clarity and affective learning. The correlation coefficient *r* was selected as a common metric to represent effects across studies; in cases where studies did not report *r*, common formulae found in Hunter and Schmidt (1990) were used to convert available information into the *r* statistic. Reliability information, when available, was used to correct observed correlations for attenuation due to unreliability; where reliability information was missing or in instances where variables were manipulated, a reliability value of 1 was entered. This approach is more conservative since it would yield less correction on the estimated value for *r*. Values reported in Table 1 show observed correlation coefficients ($r$) for each study as well as the corrected coefficients ($r'$). It should be noted that several of the effects reported in the Fendick (1990) dissertation lacked precise values for *N* because classes were analyzed as the unit of analysis. For those effects, it was not possible to weight the reported *r* value by the number of participants.

After identifying observed and corrected correlation coefficients, the researchers coded each effect to identify ways in which clarity was operationalized (e.g., use of a particular measure or manipulation approach in an experiment), the outcome variable being explored (i.e., affective or cognitive learning), the discipline of origin, and the method employed to assess the outcome variable. Effects were also coded as either being an experimental or correlational design.

*Statistical Analysis*

Meta-analytic studies are useful for aggregating effects across a body of literature. Unlike vote-counting approaches, where studies are tallied as either significant or not significant, meta-analyses allow researchers to aggregate information about the relative magnitude of an effect. This is advantageous for several reasons. First, null hypothesis significance testing, the basis for vote-counting approaches, is subject to both Type I and Type II error. Thus, in a given study either type of error could potentially lead to a miscounting of that study's result. Variance-centered meta-analysis focuses attention on the magnitude of the effect, regardless of statistical probability levels reported in the original study. When quantitatively summarizing effects across studies, researchers are also able to weight effects by the sample contributing to that effect. Thus, in the aggregate effects reported here, studies with larger sample sizes have greater influence on the effect size estimate than do studies with smaller sample sizes.

Average effects, weighted by sample size (when available), were calculated for the overall data set as well as for two subgroups of studies: those using cognitive learning as an outcome variable and those using affective learning as an outcome variable. Each effect was assessed for homogeneity of variance using the chi-square statistic (see Arthur, Benett, & Huffcutt, 2001). Effects with accompanying significant chi-square values were deemed heterogeneous, and post hoc analyses were used to

**Table 1** Studies Reporting Relationships between Teacher Clarity and Students' Learning[a]

| Study[b] | Design | Field | Clarity[c] | Outcome[d] | *r* | *r′* [e] | *N* |
|---|---|---|---|---|---|---|---|
| Chesebro (2003) | Exp | COMS | Cluster | CL-Selected Response | .36 | .39 | 192 |
| Chesebro and McCroskey (2001) | Corr | COMS | TCSI | CL-Perception | .52 | .54 | 360 |
| Comadena et al. (2007) | Exp | COMS | Present | CL-Selected Response | .50 | .50 | 233 |
| Denham and Land (1981) | Exp | EDUC | Cluster | CL-Selected Response | .75 | .75 | 30 |
| Evans and Guymon (1978) | Exp | EDUC | Cluster | CL-Selected Response | .61 | .61 | 19 |
| Hines et al. (1985) | Corr | EDUC | Observ. | CL-Perception | .63 | .87 | 202 |
| Land and Denham (1979) | Exp | EDUC | Cluster | CL-Selected Response | .30 | .30 | 78 |
| Land and Smith (1979a) | Exp | EDUC | Vague (neg) | CL-Selected Response | .31 | .31 | 50 |
| Land (1981a) | Exp | EDUC | Cluster | CL-Selected Response | .76 | .76 | 41 |
| Land (1981b) | Exp | EDUC | Cluster | CL-Selected Response | .29 | .29 | 84 |
| Land (1980) | Exp | EDUC | Cluster | CL-Selected Response | .28 | .28 | 77 |
| Land (1979) | Exp | EDUC | Cluster | CL-Selected Response | .30 | .30 | 78 |
| Smith and Land (1980), effect 1 | Exp | EDUC | Vague (neg) | CL-Selected Response | .15 | .15 | 160 |
| Smith and Land (1980), effect 2 | Exp | EDUC | Mazes (neg) | CL-Selected Response | .19 | .19 | 160 |
| Smith (1984), effect 1 | Exp | EDUC | Uncert (neg) | CL-Selected Response | .14 | .14 | 160 |
| Smith (1984), effect 2 | Exp | EDUC | Notes | CL-Selected Response | .21 | .21 | 160 |
| Smith (1985a), effect 1 | Exp | EDUC | Uncert (neg) | CL-Selected Response | .15 | .15 | 448 |
| Smith (1985a), effect 2 | Exp | EDUC | Notes | CL-Selected Response | .20 | .20 | 448 |
| Spicer and Bassett (1976), effect 1 | Exp | EDUC | Org Cues | CL-Selected Response | .25 | .29 | 95 |
| Spicer and Bassett (1976), effect 2 | Exp | EDUC | Org Cues | CL-Free Recall | .30 | .43 | 95 |
| Titsworth (2001b) | Exp | COMS | Org Cues | CL-Free Recall | .30 | .30 | 223 |
| Titsworth and Kiewra (2004), effect 1 | Exp | COMS | Org Cues | CL-Detail Test | .50 | .53 | 60 |
| Titsworth and Kiewra (2004), effect 2 | Exp | COMS | Org Cues | CL-Organization Test | .69 | .70 | 60 |
| Titsworth (2004), effect 1 | Exp | COMS | Org Cues | CL-Note Details | .35 | .36 | 50 |
| Titsworth (2004), effect 2 | Exp | COMS | Org Cues | CL-Note Organization | .60 | .62 | 50 |
| Zhang and Huang (2008) | Corr | COMS | TCSI | CL-Perception | .56 | .61 | 695 |
| Zhang and Zhang (2005) | Corr | COMS | TCSI | CL-Perception | .33 | .37 | 421 |

