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A Methodological Synthesis of Judgment Tasks in Second Language Research

Journal:	<i>Second Language Research</i>
Manuscript ID	SLR-18-0029.R1
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Keywords:	methodological synthesis, judgment task, instrumentation, meta-analysis, L2 knowledge
Abstract:	<p>Judgment tasks (JTs, often called acceptability or grammaticality judgment tasks) are found extensively throughout the history of second language (L2) research (Chaudron, 1983). Data from such instruments have been used to investigate a range of hypotheses and phenomena, from generativist theories to instructional effectiveness. Though popular and convenient, JTs have engendered considerable controversy, with concerns often centered on their construct validity in terms of the type of representations they elicit, such as implicit or explicit knowledge (Ellis, 2005; Vafaei et al., 2016). A number of studies have also examined the impact of JT conditions such as timed vs. untimed, oral vs. written (e.g., Murphy, 1997; Spada et al., 2015). This paper presents a synthesis of the use of JTs and a meta-analysis of the effects of task conditions on learner performance. Following a comprehensive search, 385 JTs were found in 302 individual studies. Each report was coded for features related to study design as well as methodological, procedural, and psychometric properties of the JTs. These data were synthesized in order to understand how this type of instrument has been implemented and reported. In addition to observing a steady increase in the use of JTs over the last four decades, we also found many of the features of JTs, when reported, varied substantially across studies. In terms of the impact of JT design, whereas modality was not found to have a strong or stable effect on learner performance (median $d=.14$; $IQR=1.04$), scores on untimed JTs tended to be substantially higher than when timed ($d=1.35$; $IQR=1.74$). In examining these features and their links to findings, this paper builds on a growing body of methodological syntheses of L2 research instrumentation (e.g., Derrick, 2016; Marsden et al., in press) and makes a number of empirically grounded recommendations for future studies involving JTs.</p>

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A methodological synthesis of judgment tasks in second language research

To understand human language, we must understand the grammatical boundaries of language. Since the early days of generative linguistic research, one of the most common ways of determining those boundaries has been to use judgments of grammaticality, or to use a more appropriate term, judgments of acceptability. However, in many disciplines, including second language (L2) research both within and outside of the generative tradition, the validity, reliability, and suitability of acceptability judgments for data elicitation has been questioned. In particular, there are longstanding concerns about the type of knowledge that judgment data reflect. Additionally, particularly in L2 research, there are numerous variables and issues that need to be considered not only in relation to data elicitation and test format, but also in relation to reporting, analysis, and interpretation. In this article, we present a methodological synthesis of 385 judgment tasks (henceforth JTs) found in 302 individual studies. In doing so, we systematically examine the use of JTs and the ways JTs have been designed, administered and reported. We also quantify the influence of JT design on observed L2 knowledge in this body of research.

Background

As Sprouse (2013) noted, “[a]cceptability judgments form a substantial portion of the empirical foundation of nearly every area of linguistics...and nearly every type of linguistic theory” (p. 1). In L2 research, JTs have been used (a) across a wide variety of theoretical frameworks and substantive domains, including generative and cognitive psycholinguistic studies, L2 pragmatics, and classroom-based research, and (b) for numerous and wide-ranging purposes, including to show development over time, to assess proficiency, to screen participants, and to determine knowledge types (see Spinner and Gass, in press, for greater detail and

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2
3 elaboration on the history of acceptability judgment tasks in linguistics and L2 research). To
4
5 some extent, the widespread use of JTs can be attributed to the influence of linguistic research
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7 methods on L2 research, but another important factor is their practicality. For example, it has
8
9 been widely thought that JTs are relatively easy to develop and administer; their scoring is
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11 relatively straightforward; they can target language features that are rare or difficult to elicit in
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13 production (Loewen, 2009); and because of apparent commonalities in target structures, formats,
14
15 and the overarching ambition to elicit judgments about language, they allow for cross-study
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17 comparisons.
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22 Despite the widespread use of JTs, there have been and continue to be concerns about
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24 important methodological issues that affect our ability to interpret what judgment data mean (see
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26 Chaudron, 1983, for an early review of the use of JTs in L1 and L2 acquisition). JTs have even
27
28 been challenged as a valid research tool for understanding acquisition. Perhaps the earliest and
29
30 most direct challenge for L2 researchers came from Selinker (1972), who said that researchers
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32 should “focus our analytical attention upon *the only observable data to which we can relate*
33
34 *theoretical predictions*: the utterances which are produced when the learner attempts to say
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36 sentences of a TL” (target language) (pp. 213-214, emphasis in original). This notion was
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38 furthered by Selinker (1974), who noted that “the only observable data from meaningful
39
40 performance situations we can establish as relevant...are...IL utterances produced by the
41
42 learner” (p. 35). In other words, in his view, only oral production data are valid data—which
43
44 rules out the usefulness of judgment data for L2 research. However, Schachter, Tyson, and
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46 Diffley (1976), among others, challenged Selinker’s belief that judgment data are inappropriate
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48 or unnecessary. In particular, a focus on production data alone makes it impossible to determine
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50 what learners do not know or cannot do and to measure their sensitivity to norms in the input.
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3 (See Gass and Polio, 2014, for a more detailed discussion of this issue). Despite these concerns
4 and debates about judgment data in the early years of the field, judgment data have continued to
5 be questioned throughout the years, with justification often being given for using them. We turn
6 to a discussion of more recent debates about the nature of judgment data.
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14 *The nature of knowledge elicited by JTs*

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17 In their most common form, judgments are elicited on isolated sentences using either a
18 dichotomous response or a Likert-style response. One of the main concerns about JTs is such a
19 task is ‘unnatural’. That is, they do not reflect real-world use of language. Similarly, there is
20 concern that acceptability judgments may not tap into learners’ implicit knowledge of language,
21 but rather only access explicit or even only metalinguistic knowledge. For some researchers,
22 measuring explicit or metalinguistic knowledge is exactly the goal, but often researchers are
23 attempting to discern the nature of implicit knowledge underlying an interlanguage grammar.
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Implicit knowledge is often considered a type of knowledge that is most useful to understanding the nature of L2 knowledge in that it is more stable, less prone to decay (e.g., after instruction), more immediately accessible, and less context-dependent (Rebuschat and Williams, 2012). There is some debate about whether acceptability judgments can tap into more implicit knowledge types, and if so, how they should be designed and analyzed to best accomplish this task (see e.g., Ellis, 2005).

As part of this debate, various task features and conditions have been tested as a means to validate measures of different types of L2 knowledge elicited by JTs. For instance, using timed or speeded judgments may lead learners to employ (more) implicit knowledge in part because there is not enough time to think back to learned rules, typical of those presented in a classroom

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3 context (Ellis, 2005). Godfroid et al. (2015) used eye-tracking to monitor learners' eye
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5 movements during timed and untimed judgement tasks and found that learners regressed more
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7 during the untimed tasks; they concluded that the untimed tasks may lead to more use of explicit
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9 knowledge than the timed tasks. However, the matter is far from settled. Kim and Nam (2016)
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11 compared timed judgment data and oral elicited imitation data and found that the latter to be a
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13 better indicator of implicit knowledge. The authors also found that JTs measured implicit
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15 knowledge more directly when the stimuli were presented aurally as opposed to in the written
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17 modality.
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21 Gutierrez (2013), investigating the construct validity of JTs, administered a timed and an
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23 untimed JT and a metalinguistic knowledge test to 49 L2 learners of Spanish. The timed JT was
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25 intended to measure implicit knowledge and the other two were intended to measure explicit
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27 knowledge. Using exploratory and confirmatory factor analyses, grammatical sentences (timed
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29 and untimed JTs) loaded on a construct interpreted as implicit knowledge and the ungrammatical
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31 sentences loaded on a constructed interpreted as explicit knowledge. Based on their results, the
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33 author argued that time pressure does not distinguish between knowledge types; what does
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35 distinguish between knowledge types is whether or not a sentence is grammatical or
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37 ungrammatical.
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41 Vafae, Suzuki, and Kachisnke (2017) argued against Gutierrez's (2013) heavy reliance
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43 on JTs as the means to measure explicit and implicit knowledge. They conducted a confirmatory
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45 factor analysis based on data elicited from 79 Chinese L1 speakers learning English as an L2.
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47 They used timed and untimed grammaticality JTs, a self-paced reading task, a word-monitoring
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49 task, and a metalinguistic task to test the extent to which a JT reflected implicit knowledge. They
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51 concluded that grammaticality judgments, regardless of being timed or untimed, were not a
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3 suitable elicitation technique for measuring implicit knowledge.
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5 Other design and analysis issues may also influence learners' use of implicit or explicit
6 knowledge or may, more generally, influence participant scores on JTs. For instance, having
7 learners indicate errors or provide corrections to them while they make judgments may lead to
8 increased use of explicit and/or metalinguistic knowledge (e.g., Ellis, 1991). Additionally, task
9 modality (e.g., written versus aural), response types (e.g., binary vs. scaled), the balance of
10 grammatical/acceptable to ungrammatical/unacceptable items, and confidence ratings, all of
11 which feature to varying degrees in JTs, may also impact learner performance.
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21 Framing their study within the context of the frequent use of JTs, Shiu, Yalçın, and Spada
22 (2018) pointed out that “it is essential to continue to explore how design features contribute to
23 learners' GJT performance (p. 216). They considered four key design features (time constraints,
24 task stimulus, task modality, and target features) and investigated the potential outcomes of
25 results when these features were manipulated. They found that time constraints, modality, and
26 task stimulus impacted results, thereby further illustrating the complexities involved in using and
27 interpreting judgment data.
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37 In another study focusing on design features, Murphy (1997) investigated declarative
38 sentences with embedded questions and *wh*-questions that violated Subjacency, asking whether
39 there might be differences in outcomes if sentences were presented visually or aurally.
40 Participants were not only slower when sentences were presented aurally, but even more
41 important they were less accurate. She emphasized the importance of methodology when
42 interpreting research results.
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51 The studies reviewed here underscore the overall importance of a thorough understanding
52 of the potential impact of different design features.
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Methodological transparency and rigor