Table 1 (*Continued*)

| Study[b] | Design | Field | Clarity[c] | Outcome[d] | $r$ | $r'$ [e] | $N$ |
|---|---|---|---|---|---|---|---|
| Armento (1977), effect 1 | Corr | EDUC | Explanation | CL-Selected Response | .46 | .50 | 288 |
| Avtgis (2001), effect 1 | Corr | COMS | TCSI | AL-Motivation | .67 | .72 | 180 |
| Avtgis (2001), effect 2 | Corr | COMS | TCSI | AL-Overall | .69 | .74 | 180 |
| Chesebro (2003), effect 1 | Exp | COMS | Cluster | AL-Course | .37 | .41 | 187 |
| Chesebro (2003), effect 2 | Exp | COMS | Cluster | AL-Instructor | .39 | .42 | 187 |
| Chesebro and McCroskey (2001), effect 1 | Corr | COMS | TCSI | AL-Course | .74 | .80 | 360 |
| Chesebro and McCroskey (2001), effect 2 | Corr | COMS | TCSI | AL-Instructor | .53 | .58 | 360 |
| Comadena et al. (2007) | Exp | COMS | Structuring | AL-Overall | .38 | .39 | 231 |
| Comadena et al. (2007) | Exp | COMS | Structuring | AL-Motivation | .52 | .54 | 231 |
| Faylor et al. (2008) | Exp | COMS | Effective vs. Ineffective | AL-Course | .37 | .39 | 106 |
| Faylor et al. (2008) | Corr | COMS | TCSI | AL-Course | .45 | .49 | 106 |
| Hines et al. (1985) | Corr | EDUC | Cluster | AL-Satisfaction | .46 | .51 | 202 |
| Houser and Frymier (2009) | Corr | COMS | TCR | AL-Overall | .64 | .67 | 397 |
| Mottet et al. (2008), effect 1 (math) | Corr | COMS | TCSI | AL-Overall | .38 | .42 | 157 |
| Mottet et al. (2008), effect 2 (non math) | Corr | COMS | TCSI | AL-Overall | .39 | .43 | 233 |
| Powell and Harville (1990), effect 1 | Corr | COMS | TCS | AL-Class | .53 | .58 | 311 |
| Powell and Harville (1990), effect 2 | Corr | COMS | TCS | AL-Instructor | .54 | .57 | 311 |
| Schrodt et al. (2009) | Corr | COMS | TCSI | AL-Overall | .51 | .56 | 1416 |
| Sidelinger and McCroskey (1997), effect 1 | Corr | COMS | TCS | AL-Content | .60 | .65 | 204 |
| Sidelinger and McCroskey (1997), effect 2 | Corr | COMS | TCS | AL-Enroll | .71 | .75 | 204 |
| Titsworth (2001a), effect 1 | Corr | COMS | SDScale | AL-Instructor | .28 | .31 | 223 |
| Titsworth (2001a), effect 2 | Corr | COMS | SDScale | AL-Course | .16 | .17 | 223 |
| Zhang and Huang (2008), effect 1 | Corr | COMS | TCSI | AL-Motivation | .61 | .66 | 695 |
| Zhang and Huang (2008), effect 2 | Corr | COMS | TCSI | AL-Overall | .65 | .71 | 695 |
| Zhang and Zhang (2005), effect 1 | Corr | COMS | TCSI | AL-Overall | .44 | .50 | 421 |
| Zhang and Zhang (2005), effect 2 | Corr | COMS | TCSI | AL-Motivation | .28 | .32 | 421 |

[a]In the interest of space, the 91 effects from Fendick (1990) are not included. They are available from the corresponding author.
[b]Multiple effects from a given study were reported separately and noted in this column.
[c]The clarity column shows how clarity was operationalized in a given study. Please refer to original articles for explanation.
[d]Descriptors for each effect indicate how the variable was operationalized. For affect, both motivation and satisfaction were included as part of the affective domain of leaning.
[e]The $r'$ column reports the observed correlation corrected for attenuation due to unreliability.
CL, Cognitive Learning; AL, Affective Learning.

explore potential moderating variables. Also, for each effect the 95% confidence interval was calculated and is reported.

## Results

Across the dataset, a strong positive average correlation was observed for the relationship between clarity and the two outcome variables, $r$ = .36, var. = .06, $k$ = 144, $N$ = 73,281. The 95% confidence interval for the average $r$ ranged from .33 to .38. The chi-square test for homogeneity indicated that the effects were heterogeneous, $\chi^2$ = 37565.26 (143), $p$ < .05. Thus, overall clarity accounts for approximately 13% of the variance in student learning outcomes.

Breaking the outcomes down by type of learning, clarity had a larger effect for affective learning, $r$ = .52, var. = .03, $k$ = 25, $N$ = 8,241, than for cognitive learning, $r$ = .34, var. = .06, $k$ = 119, $N$ = 65,040. The 95% confidence intervals for clarity's relationship with affective and cognitive learning ranged from .48 to .54 and from .31 to .36, respectively. Neither the effect for cognitive learning, $\chi^2$ = 37408.58 (118), $p$ < .05, nor affective learning, $\chi^2$ = 785.08 (24), $p$ < .05, was homogeneous. The lack of homogeneity for either the overall effect, or for the effects broken down by outcome variables, indicates that one or more moderating variables are influencing the relationships. Exploring possible subsets of studies in the database to identify homogeneous effects is one approach that can help identify such moderating variables.

One possibility is that various methodological choices made by researchers in individual studies could result in differences for observed effects. Various subsets of studies employing similar methodological approaches were analyzed for homogeneous effects. Heterogeneous effects were found for articles using the TCSI (Chesebro & McCroskey, 1998), $r$ = .56, $k$ = 9, $\chi^2$ = 174.38 (8), $p$ < .05, all studies employing a correlation-based research design, $r$ = .36, var. = .07, $k$ = 111, $\chi^2$ = 37757.09 (110), $p$ < .05, and experimental designs, $r$ = .26, var. = .03, $k$ = 33, $\chi^2$ = 2892.30 (32), $p$ < .05. Thus, different methodological choices did not emerge as a likely moderating variable.

Another possibility is that different understandings of what constitutes teacher clarity could result in divergent ways of operationalizing the variable, which in turn, could lead to differences in observed effects. Specifically, some studies manipulated clusters of clarity behaviors, while others manipulated only specific behaviors. As an example of the clustering approach, Chesebro's (2003) study manipulated several teacher behaviors at once to simulate clear and unclear teaching, as did several of the Land (e.g., Land 1979, 1980, 1981a, 1981b) studies. The clustering approach did not yield a homogeneous effect, $r$ = .38, $k$ = 8, $\chi^2$ = 274.13 (26), $p$ < .05. Other studies took a divergent approach and manipulated only specific clarity behaviors. For instance, Spicer and Bassett (1976) and three studies reported by Titsworth (e.g., Titsworth, 2001b; Titsworth, 2004; Titsworth & Kiewra, 2004) manipulated only the use of spoken and/or written organizational cues used by the teacher. Effect sizes resulting from these studies were also heterogeneous, $r$ = .38, $k$ = 7, $\chi^2$ = 22.52 (6), $p$ < .05.

Finally, we considered the possibility that there are disciplinary differences that could impact how clarity is understood and operationalized. Using only studies reported in communication journals, the observed average $r$ was .50 with a heterogeneous effect, $k$ = 37, var. = .02, $\chi^2$ = 1046.633 (36), $p$ < .05. Studies from education journals also had a heterogeneous effect, $k$ = 110, var. = .07, $\chi^2$ = 39109.63 (109), $p$ < .05, with an average $r$ of .33. An analysis comparing effects reported in the Fendick (1990) dissertation, $r$ = .33, $k$ = 92, var. = .07, $\chi^2$ = 36469.77 (91), $p$ < .05, with those reported in all journal articles published after the dissertation, $r$ = .46, $k$ = 52, var. = .04, $\chi^2$ = 2334.03 (51), $p$ < .05, did not yield a homogeneous effect. This latter analysis shows that although the average effect size differed between the earlier studies reported in the dissertation and those published after, neither average effect could be characterized as consistent.

## Meta-analysis 2

Alan K. Goodboy, San Bolkan, and Scott A. Myers

### Method

*Literature Search/Inclusion Criteria*

To obtain studies that empirically examined instructor clarity and student learning (i.e., affective and cognitive learning), several steps were taken. First, all of the studies summarized in Titsworth and Mazer's (2010) narrative review were included. Second, a keyword search was conducted using the following databases: Communication and Mass Media Complete, PsychInfo, Education Research Complete, Academic Search Complete, and ERIC. The search terms included "instructor clarity," "teacher clarity," and "clarity" coupled with "learning" and "achievement." Third, obtained articles were back referenced to identify other potential studies. Fourth, a call for manuscripts was posted on CRTNET (a communication studies listserve), and was sent by email to all active members of the National Communication Association's Instructional Development Division seeking unpublished convention papers or dissertations examining instructor clarity and student learning (i.e., cognitive learning and/or affective learning). This call yielded several unpublished works that were not accessible in the keyword search.

To be retained for our analyses, studies had to include the following criteria: (a) quantitative measurements and/or manipulations of instructor clarity (i.e., student perceived clarity, manipulations of clarity behaviors, cues, or dimensions), (b) quantitative measurements of student affective learning and/or cognitive learning (i.e., student reported learning/learning loss, achievement tests, quizzes, recall assessments), and (c) necessary statistical information to allow for the calculation/ conversion of effect sizes (in this case, a sample size or subgroup sample sizes along with reports of Pearson $r$, independent group means/standard deviations, or standardized difference in means converted from F-ratios). Convention papers reporting the same data as published articles were not duplicated. A total of 46 studies ($N$ = 13,501) were included in the meta-analysis.

### Effect-size Calculations

Effect sizes were calculated using Comprehensive Meta-Analysis 2.0 (Borenstein, Hedges, Higgins, & Rothstein, 2006) in the metric of $r$. In cases of multiple measurements of the same variable (e.g., clarity parcels, subscales of affective learning), average correlations were calculated (Schmidt & Hunter, 2015). In some experimental studies, when multiple treatment groups were used to manipulate clarity as one of two or more independent variables in a factorial design (e.g., 2 × 2), only the high-clarity groups were compared with low-clarity groups to avoid confounding effects. In other words, moderate-clarity groups were excluded from analysis in factorial experiments. For instance, Smith (1985a) conducted a 2 (uncertainty vs. no uncertainty) × 2 (bluffing vs. no bluffing) × 2 (discontinuity vs. no discontinuity) × 2 (notes handouts vs. no notes handouts) factorial experiment using 16 groups of students receiving various levels of clarity across four manipulations in concert; only 2 of the groups were compared in our analysis (i.e., the high-clarity condition consisted of no uncertainty, no bluffing, no discontinuity, but received notes handouts) and the 14 moderately low- to moderately high-clarity groups were excluded.

### Moderators

Instructor clarity research has used a variety of methodological and measurement decisions in an effort to link different types of clarity to student learning. Based on recommendations that a meta-analysis should identify moderators to explain heterogeneity in findings (Anker, Reinhart, & Feeley, 2010), we examined the following moderators.

*Cognitive learning measurement.* In much instructional communication research, cognitive learning has been operationalized as a perceived learning outcome via student self-report measures such as cognitive learning loss measure (Richmond, McCroskey, Kearney, & Plax, 1987) or the revised learning indicators scale (Frymier & Houser, 1999). In other published research, cognitive learning has been operationalized by performance-based assessments including multiple-choice tests or recall tests about taught content (e.g., the Smith and Land studies). Differences in perceived versus actual learning outcomes have been documented, suggesting that performance-based measures are more objective and valid indicators of cognitive learning (Witt, Wheeless, & Allen, 2004). For instance, Hess, Smythe, and Communication 451 (2001) found that students' perceived learning was unrelated to exam performance, while Witt and Wheeless (2001) found a small correlation that only accounted for 4% of the variance between perceived learning and student recall ability. Because the operationalization of learning through self-report or performance-based tests may influence reports of student learning, the cognitive learning measurement choice was coded as a moderator (0 = perceived/self-reported learning, 1 = achievement tests/quizzes/recall).

*Study design.* Likewise, previous meta-analyses in instructional communication have revealed differential learning effects of effective teaching based on study design. For instance, in another meta-analysis on instructional communication, Witt et al. (2004) found that the effects of instructor immediacy on learning were stronger in survey designs than experimental designs. Thus, similar to previous researchers, in the current manuscript study design was coded as a moderator (0 = survey, 1 = experiment).