In recent years, methodological issues have been a focus of attention as the field of L2 research matures. Emphasizing this point, Byrnes (2013: 825) points out that “methodological issues inherently merit a certain level of attention inasmuch as they assure the quality of our work. But it appears that at this point in the development of applied linguistics, they demand a kind of professional scrutiny that goes directly to the core of what we do and what we know and what we can tell our publics that we know—and not only how we do it.”

As the result of greater attention as well as, perhaps, improved training and stricter journal standards (e.g., Norris, Plonsky, Ross, & Schoonen, 2015), methodological rigor in the field appears to be improving. However, there is still a long way to go: Using techniques characteristic of methodological synthesis, Derrick (2016) reviewed the reporting practices surrounding 925 data collection instruments found in 385 articles spanning a wide variety of domains in L2 research. Her results indicate a general lack of transparency and rigor in data collection, reporting, and methodological transparency (as discussed by Marsden and Plonsky, 2018). For example, only 17% of the sample was found to have reported piloting their data collection instruments, and reliability coefficients were available for only 28% of the sample (see similar results found by Plonsky, 2013).

Of course, we cannot equate unreported reliability with low reliability (i.e., high measurement error). Plonsky and Derrick (2016) set out to measure the error in L2 research instruments as reported in primary studies. Their approach, known as ‘reliability generalization meta-analysis’ (Rodriguez and Meada, 2006; Wheeler, Vassar, Worley, and Barnes, 2011), involves systematically reviewing and aggregating reliability coefficients to estimate the overall reliability in a given sample of studies. The median reliability coefficient (for internal

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3 consistency) was .82, with somewhat higher interrater and intrarater estimates. This estimate was
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5 also found to vary as a function of several additional features of L2 designs and instruments.
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8 Marsden, Thompson, and Plonsky (2018b) represents another recent methodological
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10 synthesis with parallels to the present study. Their review, like ours, focused on the use of one
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12 particular elicitation procedure, self-paced reading, which has likewise been the subject of a
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14 number of debates concerning construct validity as well as what might constitute appropriate
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16 implementation and design (e.g., Keating and Jegerski, 2015; Roberts, 2016). Marsden et al.'s
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18 results indicated that the features of self-paced reading tasks vary widely. For example, there was
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20 little consistency in terms of target regions being analyzed, even among reports examining the
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22 same linguistic features. Substantial variability was also observed in a number of other features
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24 such as the inclusion of comprehension questions, the number of target vs. distractor or filler
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26 items, and sentence length, all potentially affecting learner outcomes as measured in
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28 milliseconds. Reporting of data related to self-paced reading was also inconsistent. For example,
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30 only two of the 64 studies in their sample provided an estimate of instrument reliability.
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35 Beyond the reviews described here, the current study follows on the heels of a growing
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37 body of methodological investigation and meta-science. Some syntheses in this area deal with
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39 issues particular to a given substantive domain (e.g., L2 written feedback, Liu and Brown, 2015;
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41 interactionist SLA, Plonsky and Gass, 2011; learner corpus research, Paquot and Plonsky, 2017;
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43 and interaction and computer-mediated communication, Ziegler, 2016). Others, however, address
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45 one or more practices across domains, such as: mixed methods (Hashemi and Babaii, 2013);
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47 reporting practices (Larson-Hall and Plonsky, 2015); replication research (Marsden, Morgan-
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49 Short, Thompson, and Abugaber, 2018a); quantitative analyses and reporting practices, (Plonsky,
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51 2013, 2014); study designs (Marsden and Torgerson, 2012; Plonsky and Gonulal, 2015);
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multiple regression (Plonsky & Ghanbar, in press; Plonsky and Oswald, 2017); and structural equation modeling (Winke, 2014). Motivating this line of research is, in part, an acute awareness that “respect for the field [...] can come only through sound scientific progress” (Gass, Fleck, Leder, and Svetics, 1998: 407). However, an even more fundamental rationale for describing and evaluating the empirical efforts in L2 research is the need to ensure the validity of the theoretical and practical claims we make based on our research. In view of the importance of this for the progress and usefulness of science both generally and in the case of efforts in the field of L2 research, we support the notion put forth by Plonsky (2013) that methodological practices and study quality need to be measured, not assumed. Following this line of thinking, in the present study, we systematically review the methodological issues and choices involved when eliciting judgment data in L2 research and we investigate how some of these choices can affect substantive findings.

Research questions

1. To what extent and in what ways have JTs been used in L2 research?
2. How have JTs been designed and administered in L2 research?
3. To what extent do design features of JT (modality and timing) influence the scores they elicit?

Method

Study Identification

The first step in conducting a research synthesis or meta-analysis is to identify inclusion/exclusion criteria based on the substantive or methodological domain of interest (Plonsky and Oswald, 2015). As our primary criterion for inclusion in the present review, a study had to include one (or more) JTs that targeted participants’ morphosyntactic knowledge in their L2 (L3, L4, etc.). Studies targeting either L1 morphosyntactic knowledge or L2 pragmatic or

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3 discourse knowledge were excluded in order to arrive at a more homogenous sample. In addition,
4 specific criteria were established regarding the type of JTs eligible for inclusion, chosen in order
5 to arrive at a relatively homogeneous and representative—if not exhaustive—sample of the use
6 of JTs in L2 research:
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- 11 • Outcome measures had to be accuracy (i.e., based on a judgment) rather than a measure
12 of processing (e.g., ERP, reaction times).
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- 14 • Judgment was made on an individual sentence (rather than on a longer stretch of
15 discourse).
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21 Studies with items requiring a comparison of grammaticality/acceptability across multiple
22 sentences (e.g., which sentence is most acceptable?) were excluded. Also excluded were
23 magnitude estimation tasks.
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28 With the eligibility criteria established, we began an extensive keyword database search.
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30 Following Plonsky and Oswald (2015) we included Linguistics and Language Behavior
31 Abstracts (LLBA), Education Resources Information Center (ERIC), PsycInfo, and Google
32 Scholar. In addition, we searched the L2 Research Corpus (Plonsky, n.d.) as well as the IRIS
33 Digital Repository (<https://www.iris-database.org>; see Marsden, Mackey, and Plonsky, 2016).
34
35 Although it was likely that there would be significant overlap between the six databases,
36 comprehensiveness was prioritized over redundancy (following Lee et al., 2015; Plonsky and
37 Oswald, 2015). The key words were ‘grammaticality’, ‘acceptability’, and ‘truth value’, which
38 were required to occur alongside both ‘judgment’ (or ‘judgement’) and either ‘task’ or ‘test’
39 (e.g., ‘acceptability judgment test’, ‘grammaticality judgment test’). A number of additional
40 criteria were then also applied to reduce the number of identified studies into a more
41 homogeneous sample. First, all included studies were required to feature primary research
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3 published in a journal on the Social Science Citation Index (SSCI) ([http://ip-](http://ip-science.thomsonreuters.com/mjl/)
4 [science.thomsonreuters.com/mjl/](http://ip-science.thomsonreuters.com/mjl/)), with all non-indexed journal articles, book chapters,
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6 dissertations, theses, and conference proceedings excluded. Any duplicate reports or datasets
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8 (i.e., data that had been analyzed exactly as they were analyzed in previous publications) were
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10 excluded. In total, 302 studies were identified comprising a total of 385 JTs (see materials
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12 associated with this study on IRIS; iris-database.org). (Just under a third of the full JTs and
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14 records for all the articles in our sample are available under ‘Special Collections’ at [www.iris-](http://www.iris-database.org)
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16 [database.org](http://www.iris-database.org), discussed below).