*Clarity measurement.* In much of the survey research in communication studies, instructor clarity has been operationalized as a high-inference student perception (e.g., my teacher is clear) in measures such as the TCS (Powell & Harville, 1990), Teacher Clarity Measure (Sidelinger & McCroskey, 1997), or TCSI (Chesebro & McCroskey, 1998). In other research, clarity cues (e.g., organizational cues; Titsworth, 2004) or clarity behavioral clusters (e.g., vagueness cues and mazes; Smith & Land, 1980) have been communicated by instructors and observed by students for low-inference interpretation. Because student perceptions of teaching behavior have been uncorrelated with observer reports of the same behavior (e.g., Smythe & Hess, 2005), it may be the case that students' perceptions of clarity are differentially associated with student learning outcomes compared with behavioral manipulations. Therefore, differences in how instructor clarity was operationalized were coded as a moderator (0 = perceived clarity, 1 = observed clarity).

*Sample.* Finally, instructor clarity research has been conducted using middle school, high school, and college samples. Thus, it is possible that the type of student sample serves a moderator. Therefore, sample type was coded (0 = secondary school student sample, 1 = college student sample).

### *Statistical Analysis*

A random-effects model was chosen over a fixed-effect model because it assumes heterogeneity in effects and is more conservative (i.e., reduces type 1 error; Anker et al., 2010). To calculate a summary effect we used original or transformed correlation coefficients which were converted to Fisher's z scale. All analyses were calculated using the transformed values and confidence intervals, which were then converted back to correlations for interpretation (see Borenstein, Hedges, Higgins, & Rothstein, 2009). To assess the heterogeneity effects, we calculated the $Q$ statistic, $T^2$, $T$, and $I^2$. The $Q$ statistic tests the null hypothesis that the studies share a common effect size and "follow a central chi-squared distribution with degrees of freedom equal to $k - 1$" (Borenstein et al., 2009, p. 112). $T^2$ is an estimate of between-studies variance in effect sizes for a random-effects model, and $T$ is the standard deviation of these effects from the mean effect. Finally, $I^2$ is the "ratio of true heterogeneity to total variation in observed effects" (Borenstein et al., 2009, p. 120). Higgins, Thompson, Deeks, and Altman (2003) assigned heterogeneity values that are low (25%), moderate (50%), and high (75%) for $I^2$.

To examine a potential publication bias in this meta-analysis (i.e., a bias in publishing nonsignificant findings which may overestimate population effect sizes; see Levine, Asada, & Carpenter, 2009), we examined (a) the funnel plots, (b) Rosenthal's classic fail-safe *N* (Rosenthal, 1979), (c) Orwin's fail-safe *N* (Orwin, 1983), and (d) Egger's Test of the Intercept (Egger, Smith, Schneider, & Minder, 1997). Because the moderators were coded as dichotomous groups, subgroup analyses were conducted (Borenstein & Higgins, 2013) by computing omnibus tests of between-groups differences ($Q_B$) for mixed-effects models (Hedges & Pigott, 2004).



## Results

Before interpreting the results of the meta-analysis, publication bias tests were performed because "research published in many journals is more likely to present statistically significant findings" (Cooper, 2010, p. 64). Funnel plots were examined for affective learning and cognitive learning effects. Both funnel plots were generally symmetrical, suggesting a lack of publication bias (see Figures 1 and 2).

To further ascertain the extent to which publication biases were a problem, we examined Rosenthal's (1979) fail-safe *N*. Results from this analysis suggested that 10,416 missing studies would be needed to nullify the effect (i.e., bring the *p*-value to > alpha) for affective learning and 11,728 missing studies would be needed to nullify the effect for cognitive learning. Orwin's fail-safe *N* was examined by setting the criterion for obtaining a trivial/small correlation at $r = .10$. This analysis revealed that 91 missing studies would be needed for affective learning and 148 missing studies would be needed for cognitive learning to reduce *r* to below .10. Egger's regression intercepts were calculated for affective learning (Egger's intercept = 1.274, 95% CI

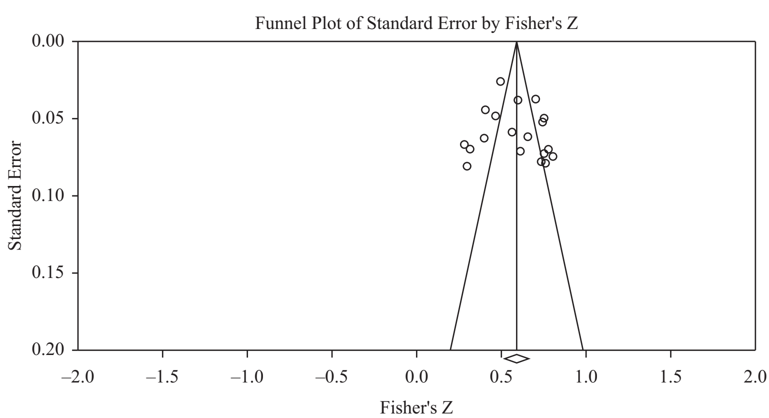


**Figure 1** Funnel plot for affective learning (random effects).

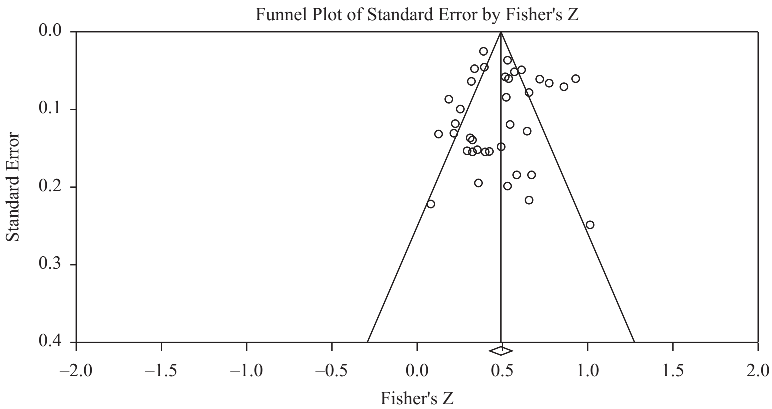


**Figure 2** Funnel plot for cognitive learning (random effects).

[−3.050, 5.598], $t$ = .622, df = 17, $p$ = .27) and cognitive learning (Egger's intercept = −.065, 95% CI [−1.618, 1.487], $t$ = .085, df = 35, $p$ = .47). Neither test revealed statistically significant intercept deviations from zero, indicating there was no asymmetry. Based on these results, publication bias was deemed not to be an issue in this meta-analysis.