21 *Coding*

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24 A coding scheme was designed to collect data on each study and on the JTs in our
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26 sample. This tool was similar in design to those of other methodological syntheses (e.g., Plonsky,
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28 2013; Marsden et al., 2018b). The final version included 69 items eliciting substantive and
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30 methodological features across five themes: (a) *Bibliographic Information* (e.g., authors, journal,
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32 year); (b) *Study Design* (e.g., *N*, target L2, one-shot vs. developmental); (c) *JT Design* (timed vs.
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34 untimed; modality; number of items; balance between grammatical/acceptable vs.
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36 ungrammatical/unacceptable items); (d) *Reliability, Transparency, and Validity* (e.g., reporting
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38 of instrument reliability; instrument availability; and (e) *Theoretical Framework* the study was
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40 conducted under (e.g., formal, functional, usage-based). Appendix A lists all coded features
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42 along with descriptors used. In addition, the full instrument in its original format (an Excel
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44 spreadsheet), including the entire dataset for the present study will be available on the IRIS
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46 database upon publication.

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51 Following several rounds of revising and piloting the instrument, one of the study’s co-
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53 authors coded the entire sample. This co-author then provided extensive training to a research
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3 assistant (a doctoral student in applied linguistics) who re-coded 15% of the JTs in the sample
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5 ($K_{\text{study}} = 43$, $K_{\text{task}} = 62$). Though a low percentage of the overall sample, the total number of
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7 recoded studies is well above the minimum requirement by Lipsey and Wilson (2001). Although
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9 agreement between the first and second coder was generally high (median across all items =
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11 87%), with an overall Cohen's Kappa (κ) of .72, the range of reliability coefficients (Kappa)
12
13 across categories was wide ($\kappa = .11$ – 1.00). The two coders discussed and resolved disagreements
14
15 whenever present. Based on discussions amongst the researchers, problematic studies were
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17 recoded. The source of low Kappa-values was almost always attributed to a difference between
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19 items coded as “No” (i.e., “not present”) and “Not Reported”. For example, when asked if the
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21 judgment task in question had two counterbalanced forms, one coder often recorded “No” (no
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23 counterbalancing), while the other would say that this information was “Not Reported”. While
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25 the range of Kappa values is not ideal, the overall Kappa of .72 is within range of many other
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27 meta-analyses and methodological syntheses (e.g., Marsden et al., 2018b; Paquot and Plonsky,
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29 2017). In addition to the coded features, Appendix A reports both Kappa and percent agreement
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31 for each item in the coding scheme.
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37 *Analysis*

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39 RQ1 was concerned with the frequency, publication outlet, context, and design of studies
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41 employing JTs in L2 research. To address this question, we calculated the frequencies and
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43 percentages of JTs found overall, over time, and across different journals. We also calculated the
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45 frequency and percentage of major types of designs as well as demographic and contextual
46
47 features found in our sample.
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51 RQ2 focused on the design and implementation of the JTs themselves. Similar to RQ1,
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53 frequencies and percentages of JT features were calculated across the study sample. One of the
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3 features of interest was reliability (internal measurement consistency). We recorded not only
4 whether estimates of reliability were available but also the estimates as reported. Using reliability
5 generalization meta-analysis, reliability estimates associated with the JTs in our sample were
6 aggregated, and measures of central tendency and dispersion were examined.
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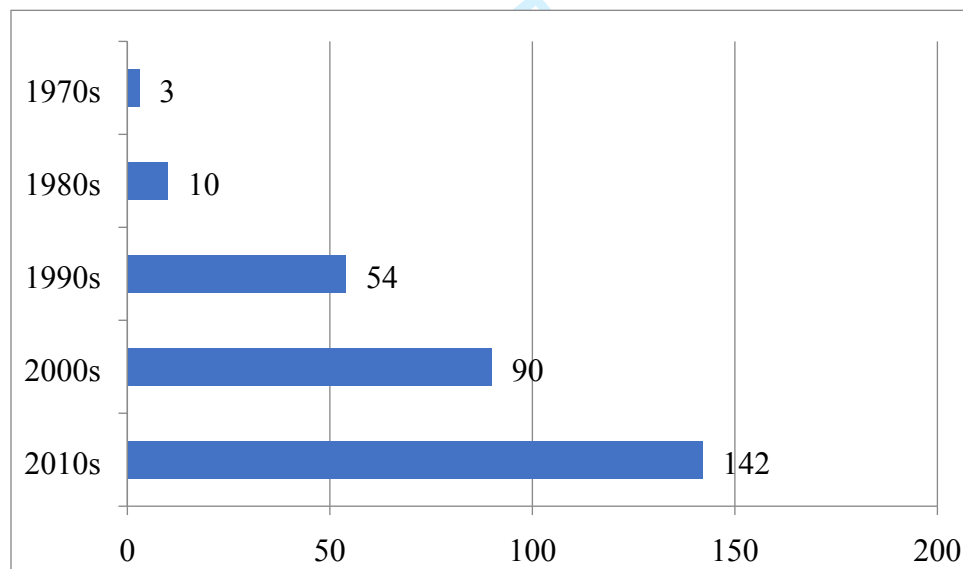
12 Our third and final research question addressed the extent to which different features of
13 JTs might impact the data they produce. To answer this question, we first identified the studies
14 that administered the same JT but with different design features to the same sample of
15 participants. Specifically, we selected those studies that investigated either or both of the
16 following two parameters within the same study: modality (aural vs. written) and/or timing
17 (timed vs. untimed). These variables were chosen for this phase of the analysis on the grounds
18 that they are (a) commonly discussed in the literature on JTs, (b) varied substantially across the
19 sample, and (c) were available in sufficient numbers for within-group comparisons. When
20 sufficient data were available (i.e., means, standard deviation, and sample size), we then
21 calculated a Cohen's d value for these contrasts (e.g., written vs. aural), representing the
22 standardized mean difference between the JT scores in each pair of conditions within each
23 parameter. The d values were then combined and compared using standard meta-analytic
24 procedures (Plonsky and Oswald, 2015).
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42 Results

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45 *RQ1. To what extent and in what ways have JTs been used in L2 research?*
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47 Our first research question was concerned with when, how, and where JTs are used in L2
48 research. As a first step in understanding this particular tool, we examined the frequency of its
49 use over time and across different journals. As shown in Figure 1 and, in greater granularity in
50 the following Sparkline (see Larson-Hall, 2017; ) , the field's use of JTs has increased
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3 substantially and regularly across the last 4 decades, with between approximately 40-50 more JT
4 studies published each decade since 1980. This regular increase is in part a reflection of the total
5 increase in volume of publications in the field. JTs are also found across a number of different
6 journals. However, Figure 2 shows that there is much stronger presence of JTs in some journals
7 (e.g., *Second Language Research*, *Studies in Second Language Acquisition*) than others (e.g.,
8 *TESOL Quarterly*), though of course this is partially influenced by the longevity of each
9 particular journal.
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Figure 1. Frequency of JTs Observed Over Time

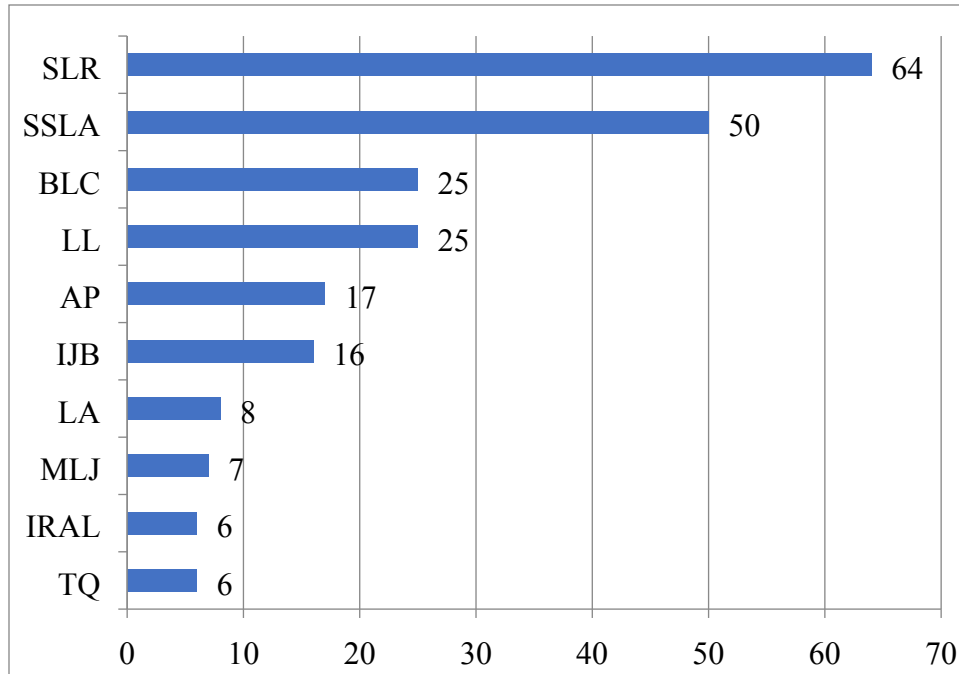


Figure 2. Raw frequency (>5) of JTs Found in L2 Research Journals

Note. SLR = *Second Language Research*; SSLA = *Studies in Second Language Acquisition*; BLC = *Bilingualism: Language and Cognition*; LL = *Language Learning*; AP = *Applied Psycholinguistics*; IJB = *International Journal of Bilingualism*; LA = *Language Awareness*; MLJ = *Modern Language Journal*; IRAL = *International Review of Applied Linguistics*; TQ = *TESOL Quarterly*