After examining the publication bias tests, we conducted the random-effects meta-analysis for learning effects first for affective learning and next for cognitive learning. The meta-analysis of affective learning included 19 studies and a total of 6,889 participants. Results revealed that, together, the studies combined for an average correlation of .53 with a 95% confidence interval ranging from .48 to .58 ($p$ < .001). The results are reported in Table 2.

The analysis of cognitive learning included 37 studies and a total of 6,612 participants. Results revealed that the studies combined for an average correlation of .46 with a 95% confidence interval ranging from .40 to .51 ($p$ < .001). The results are reported in Table 3.

Next, we examined the moderators to test for significant differences between subgroups within affective learning. The test for heterogeneity was significant ($Q$ = 149.21, $p$ < .001) and indicated that a substantial portion of the observed variance reflected real differences in the effect sizes across studies ($T^2$ = .02, $T$ = .15, $I^2$ = 87.94). We first tested the moderator of study design. Because the clarity measurement reported in the research projects matched with methodological design, this moderator analysis includes clarity measurement as well. Results revealed that there were significant differences ($Q_B$ = 18.84, $p$ < .01) between survey reports ($k$ = 15, $r$ = .57, $p$ < .001) compared with experiments ($k$ = 4, $r$ = .32, $p$ < .001). Next, we examined the sample type as a potential moderator. Results revealed that there were no significant differences ($Q_B$ = 1.51, $p$ = .22) between secondary school students ($k$ = 1, $r$ = .39, $p$ < .01) compared with college students ($k$ = 18, $r$ = .54, $p$ < .001).

Following affective learning, we examined the proposed moderators for cognitive learning. The test for heterogeneity was significant ($Q$ = 211.08, $p$ < .001) and

**Table 2** Meta-Analysis for Affective Learning

| | 95% CI | | | | Moderators | | | |
|---|---|---|---|---|---|---|---|---|
| Study | *r* | LL | UL | *Z*-Value | r-w | a | b | c |
| Avtgis (2001) | .670 | .581 | .743 | 10.786 | 4.90 | 0 | 0 | 1 |
| Chesebro and McCroskey (2001) | .635 | .569 | .639 | 14.166 | 5.49 | 0 | 0 | 1 |
| Chesebro (2003) | .311 | .182 | .430 | 4.567 | 5.03 | 1 | 1 | 1 |
| Comadena et al. (2007) | .384 | .274 | .485 | 6.392 | 5.22 | 1 | 1 | 1 |
| Goodboy, Martin, and Bolkan (2009) | .630 | .528 | .714 | 9.437 | 4.80 | 0 | 0 | 1 |
| Houser and Frymier (2009) | .640 | .578 | .695 | 15.049 | 5.55 | 0 | 0 | 1 |
| Malachowski et al. (2011) | .580 | .493 | .655 | 10.620 | 5.24 | 0 | 0 | 1 |
| McCroskey (2003) | .645 | .545 | .727 | 9.637 | 4.77 | 0 | 0 | 1 |
| McCroskey, Richmond, and Bennett (2006) | .640 | .574 | .717 | 10.340 | 4.95 | 0 | 0 | 1 |
| Mottet et al. (2008) | .390 | .313 | .462 | 9.153 | 5.67 | 0 | 0 | 0 |
| Myers, Goodboy, and Members of Comm 600 (2014) | .515 | .424 | .595 | 9.581 | 5.32 | 0 | 0 | 1 |
| Schrodt et al. (2009) | .463 | .421 | .503 | 18.837 | 6.02 | 0 | 0 | 1 |
| Sidelinger and McCroskey (1997) | .655 | .569 | .727 | 11.115 | 5.02 | 0 | 0 | 1 |
| Titsworth (2001b) | .280 | .154 | .397 | 4.267 | 5.11 | 1 | 1 | |
| Toale (2001) | .540 | .484 | .592 | 15.615 | 5.80 | | 0 | 1 |
| Violette (2002) | .295 | .143 | .433 | 3.726 | 4.72 | 1 | 1 | 1 |
| Zhang and Huang (2008) | .610 | .561 | .655 | 18.649 | 5.82 | 0 | 0 | 1 |
| Zhang and Zhang (2005) | .440 | .360 | .514 | 9.655 | 5.58 | 0 | 0 | 1 |
| Zhang (2011) | .550 | .444 | .641 | 8.613 | 4.99 | 0 | 0 | 1 |
| Random Effects Average | .530 | .477 | .579 | 16.369 | | | | |

*Note*. For moderators: a = study design (0 = survey, 1 = experiment), b = clarity measurement (0 = perceived clarity, 1 = observed clarity), c = sample type (0 = secondary school sample, 1 = college student sample). r-w, relative weight (random).

indicated that a substantial portion of the observed variance reflected real differences in the effect sizes across studies ($T^2$ = .03 $T$ = .17, $I^2$ = 82.95). An analysis of the type of cognitive learning as a moderator revealed no significant differences ($Q_B$ = 2.69, $p$ = .10) between perceived/self-reported learning ($k$ = 10, $r$ = .51, $p$ < .01) compared with achievement tests of learning ($k$ = 27, $r$ = .42, $p$ < .001). In addition, study design did not significantly moderate the effect; there were no significant differences ($Q_B$ = 2.82, $p$ = .09) between survey reports ($k$ = 13, $r$ = .51, $p$ < .001) compared with experiments ($k$ = 24, $r$ = .42, $p$ < .001). Moreover, clarity measurement did not significantly moderate the main effect. Results indicated no significant differences ($Q_B$ = 3.52, $p$ = .06) between perceived clarity ($k$ = 12, $r$ = .52, $p$ < .001) compared with observed clarity ($k$ = 25, $r$ = .41, $p$ < .001). Finally, we examined the sample type as a potential moderator. Results revealed that there were no significant differences ($Q_B$ = 2.00, $p$ = .16) between secondary students ($k$ = 9, $r$ = .38, $p$ < .001) compared with college students ($k$ = 28, $r$ = .47, $p$ < .001).