Moving toward the types of research where JTs are found, approximately two-thirds (77%) of the studies in our sample employed a one-shot design. However, this does not mean that JTs have not been used in research investigating L2 development. It was not uncommon to find cross-sectional or ‘pseudo-longitudinal’ designs, with 24% of the sample comparing performance across different proficiency levels. Additionally, 60% of the sample compared learner responses to a native speaker group. Also related to design, we note that the native speaker samples (median $n = 20$) in this body of research were often much smaller than those of L2 learners (median $n = 47$), a choice likely associated with greater consistency generally

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3 observed among native speakers and/or the greater difficulty of accessing native speaker
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5 participants for some languages.
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8 Study designs also varied in terms of the languages and learners involved. As we might
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10 expect, a large portion of the sample involved English as an L1 (45%) or target language (59%).
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12 Other L1s found with some regularity (i.e., $\geq 5\%$) included Chinese (15%), Japanese (9%),
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14 French (8%), Arabic (6%), Spanish (6%), and Korean (5%). Target languages other than English
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16 ($\geq 5\%$) included Spanish (17%), French (11%), and Chinese (5%). There was a roughly equal
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18 split between studies examining L2 knowledge among foreign (51%) as opposed to second
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20 (41%) language learners. The remainder of the sample involved learners of artificial languages
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22 (e.g., Ehrich and Meuter, 2009) or of real languages being learned by participants only for the
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24 purpose of the study (e.g., Andringa and Curcic, 2015). Heritage language learners/users were
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26 present in 7% of the sample. Primary studies were also coded for the proficiency of the learners
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28 involved. Recognizing that authors did not often provide justification (such as qualifications or
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30 amount of instruction) for claims regarding proficiency (17%), we found that 24% of the samples
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32 were beginners, 33% intermediate learners, and 47% advanced learners. (Note: Total percentage
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34 here adds to greater than 100% because some studies included groups at multiple proficiency
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36 levels. We also note that proficiency levels were coded based on authors' labels of low,
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38 intermediate, and advanced proficiency, when provided, and we recognize the potential for
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40 inconsistencies across authors). 80% of the learners involved in this body of research were
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42 adults; 4% were children and 3% were teens. The ages of participants in the remainder of the
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44 studies were either mixed or not reported.
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54 *RQ2. How have JTs been designed and administered in L2 research?*
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3 One of the central interests of the current study was to understand the different features of
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5 JTs found in published L2 research. Toward this end, we coded a number of features, the results
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7 of which we summarize here and which, overall, can be seen to vary a great deal across the
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9 sample.
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12 One of the fundamental characteristics of any language test is the number of items it
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14 includes. Although the median number of total items per instrument was 50 (median target items
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16 = 32, IQR = 40), the minimum and maximum values range from 4 to 300, presenting in many
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18 cases potential for participant fatigue. To offset this potential threat to internal validity, a small
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20 number of studies (6%) reported offering participants a break. We note here that it is difficult to
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22 determine precise item totals because 26% of the studies we surveyed were unclear on whether
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24 distractor items were included and, if so, how many. One way to understand such variability in
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26 the length of JTs is to consider the number of target features being tested. Although a clear
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28 majority (66%) were focused on a single structure, individual JTs in our sample assessed learner
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30 knowledge on up to 17 different structures. Across the entire sample, 33 unique target features
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32 were identified with a fairly large median of 21 items per feature (interquartile range = 24).
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34 Figure 3 displays the frequency of all those occurring in five or more studies. The most frequent
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36 structure, word order, is also the broadest category. The remaining features represent a range of
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38 morphosyntactic targets.
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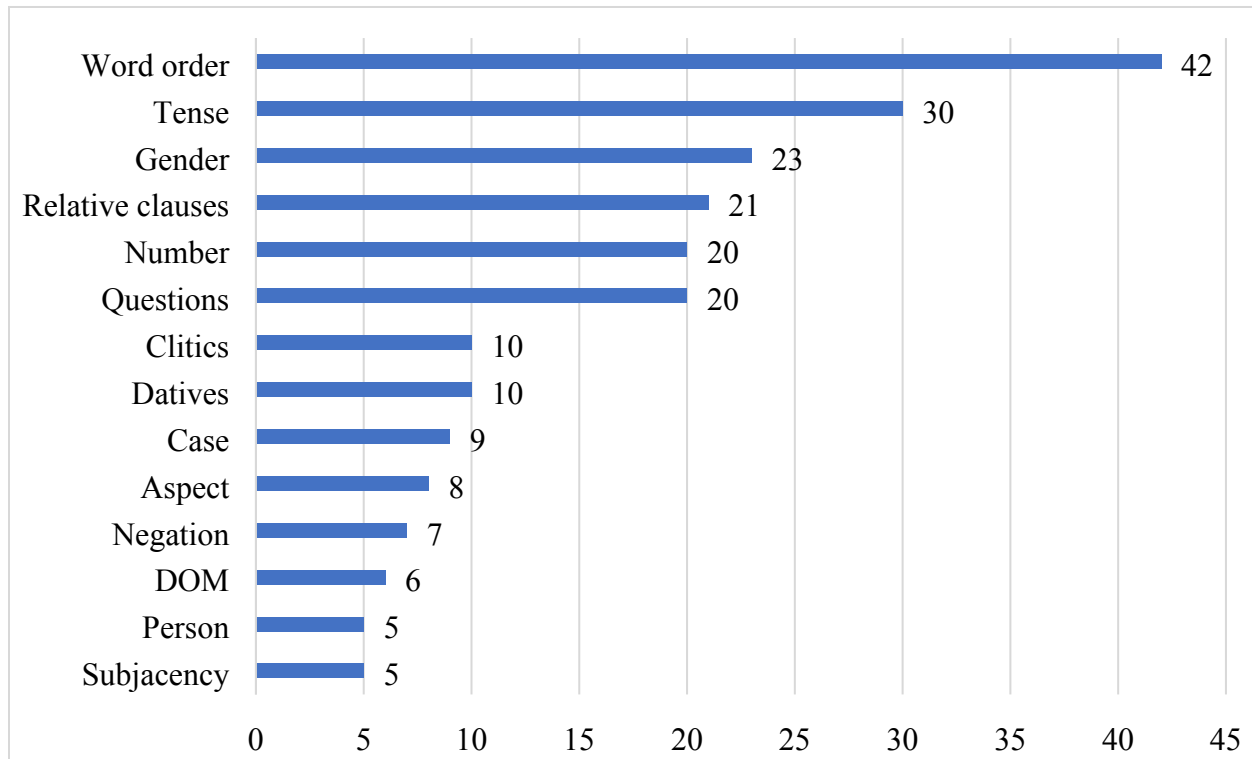


Figure 3. Raw frequency of target features examined in JTs

Note. DOM = differential object marking

Beyond the number and focus of the features they assess, JTs vary along a number of additional features including test modality and format. Although it was not uncommon for JTs to be administered in the aural modality (16%), most JTs (58%) were administered as a written task, and 6% were provided in both modalities. Modality was not reported in an additional 18% of the sample. This feature is particularly critical given that one might expect learner performance to vary as a function of the modality in which the JT is administered, a point we return to in response to RQ3. A related matter is the format of the test. Approximately one-third of the JTs in our sample were administered in a paper-and-pencil (29%) or computer-based (36%) format; the format for the remaining studies was unreported.