## Discussion

As noted in comprehensive narrative reviews, articles exploring teacher clarity have not identified precise definitions of the concept (see Civikly, 1992; Titsworth &

**Table 3** Meta-Analysis for Cognitive Learning

| | | 95% CI | | | Moderators | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Study | *r* | LL | UL | *Z*-Value | r-w | a | b | c | d |
| Chesebro and McCroskey (2001) | .520 | .440 | .592 | 10.890 | 3.61 | 0 | 0 | 1 | 0 |
| Chesebro (2003) | .733 | .672 | .784 | 15.165 | 3.49 | 1 | 1 | 1 | 1 |
| Comadena et al. (2007) | .495 | .398 | .581 | 8.802 | 3.50 | 1 | 0 | 1 | 0 |
| Dunkin (1978) | | −.019 | .635 | 1.863 | 1.67 | 0 | 1 | 0 | 1 |
| Dunkin and Doenau (1980) | .490 | .146 | .730 | 2.680 | 1.63 | 0 | 1 | 0 | 1 |
| Evans and Guymon (1978) | .770 | .486 | .907 | 4.081 | 1.23 | 1 | 1 | 1 | 1 |
| Henning (2007) | .653 | .570 | .723 | 11.551 | 3.41 | 0 | 0 | 1 | 0 |
| Hiller, Fisher, and Kaess (1969) | .590 | .304 | .779 | 3.649 | 1.78 | 1 | 1 | 0 | 1 |
| Hines, Cruickshank, and Kennedy (1985) | .530 | .222 | .742 | 3.178 | 1.78 | 0 | 0 | 1 | 1 |
| Houser and Frymier (2009) | .550 | .477 | .615 | 12.275 | 3.64 | 0 | 0 | 1 | 0 |
| Land and Denham (1979) | .385 | .099 | .612 | 2.597 | 2.12 | 1 | 1 | 1 | 1 |
| Land and and Smith (1979a) | .307 | .046 | .528 | 2.297 | 2.36 | 1 | 1 | 1 | 1 |
| Land and and Smith (1979b) | .291 | −.004 | .539 | 1.933 | 2.14 | 1 | 1 | 1 | 1 |
| Land (1979) | .405 | .124 | .626 | 2.764 | 2.13 | 1 | 1 | 1 | 1 |
| Land (1980) | .319 | .025 | .562 | 2.119 | 2.12 | 1 | 1 | 1 | 1 |
| Land (1981a) | .218 | −.037 | .446 | 1.677 | 2.45 | 1 | 1 | 1 | 1 |
| Land (1981b) | .086 | −.337 | .481 | 0.386 | 1.43 | 1 | 1 | 1 | 1 |
| Malachowski, Martin, and Frisby (2011) | .620 | .539 | .690 | 11.623 | 3.49 | 0 | | 1 | 0 |
| McCroskey (2003) | .580 | .467 | .674 | 8.327 | 3.24 | 0 | 0 | 1 | 0 |
| Myers et al. (2014) | .480 | .385 | .565 | 8.798 | 3.52 | 0 | 0 | 1 | 0 |
| Rodger, Murray, and Cummings (2007) | .484 | .346 | .602 | 6.163 | 3.14 | 1 | 1 | 1 | 1 |
| Schrodt et al. (2009) | .376 | .330 | .420 | 14.863 | 3.87 | 0 | 0 | 1 | 0 |
| Smith and Bramblett (1981) | .319 | .055 | .541 | 2.351 | 2.33 | 1 | 1 | 0 | 1 |
| Smith and Cotten (1980) | .572 | .378 | .718 | 5.033 | 2.49 | 1 | 1 | 0 | 1 |
| Smith and Edmonds (1978) | .226 | −.005 | .434 | 1.921 | 2.63 | 1 | | 1 | 1 |
| Smith and Land (1980) | .344 | .059 | .578 | 2.342 | 2.16 | 1 | 1 | 1 | 1 |
| Smith (1977) | .380 | .298 | .456 | 8.505 | 3.68 | 1 | 1 | 0 | 1 |
| Smith (1984) | .461 | .203 | .660 | 3.338 | 2.21 | 1 | 1 | 0 | 1 |
| Smith (1985a) | .131 | −.129 | .373 | 0.988 | 2.43 | 1 | 1 | 0 | 1 |
| Smith (1985b) | .189 | .018 | .349 | 2.170 | 3.11 | 1 | 1 | 0 | 1 |
| Solomon, Rosenberg, and Bezdek (1964) | .580 | .231 | .797 | 3.036 | 1.47 | 0 | 0 | 1 | 1 |
| Spicer and Bassett (1976) | .254 | .061 | .428 | 2.568 | 2.91 | 1 | 1 | 1 | 1 |
| Titsworth (2001a) | .313 | .194 | .424 | 4.567 | 3.44 | 1 | 1 | 1 | 1 |
| Titsworth and Kiewra (2004) | .501 | .305 | .657 | 4.963 | 2.61 | 1 | 1 | 1 | 1 |
| Violette (2002) | .700 | .620 | .765 | 12.016 | 3.35 | 1 | 1 | 1 | 1 |
| Zhang and Huang (2008) | .490 | .431 | .545 | 14.102 | 3.77 | 0 | 0 | 1 | 0 |
| Zhang and Zhang (2005) | .330 | .242 | .413 | 7.009 | 3.66 | 0 | 0 | 1 | 0 |
| Random Effects Average | .455 | .402 | .505 | 14.730 | - | - | - | - | - |

*Note.* For moderators: a = study design (0 = survey, 1 = experiment), b = clarity measurement (0 = perceived clarity, 1 = observed clarity), c = sample type (0 = secondary school sample, 1 = college student sample), d = cognitive learning measurement (0 = perceived learning, 1 = achievement test). r-w, relative weight (random).

Mazer, 2010). Results of these meta-analyses provide additional support for that conclusion. Despite strong average effect sizes observed in the data sets, those effects are heterogeneous, which suggests that considerable variability exists in the observed correlations across all of the studies. In fact, in Meta-Analysis 1, post hoc analyses of articles containing similar design characteristics yielded no homogeneous groupings. The presence of heterogeneity could stem from the lack of agreement on exactly what clarity is and how clarity behaviors are enacted in the classroom. Simply put, the conceptual breadth of clarity behaviors, particularly as they are enacted in diverse classroom settings, may make homogeneous effects elusive.