In addition to testing format, another condition found to vary in JT administration is

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3 timing. Whereas 55% of the sample did not report whether time restraints were involved, explicit
4 reference to a lack of a time limit was reported for 28% of the sample. The remaining 17% of the
5 studies imposed a time limit on participants to complete a judgement. However, justification for
6 the length of time given to participants to respond to an item was not uniformly provided: 33%
7 and 2% calculated the time limit based on native and non-native speaker norms (or piloting),
8 respectively. Loewen and Erlam (2006), for example, calculated the median time required by
9 native speakers to respond to each item in their study. They then added to this an additional 20%
10 “to allow for the slower processing speed of the L2 learners” (p. 8).
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22 One broad category of JT features we explored is concerned with test and item
23 construction. It is often suggested that items be randomized to avoid drawing participants’
24 attention to particular structures being targeted (e.g., Chaudron, 1983). However, only 16% of
25 the sample reported to have done so. 21% did not randomize items, and 63% did not report
26 whether items were randomized or not. Compared to randomization, a slightly higher proportion
27 (26%) of studies was found to balance the number of ungrammatical/unacceptable versus
28 grammatical/acceptable items; 49% of the JTs did not do so, and 26% did not report whether or
29 not the JT was comprised of an equal number of grammatical/acceptable and
30 ungrammatical/unacceptable items. In some studies, contextual information was provided to
31 accompany JT items. This can, for example, help to coerce particular features by ensuring
32 specific meanings or functions are expressed, or it can increase the ‘naturalness’ of the test by
33 embedding the sentences in a meaningful context. In our study sample, 9% included a story, 8%
34 paired items with an image or video, and the items in 2% of the JTs were embedded within a
35 larger body of prose. In the remaining 81% of the JTs, no additional or contextual information
36 was provided.
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We also examined a number of features related to different types of items and item responses. As shown in Figure 4, approximately half of the JTs in our sample asked participants to simply indicate whether each item was grammatical/acceptable or ungrammatical/unacceptable. Some instruments (10%) also allowed participants to indicate if they did not know whether an item was grammatical/acceptable or not. Another frequent response elicited by JTs (29%) involves asking participants to respond on a scale such as -3 (completely unacceptable) to +3 (completely acceptable) (e.g., Yuan and Dugarova, 2012). In addition, 23% of the JTs in our sample also instructed participants to identify the location of errors when present, the majority of which (80%) also asked participants to attempt to correct the errors they identified. Researchers utilized these data only sporadically and somewhat idiosyncratically. For example, of the 87 instances in which corrections were made by participants to incorrect items, some researchers analyzed incorrectly identified errors (21%), some gave partial credit (9%), and others ignored them (2%). Most often, however, researchers simply did not report how such responses were handled (43%).

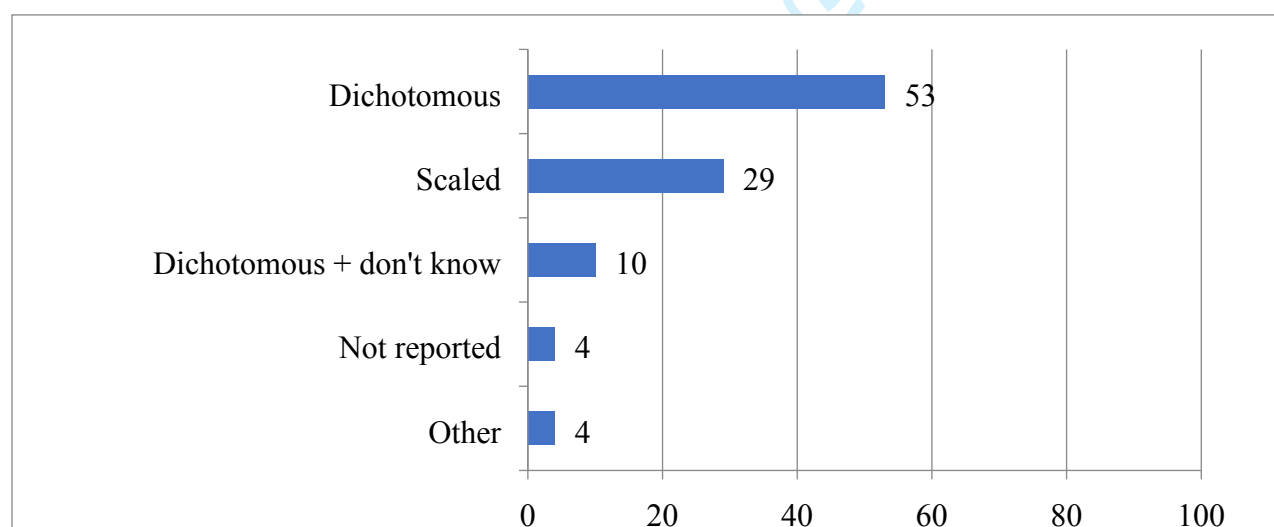


Figure 4. Percentage of different item response types

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6 A small number of studies elicited additional responses following the main task of
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8 determining item acceptability or grammaticality. These include a rating of the confidence of
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10 their responses (5%), the basis for their response (4%, e.g., intuition/feel vs. rule), and providing
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12 a response to a meaning-based comprehension question (1%).
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15 A number of the results thus far have examined the extent to which authors reported the
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17 details of their instrumentation. Also relevant to describing instrumentation is the reporting of
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19 reliability (i.e., internal consistency), which was found in only 16% of our sample. In most of
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21 these cases (81%), reliability was reported using Cronbach's alpha. In addition to coding studies
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23 for whether a reliability estimate was provided, we recorded each individual coefficient. The
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25 reliability estimates for internal consistency in the sample were then meta-analysed to examine
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27 their distribution. The bulk of the observed estimates fell in the .7-.9 range, with a median of .8
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29 (see Figure 5). This value matches very closely Plonsky and Derrick's (2016) finding of .83
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31 (interquartile range = .15) as the median estimate of reliability (internal consistency) for
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33 linguistic measures in L2 research.
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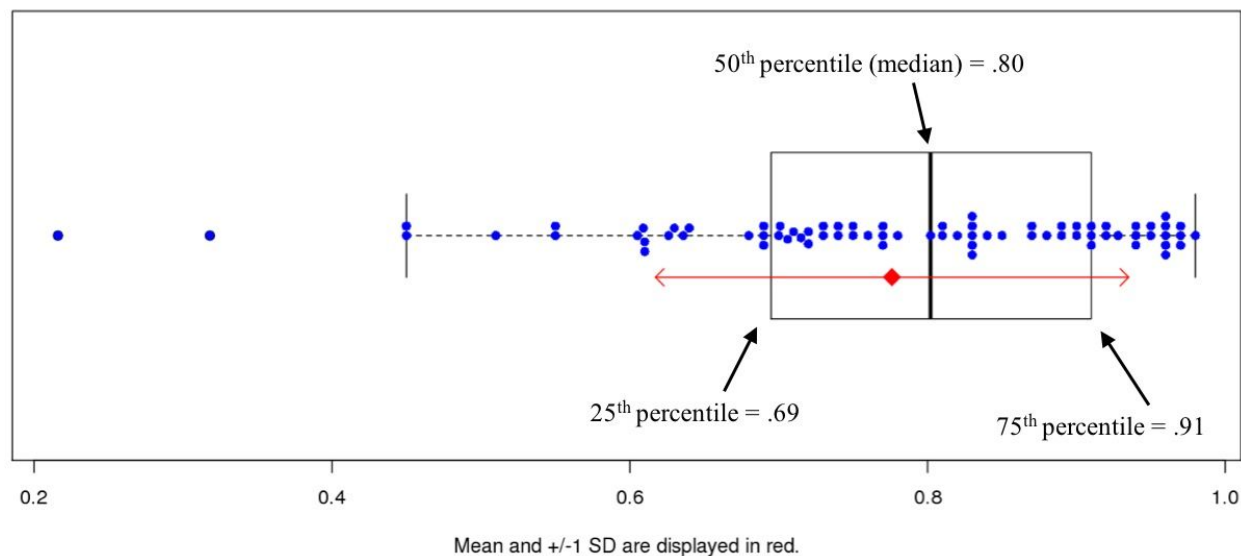


Figure 5. Reliability coefficients (internal consistency) from JTs in L2 research ($k = 75$)

56% also chose to administer additional measures of L2 learner knowledge. In most of these cases, data from two (or more) instruments were analysed separately from the JT data. But 29% took a statistical approach to compare, correlate, or otherwise examine the relationships among JTs and other measures of learner knowledge. For example, Spada, Shiu, and Tomita (2015) ran bivariate correlations between acceptability JTs and a number of additional instruments designed to measure L2 learners' implicit knowledge. Related to the approach in Spada et al., we were interested in examining researchers' assumptions regarding the type(s) of knowledge elicited by JTs. However, less than a quarter (24%) of the sample discussed this issue overtly: 10% used JTs to measure knowledge labeled as implicit, procedural, or automatized; nearly the same amount (9%) were interested in declarative or explicit knowledge; 4% of the studies used JTs to measure knowledge in both categories. Ellis (2005), for example, sought to measure learners' implicit and explicit knowledge using timed and untimed JT, respectively. Discussion of implicit/explicit knowledge types were not evenly

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3 distributed through our sample, with most occurring between
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5 2010–16, as follows: 4 out of 12 studies from 1970-1989; 6 out of 54 from 1990-1999; 10 out
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7 of 90 from 2000-2009; and 23 out of 142 from 2010-2016. The increase in the last decade might
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9 arguably be attributable to the field's increased concern about explicit/implicit knowledge types
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11 (Rebuschat and Williams 2012), but in fact the increase may be, more simply, due to the overall
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13 increase in L2 studies using JTs, as the proportions have not increased.
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17 As a final check on the methodological transparency of studies involving JTs, we also
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19 coded for whether—and where—JTs were made available, thus facilitating inspection, analysis,
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21 researcher training, and replication. Overall, 36% of the JTs were accessible, whether in primary
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23 reports (22%), on IRIS (8%), and/or elsewhere such as an author's website (3%). Of course, the
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25 flipside of this result is that 64% of the JTs in our sample were not available. Unfortunately,
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27 there is no evidence of real improvement here over time. The percentage of JTs from our sample
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29 that were available in some form are as follows: 1970s = 33%, 1980s = 57%, 1990s = 46%,
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31 2000s = 30%, 2010s = 35%.
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35 *RQ3: To what extent do features of JTs (modality and timing) influence the scores they elicit?*
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38 The results thus far have indicated wide variability in terms of how JTs are designed and
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40 implemented in L2 research. Of equal or greater importance is understanding whether variations
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42 in the features of JTs impact learner performance. More concretely, we were interested in
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44 ascertaining whether and to what extent different types of JTs might provide different
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46 information about the amount of L2 knowledge learners demonstrate under different test
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48 conditions. Very few studies have addressed this question. However, a meta-analytic approach
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50 enables us to investigate relationships observed across studies, even if those relationships are not
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the focus of any individual study. We present the results pertaining to two such features here: aural vs. written and timed vs. untimed conditions.

As shown in the results above, JTs are administered in the written modality much more frequently than aurally. In a small number of studies ($k = 17$), both modes have been employed. In those cases, a standardized mean difference (Cohen's d) was calculated between each sample's score on the two testing conditions, with the groups' pooled SD in the denominator when calculating d . Figure 6 presents the spread of d values, where a negative value indicates a higher score for aural JT scores and a positive value indicates a higher score for the same group's written JT. The median d value (.14) indicates that, overall, scores on written JTs tend to be slightly higher than for aural JTs. In addition, observed d values ranged from -0.7 (a substantially higher score for an aural JT) to $+1.28$ (a large advantage for the written condition). The (unweighted) mean was $.32$ with a fairly a large SD ($.59$) and a 95% confidence interval (CI) $[-.02, .63]$ indicating little stability to the observed point estimate.

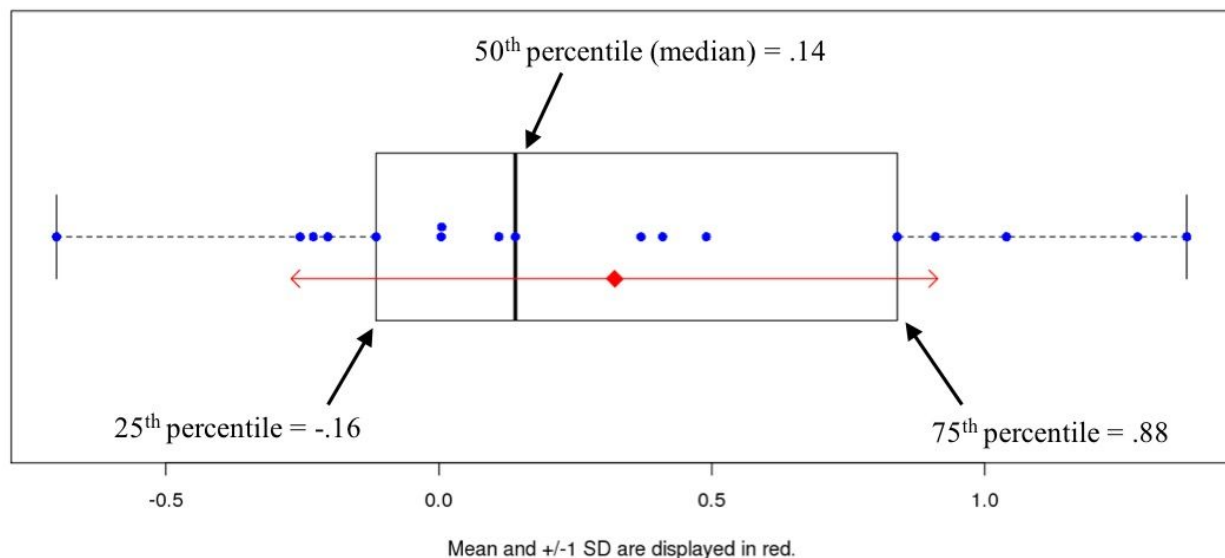


Figure 6. Within-group contrasts (d) of aural vs. written JT scores ($k = 17$; indicated in online Appendix with *)

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5 The other feature we examine here with respect to its effect on JT scores was timing.
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8 Certain studies in our sample required participants to complete a judgment within a pre-
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10 determined period of time. This condition was often imposed—or not—in an attempt to tap a
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12 certain type of L2 knowledge (e.g., implicit or explicit; see Ellis, 2005). When results for both
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14 timed and untimed conditions were presented for the same participant samples, d values were
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16 calculated to index the difference in scores for each condition. Each of those d values appears in
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18 Figure 7. As observed for scores on aural vs. written JTs, the range of d values representing the
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20 difference between timed and untimed JTs was fairly wide. Unlike the comparison between
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22 modalities, however, all results were positive and showed a substantial discrepancy, with
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24 untimed JT scores tending to be much higher than timed JT scores. The (unweighted) mean was
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26 1.6 (SD = 1.15), with a 95% CI of [1.09, 2.10] suggesting that this difference is fairly large and
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28 stable.
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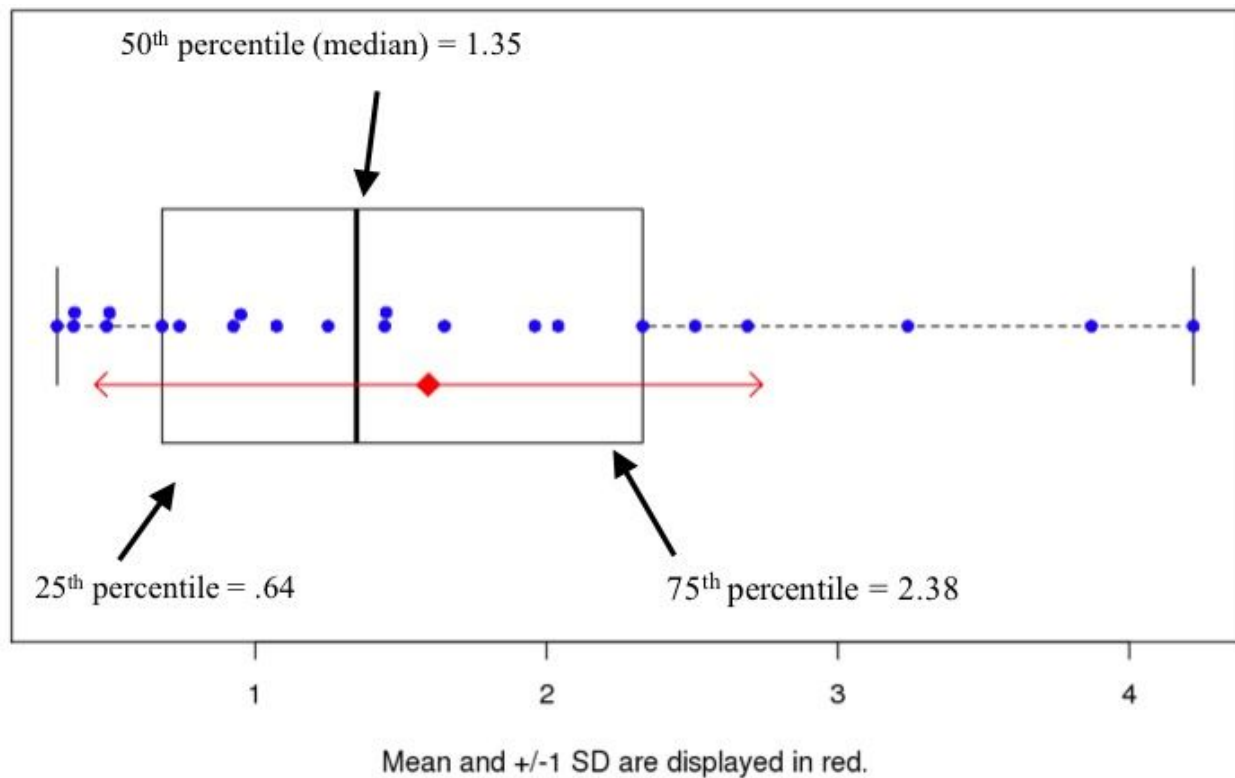


Figure 7. Within-group contrasts (d) of timed vs. untimed JT scores ($k = 22$; indicated in online Appendix with ^)