### *Teacher Clarity*

The meta-analyses revealed similarities, including nearly identical cumulative effects for clarity and affective learning (Meta-Analysis 1: $r$ = .52 and Meta-Analysis 2: $r$ = .53). Meta-Analysis 2 found a significant moderator—survey designs had a higher $r$ value than experimental designs. These similarities are important because they replicate findings despite differences in the meta-analytical approach. Despite a lack of homogeneous effects, these meta-analyses provide meaningful evidence concerning the relationship between teacher clarity and student learning. Taking into consideration the 95% confidence interval for the overall effect of clarity on learning, in Meta-Analysis 1, the average $r$ of .36 shows that clarity has a strong relationship with learning. Broken down by type of learning, clarity has a stronger relationship with students' affective learning ($r$ = .51) than their cognitive learning ($r$ = .34). The practical importance of these average effects is illustrated by values reported in the Binomial Effect Size Display (BESD, Rosenthal, 1984) found in Table 4. As shown in the table, clear teaching increases the probability of perceived cognitive learning by over 100% and affective learning by over 200%. Similar results were detected in Meta-Analysis 2 where moderate summary effects for teacher clarity on student affective learning ($r$ = .53) and cognitive learning ($r$ = .46) emerged.

Given that clarity is a cognitive function, it is reasonable to speculate that teacher clarity should have a stronger relationship with students' cognitive learning. The results of the meta-analyses suggest that this is not the case, as the findings reveal that teacher clarity produces a stronger effect on affective learning. Several reasons for this contradictory expectation might exist. First, reasoning immediately points toward a lack of clarity (ironically) in the conceptualization and operationalization of teacher clarity. Despite substantial scholarly interest in teacher clarity, no single method for

**Table 4** BESD Comparing Teacher Clarity and Student Learning Outcomes

| | Probability that a student's score is | | |
|---|---|---|---|
| Teacher clarity | Below median | Above median | Change |
| Cognitive Learning ($r$ = .34) | | | |
| High Clarity | 33% | 67% | 103.03% increase |
| Low Clarity | 67% | 33% | 50.75% decrease |
| Affective Learning ($r$ = .51) | | | |
| High Clarity | 24.5% | 75.5% | 208.16% increase |
| Low Clarity | 75.5% | 24.5% | 67.55% decrease |

assessing the construct has emerged. Whereas a variety of options exist (see Titsworth & Mazer, 2010), the use of a particular scale generally is based on the level of specificity required for a study. High-inference, single dimension constructs are difficult to study and even more difficult to teach; low-inference behaviors, on the other hand, are more realistically assessed or manipulated and are relatively easy to incorporate into teacher training. The Ohio State studies showed that what could count as clarity was broad—so broad that it could be argued that all teaching behaviors generally relate to clarity in some way (Bush et al., 1977). The breadth of what counts as clarity has made conceptual definitions of the construct difficult to generate. As Civikly (1992) noted, definitions of clarity are anything but clear.

Instructional communication researchers might consider reverting to low-inference measurements of instructor clarity and should be specific in the type of clarity they are studying. The high-inference measures (e.g., Chesebro & McCroskey, 1998; Powell & Harville, 1990; Sidelinger & McCroskey, 1997) have certainly uncovered correlates of instructor clarity, but these measurements tell researchers little beyond "my instructor was clear" and ignore the specific behavioral components that (a) promote understanding for students and (b) may lead to differences in perceptions of clarity. Instructional communication scholars would be better advised to conduct more experimental work by manipulating specific clarity dimensions because instructors can be clear/unclear in almost everything they say or do in the classroom.

A second reason for clarity's weaker relationship with cognitive learning could be due to how cognitive learning has been operationalized. Richmond et al.'s (1987) Learning Loss Measure is based on students' rather than instructors' perceptions of learning and consists of two questions: "How much did you learn in this class?" and "How much do you think you could have learned in the class had you had an ideal instructor?" Subtracting one item from the other results in a "learning loss" assessment. The questions have been utilized to operationalize cognitive learning in several teacher clarity studies (Chesebro & McCroskey, 2001; Zhang & Huang, 2008; Zhang & Zhang, 2005). Critics of the Learning Loss Scale report that the reliability is difficult to assess (Rubin, 2009); the measure is not amenable to factor analysis (Hooker & Denker, 2013); learning loss shares a limited relationship with other cognitive learning measures (Hooker & Denker, 2013); the assessment relies on students' perception of what they have learned (Hess et al., 2001); and others might question whether the measure is a true cognitive indicator, since the two items feature students' affective perceptions of learning, without benefit of exam or test scores. Alternative methods of measuring cognitive learning such as selected response content tests (Chesebro, 2003; Comadena et al., 2007; Smith & Land, 1980), free-recall measures (Spicer & Bassett, 1976; Titsworth, 2001b), detail and organization tests (Titsworth & Kiewra, 2004), and note details and organization (Titsworth, 2004) also fall prey to many of the same shortcomings as the Learning Loss Scale, yet these instruments are responsible for much of our knowledge about the relationship between teacher clarity and cognitive learning (Titsworth & Mazer, 2010).

A third reason for clarity's stronger relationship with affective learning could be that teacher clarity first triggers in students an emotional reaction that then leads to gains in cognitive learning. Although Mottet, Frymier, and Beebe (2006) proposed emotional response theory as a holistic method of synthesizing instructional communication research linking classroom communication and learning, only recent studies have begun to expand the theory (see Horan, Martin, & Weber, 2012; Mazer, McKenna-Buchanan, Quinlan, & Titsworth, 2014; Titsworth, Quinlan, & Mazer, 2010; Titsworth, McKenna, Mazer, & Quinlan, 2013). Still, various questions remain concerning the nature of emotional responses from students—the middle, and arguably, key step in the theory. Whereas Horan et al. (2012) used Mehrabian to conceptually and operationally define emotional responses from students, Titsworth et al. (2010) defined emotional responses as students' perceptions of emotional support from the instructor, emotion work required in the class, and the overall emotional valence of the class. Although neither approach is inherently correct or incorrect, the discrepancy highlights a need for additional theoretical and empirical work defining and operationalizing what constitutes students' affective/emotional responses to instruction. Clarity could primarily trigger emotional reactions in students that then prompt them to pay attention and, in the end, learn more.