To address a question raised by a reviewer, we examined whether the effects of modality and time pressure might be conflated (e.g., whether aural JTs had been more likely to be timed). In total, only 22 JTs in the study sample were both timed and administered aurally. Of these 22 JTs, only three contributed to the comparison of possible modality effects ($d = -.7, .14, 1.28$) and only 1 contributed to the comparison of timing effects ($d = .32$). Generally, also, the ‘written’ versions in these modality comparisons were untimed. By definition, aural language happens at a fixed pace for all participants, so is, in a sense ‘timed’, and so, generally with these comparisons between aural and written, ‘modality’ is inevitably conflated with timing, unless one times them both to keep timing constant (as in Morgan-Short, Marsden, Heil et al. 2018), an issue raised

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3 again in our Discussion. Note also that in the comparisons of timed vs. untimed, all the tests were
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5 written. Based on this follow-up analysis, we are fairly confident that such a conflation does not
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7 pose a threat to the validity of the results concerning modality- and timing-effects.
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10 An additional step we took to further examine the validity of the effects found in our
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12 meta-analyses was to document the extent of the use of ‘counterbalancing’ to reduce confounds
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14 caused by administering one condition of the test before the other condition across all
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16 participants. For example, if the written modality was systematically administered after the aural
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18 modality, or the untimed modality after the timed modality, this could explain differences in the
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20 scores between the different conditions. To illustrate further, if the untimed JTs were always
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22 taken after the timed JTs and if those two tests were identical, practice effects could explain the
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24 large effects that we found above in favor of untimed JTs. Researchers can counterbalance using
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26 one or both of two approaches: one is at a within-test level (so the two conditions of the tests
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28 contain slightly different, though comparable, items, such as by keeping the grammar features
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30 constant but using different lexical items of a similar frequency); the other is at the test level
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32 (half the learners undertake one version first and the other of participants undertake the other
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34 condition first. In our subset of studies examining different timing conditions, the majority of
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36 studies (8/11 [73%]; 4 not reported) presented the timed JT before the untimed JT (many of
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38 which followed Ellis, 2005, procedurally). No studies reported any kind of counterbalance
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40 between timed and untimed presentation, and, also concerning, half used the same content for
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42 both. For the subset of studies examining different modalities, only two of six studies reported
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44 counterbalancing the presentation of aural and written stimuli, with written preceding aural twice
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46 and aural preceding writing once (one not reported). Only two studies clearly indicated that
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48 they counterbalanced the items between the two modalities.
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These findings reflect a fairly low level of reporting and/or use of counter-balancing in our samples of studies used in the meta-analyses. This systematicity between studies in administering one condition before the other is an important threat to the validity of the results of the meta-analysis. It should be noted that in terms of the comparison between timed and untimed JTs, the systematicity seen in presenting timed before untimed is heavily based on the impact of Ellis (2005), which a significant portion of our subset drew upon in developing their procedure. The potential impact of practice effects may, at least in part, explain (a) the lack of stable effects in the modality meta-analysis and (b) the strong effects found for untimed JTs producing higher scores than timed JTs.

Discussion

This study set out to describe the design and implementation of judgment tasks in L2 research. Our interest in doing so was motivated in part by their popularity in the field: JTs are employed frequently and increasingly in L2 research. Prior to the present study, however, the extent of their usage was unknown. Following a comprehensive search, we identified 302 published studies using 385 JTs. By comparison, Marsden et al. (2018b) identified 64 studies that have employed self-paced reading (SPR) tasks, Watanabe and Koyama's (2008) synthesis of cloze passage research was based on a sample of 33 studies; Yan, Maeda and Ginther (2016) surveyed 76 studies using elicited imitation and included 21 in their meta-analysis; and Bowles (2010) meta-analysed 14 samples using concurrent verbal reports (i.e., think-alouds).

The popularity of JTs in L2 research is likely due to a combination of at least three main factors, the first of which is versatility. Indeed, our results show that JTs have been used to examine learner knowledge on a variety of morphological and syntactic features. JTs have also

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3 been applied frequently to support heavily debated claims regarding the nature of learner
4 knowledge (e.g., Vafaei et al., 2017). Such studies address long-standing, fundamental questions
5 posed and refined since the inception of the field (e.g., learning vs. acquisition; implicit vs.
6 explicit knowledge, declarative vs. automatized knowledge). However, our data showed that
7 only about a quarter of studies using JTs overtly discussed knowledge type. Further, although we
8 observed more of such discussion in more recent publications, we did not observe an increase in
9 terms of the *proportions* of JT studies that have alluded to knowledge type. To put this in
10 context, overt discussions of knowledge types was higher in studies reporting on self-paced
11 reading tests, where 26 out of 64 articles (41%) mentioned knowledge types, with 18 describing
12 the knowledge elicited as automatic/automaticity; 8 as implicit; and 14 as ‘not explicit’ (Marsden
13 et al., 2018b).

14
15 A second contributor to the popularity of JTs—and a consideration for any type of
16 language assessment (see Brown and Douglas, 2004)—is their practicality. JTs can target learner
17 knowledge on numerous features and can be administered and scored very quickly compared to
18 other elicitation methods. By contrast, much greater time and effort is required to elicit, code,
19 and score oral or written production, and with less guarantee of being able to elicit the target
20 feature or control the nature or amount of the feature elicited across participants. Further
21 contributing to their practicality as a research tool is the relative ease in comparing JT data across
22 participant groups (e.g., L1 vs. L2; low vs. high proficiency; learners who started learning at
23 different ages) relative, at least, to elicitations of L2 production.

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25 Third, JTs appear to many to be easy to develop. It might be tempting to simply compose
26 a set of sentences that contain the target feature(s) and to then simply adjust some portion of
27 them to violate the structure(s) in question. As with other types of instruments that appear very

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3 straightforward (e.g., gap fill, questionnaires), though, there are a number of considerations that
4 often go unattended and unexamined (see Spinner & Gass, in press). We have attempted in this
5 study to reveal and scrutinize many such considerations and features. In doing so we hope to
6 impress upon researchers using JTs that these choices merit not only consideration but
7 justification given the goals of the study, the substantive interests, the learner population (e.g.,
8 proficiency), assumptions about the type of knowledge being assessed, and so forth.

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Regardless of the causes for its popularity, the widespread use of JTs requires that we understand the extent of variation in their design and administration, and how such variation can affect the data they elicit. The results of our study show that there has been very little consistency in JT design and administration, even when similar linguistic features were being targeted. To name a few of the design characteristics found to vary: item response types, item randomization, the number of items per target feature, error identification, confidence ratings, comprehension questions, task modality, and time pressure. In this respect, very little has changed since Chaudron's early (1983) review of the use of JTs in L1 and L2 acquisition research, which observed similarly disparate application of this instrument type. Studies in his sample exhibited, for example, both binary and scaled judgements, timed vs. untimed conditions, and a wide variety of target structures. In and of itself, this disparity is not a cause for concern, but our review suggested that researchers are not sufficiently informed about whether and to what extent such characteristics influence results.

To be clear, we are not arguing that all variants of JTs *necessarily* affect the data. We are arguing, rather, that the lack of standardization across instruments presents a potential threat to the validity of findings based on JTs and comparisons across studies. For example, our results for RQ3 indicated that certain test characteristics do indeed influence results. All things being equal,

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3 the results for RQ3 show that studies using untimed JTs can expect participant scores on average
4 to be higher by one standard deviation than if they impose a time limit. To put this in more
5 meaningful terms, the median difference between JT scores on timed and untimed conditions (d
6 = 1.35)—when measuring the *same* target features from the *same* participants—is comparable to
7 the difference typically observed between pre- and post-tests in L2 instruction research (Plonsky,
8 2017; Plonsky and Oswald, 2014). This result might also be interpreted as evidence of the
9 validity of untimed and timed JTs as measures of more explicit and more implicit L2 knowledge,
10 respectively. Making such a claim was not the purpose of our study. What is clear, though, is that
11 our results do align with arguments that propose some discriminant validity of these two JT
12 conditions.
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17 On the other hand, our analysis of the influence on scores of another design feature,
18 modality, did not find such a stark or reliable effect. Nevertheless, this finding is still useful for
19 researchers making JT design decisions, as it shows that any a priori assumptions about the effect
20 of modality require corroboration (see Johnson, 1992; Murphy, 1997). It is also illuminating in
21 that it is, arguably, counter-intuitive. One might have expected aural JTs to impose more
22 difficulties, all things being equal, as listening does not permit back-tracking/regression (other
23 than that provided by a participant's own traces in their phonological store), and listening is often
24 thought to provide less opportunity for participants to access controlled, explicit or
25 metalinguistic knowledge (see discussion attention to form in oral vs. written mode in Morgan-
26 Short, Marsden, Heil, et al. 2018).
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31 Also critical to assessing validity, whether of a single instrument or a study, is the notion
32 of methodological and psychometric transparency. In addition to being coded for different
33 methodological variants, the primary studies in our sample were also coded for whether or not
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3 those features were reported. The results were quite mixed. For example, 43% of the studies that
4 asked participants to identify errors did not report how those data were then handled. The use of
5 time pressure is another example and one that we know to influence task results. Not only did
6 many studies fail to report whether JTs were timed or not, even among those that did report
7 timing, few stated how the time limit was determined. Other frequent ‘sins of omission’
8 regarding task features include not reporting JT modality (18%) or format (i.e., computer-based
9 or paper-and-pencil) (31%).