### *Methods of Meta-analysis*

The differences between the two meta-analyses were mostly methodological; they include (a) our approaches to treating individual study effects (i.e., the analysis of the effects themselves and the treatment of multiple effects from the same study), (b) the inclusion criteria and sample (e.g., Meta-Analysis 1 included motivation and satisfaction as affective variables and Meta-Analysis 2 did not; Meta-Analysis 1 included 92 effect sizes from Fendick), and (c) differences in chosen statistics that test for homogeneity (e.g., using a chi-square vs. Q statistic).

The absence of homogeneous subsets of effects across the clarity literature underscores the continued need for research on the topic. Consistent heterogeneous effects point to the presence of one or more moderating variables. Stated more simply, something in the way clarity behaviors are practiced, experienced, or studied likely influences the relationship between clarity and learning outcomes. This result is similar to what was observed in Witt, Wheeless, and Allen's (2004) meta-analysis exploring relationships between teacher immediacy and student learning. Although Meta-Analysis 2 set out to explain this heterogeneity through various moderators (i.e., study design, clarity measurement, cognitive learning measurement, sample type), only study design was a significant moderator, and this was only true for affective learning. Considering Meta-Analysis 2 was unable to find significant moderators for these outcomes, the results suggest that the unexplained heterogeneity in the data is due to either unidentified moderators or issues of equivalence (Levine, 2012). That said, we are not concerned about issues of equivalence because every study included in these meta-analyses examined only instructor clarity and student learning. Meta-Analysis 2 did not synthesize dissimilar outcomes together in the same analysis (e.g., grouping student learning outcomes with other educational outcomes such as participation) to yield an uninterpretable summary effect (see Levine, 2012) which Borenstein et al. (2009) refer to as the "mixing apples and oranges" problem (p. 379).

If equivalence can be ruled out, it could be the case that unmeasured moderators influence the differences in the effectiveness of clarity on student learning. For example, the variations in different performance-based tests as measurements of cognitive learning could be causing some of the heterogeneity in cognitive learning (e.g., length of test, difficulty of test questions, format for responses, difficulty of subject matter). To uncover potential moderating variables, future studies should attempt to discern differential effects of clarity across various circumstances (e.g., class type, class size, academic field, etc.). Whereas the current analyses suggest that there is a real effect of clarity across correlational and experimental study designs, readers should note remarkable inconsistency across these studies given the stated importance of this teacher behavior. Inconsistency at the operational level could potentially diminish validity for specific findings and account for heterogeneity of effects observed in these analyses.

The two analyses reported here offer different methods of conducting the commonly utilized meta-analytic procedure. Although each is unique in its own right, the studies offer readers two statistical avenues to take when pursuing a meta-analytical project. Indeed, future research is needed to further investigate the differences in these approaches; however, the studies offer similar findings and reinforce the value of meta-analysis as a method of synthesizing global effects in a body of research.

### *Moving Forward*

Echoing Titsworth and Mazer's (2010) recommendation, instructor clarity must be treated as a multidimensional construct, even though much of the past research has treated it as unidimensional. Yet, instructional communication researchers continue to treat instructor clarity as a unidimensional high-inference student perception. Specific attention should be given to process clarity, which can include communicating clear expectations, feedback, emails, and course content. Likewise, as previously mentioned, this focus should use low-inference measurements to specifically examine multidimensional clarity behaviors.

Future research should investigate clarity within and across disciplines. While some studies (e.g., Mottet et al., 2008; Smith & Land, 1980) have examined clarity within particular disciplines, key questions remain. For instance, does the process of clarifying differ when comparing STEM disciplines with conceptual disciplines in the humanities and social sciences? How does clarity function in novel learning situations involving extensive group work or other experiential learning activities?

Teacher clarity has the potential to backfire, although research has not empirically demonstrated this possible outcome. Titsworth and Mazer (2010) pointed out that research on instructor clarity suffers from a positive bias which "assumes that clarity is always related positively to achievement" (p. 257). From these meta-analyses, we concur that the evidence supports a positive bias, and we agree that sometimes strategic ambiguity in the classroom may be warranted (e.g., being vague about defining a concept to generate classroom discussion). Moreover, we would like to add to Titsworth and Mazer's observation of the positive bias by proposing that an overabundance of instructor clarity (e.g., repeating the same lecture point ad naseum to be clear and sending lengthy emails to students) may be frustrating or annoying for some students. That is, although instructor clarity is unquestionably beneficial for student learning, the effectiveness may suffer from ceiling effects.

All studies carry limitations, and meta-analyses are no different. First, the lack of a clear definition surrounding clarity could have led to a mis-selection of articles in the databases. Although every attempt was made to include articles explicitly identifying clarity as a variable of interest, an argument exists that the database should be widely expanded to include less obvious manifestations of clarity, particularly those found in the education field. Second, most studies included in the analysis were primarily obtained from published sources. Although the article search process was not designed to exclude unpublished sources, locating such studies is difficult because they are rarely indexed; nor are they widely (if ever) cited. The reliance on published sources in these datasets could potentially result in a positively biased estimate of the average correlation if there are several unpublished studies with much lower (and likely nonsignificant) observed effects (see Copas & Shi, 2000). Finally, it is possible that the database of literature on clarity is not ready for meaningful quantitative synthesis because there is no unified understanding of what, exactly, constitutes clarity.

The current body of clarity literature has consistently revealed the positive effects of clarity, but has not systematically attempted to reveal specific processes influencing the ways that clarity functions to help students. As such, the present studies allow the conclusion that clarity has a strong effect, but future research must delve more deeply into questions asking how clarity helps learning and what circumstances influence that relationship. Instructors are well advised to continue using clarity to foster student learning. However, much of the quality experimental work is dated, and current instructional communication research on teacher clarity has been stagnating. Instructional communication scholars should consider how to advance our current understanding of clarity through low-inference experimental investigations that explain *how* clarity is communicated as a process and at the microlevel of incorporating cues or behaviors that enhance students' understanding (e.g., Titsworth, 2004). Otherwise, the mounting evidence and results of these meta-analyses tell instructors little about how to be effective in their classes beyond reminding them to "be clear."

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