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19 These results are not particularly surprising. Marsden et al. (2018b) observed a similarly
20 spotty record for the reporting of features of self-paced reading tasks. For example, they found
21 that sentence length was not reported in almost half the studies (30/64); 17/28 SPRs that used
22 multi-word segments did not report the length or number of the segments; 25 of the 50 studies
23 that used comprehension questions following critical trials provided no example of the questions.
24 Similar to the current findings showing a lack of clarity or consistency about what was included
25 in the JT analysis, Marsden et al. also found low levels of consistency (where consistency might
26 be expected, i.e. between related studies) about which regions were analyzed and whether
27 reaction times were measured for individual words or for the sum or means of multi-word
28 segments. Further reflecting the current study’s findings about the unequal or unreported balance
29 between grammatical/acceptable and ungrammatical/unacceptable items, Marsden et al. found
30 that in SPRs the balance between number of items in different conditions was not always
31 reported, varied widely between studies, and did not align with recommended ratios. The extent
32 to which conditions are balanced is important as too many items per condition can fatigue
33 participants, desensitize them to ungrammaticality, or raise their awareness about the
34 manipulations, whereas too few items per condition may fail to provide a stable result.
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3 Another feature associated with transparency and instrument validity is reliability.
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5 Unfortunately, Chaudron's (1983) blunt assessment that "reliabilities have generally not been
6 reported" (p. 368) still applies. Only 16% of the sample reported an estimate of instrument
7 reliability. This domain lags behind many others in L2 research (see Al-Hoorie and Vitta, 2018;
8 Plonsky and Derrick, 2016; Plonsky and Gass, 2011; but cf. Marsden et al., 2018b).
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14 More important than the availability of reliability estimates is, of course, the actual
15 reliability of the instruments employed. In this context, reliability refers to the internal
16 consistency of the measurement tool. Higher reliability indicates greater consistency and
17 therefore less error in individual and group estimates of learner knowledge. In order to determine
18 the reliability across the JTs in our sample, we recorded and meta-analyzed each estimate that
19 was available. The median reliability coefficient was .8 (interquartile range = .22). This value
20 aligns very closely with Plonsky and Derrick's (2016) meta-analytic estimate of instrument
21 reliability throughout quantitative L2 research (.82, interquartile range = .15). We recognize,
22 though, that the reliability estimates that are available in published studies may not be
23 representative of the population of JT reliabilities; reported estimates are likely to be higher than
24 those that go unchecked and/or unreported.
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40 A final issue related to methodological transparency, one closely related to addressing
41 some, if not all, of the 'sins of omission' discussed above, was the extent to which the full
42 instrument itself was available. We found 64% of our JTs were not available, thus restricting our
43 own capacity to synthesize some of the methodological decisions taken and the field's capacity
44 to scrutinize the validity and reliability of JT research. Although low, this is in fact a slightly
45 higher level of transparency than that found for SPR tests by Marsden et al. (2018b), where 77%
46 (49/64) of studies had only a brief example (e.g., one or two items) of stimuli available in the
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3 article. This generally low level of transparency threatens the replicability of research, as
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5 replication studies either have to extrapolate full instruments from a few examples in the article,
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7 or re-construct the entire instrument, both routes introducing significant heterogeneity into any
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9 replication study (see Marsden et al., 2018a, for further discussion). This situation may change as
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11 more journals encourage their authors towards greater transparency (by, for example, adopting
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13 the Centre for Open Science badge scheme (Blohowiak et al., 2016) shown to increase long-term
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15 availability of materials and data (Kidwell et al., 2016)).
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19 Also with a view to promoting greater transparency, and as a follow-up to the current
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21 study, we have established a ‘special collection’ of JTs on IRIS. The collection now holds 107
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23 JTs from our synthesis and the metadata for 317 individual JTs (out of the total 385 JTs from 302
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25 studies in our synthesis). Considering that many JTs were used for research published several
26
27 decades ago, the generally positive response to our requests demonstrated to us a willingness of
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29 the field to engage in a more collaborative and synthetic effort. We hope that this JT collection
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31 will serve as a reference corpus for future syntheses and as resource for training and replication
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33 research.
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36 37 *Suggestions for Future Research*

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40 Our interest in this study was not solely in the use of JTs as matter of history. We are just
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42 as interested—if not more so—in future applications of this particular type of tool. With that
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44 interest in mind, we offer here a number of suggestions based on our results.
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48 Our review has shown many of the same limitations observed elsewhere in L2 research in
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50 terms of target populations and demographics. For example, a small range of languages comprise
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52 the bulk of L1s and L2s involved (see Spinner, 2011). Somewhat unique to this body of research
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54 is the distribution of proficiency levels found in our sample. Whereas L2 research as a whole has
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3 focused more often on relatively equal amounts of samples described as beginner/novice or
4 intermediate (Plonsky, 2017), the samples found in studies employing JTs were much more
5 likely to be labeled as advanced. One reason for this may be that there has tended to be interest in
6 advanced stages of the learning process in generative research, and 50% of our JTs were situated
7 within a generative framework (for example, generative-inspired research that focused on
8 whether L2ers can attain native-like levels of sensitivity to grammatical constraints).
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10 Nevertheless, researchers employing JTs would do well to sample a wider range of
11 demographics and research contexts as a means to assess generalizability. We also suggest an
12 increase in the size of the samples in order to achieve more precise, reliable findings.
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24 Our results also point to several suggestions pertaining to the design and implementation
25 of JTs. The general recommendation here is to strive for greater transparency with respect to the
26 features reported in our review. We would highlight among them the importance of piloting,
27 stating JT modality and format, stating whether the task was timed and, if so, how this was
28 calculated, making instruments available for future studies and replications, and calculating and
29 reporting instrument reliability. See Marsden et al. (2018a) for evidence that methodological
30 transparency makes research more replicable and renders findings from related studies more
31 interpretable. More fundamentally, we also call on our colleagues to make more deliberate and
32 justifiable choices regarding the design of their instruments, particularly when claims about
33 different types of L2 knowledge are involved. Reviewers and editors, too, must require greater
34 transparency in this regard. Study pre-registration provides yet another approach to avoiding
35 what often comes across as methodological idiosyncrasy. This step, taken before data collection
36 has begun, can demonstrate to readers that the design and analyses of a given study were
37 determined and registered a priori, and deviation from them did not occur as data was being
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3 collected or analysed. For further explication on the benefits of and rationale for pre-registration
4 and also, critically, the benefits of peer-review of studies before data is collected, we recommend
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6 Marsden, Morgan-Short, Trofimovich, and Ellis' (2018) editorial on registered reports in
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8 *Language Learning*. Outside of applied linguistics, we recommend Wagenmakers, Wetzels,
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10 Borsboom, van der Maas, and Klevit (2012) and Munafò et al. (2017).
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15 The final area we recommend for future research also fits within the domain of the SLA-
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17 assessment interface (see Norris and Ortega, 2012; Gu, 2014). Recall from our results the
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19 relatively small number of studies (29%) that directly triangulated and/or compared the scores of
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21 JTs and other instruments intended to measure the same linguistic target(s) using one or more
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23 statistical analyses (see for example, Spada, Shiu, and Tomita, 2015). More research is needed
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25 here as well. Again, this recommendation is not new. Similar to Chaudron (1983), who argued
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27 that “judgments should be validated by other measures” (p. 369), we would also encourage
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29 authors to provide evidence of concurrent validity by administering multiple and complementary
30
31 assessments of target constructs. Here, too, reviewers must play a critical role, insisting that
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33 researchers provide evidence of validity and reliability. We hope that the current climate of
34
35 enhanced methodological awareness and assessment literacy (e.g., Grabowski and Dakin, 2014)
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37 will further encourage researchers in the field to embrace this idea.
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42 **Conclusion**

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44 Our study demonstrates a heavy reliance on acceptability judgement data in L2 research.
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46 Although our sample of studies reflected a wide variety of theoretical perspectives and linguistic
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48 features, JT usage seemed relatively unified in its purpose: to inform researchers about learners'
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50 sensitivity to norms in the input. However, our data have not allayed concerns about the wide
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52 variety of designs and administration conditions of JTs, as the effects of this variety on the
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3 interpretability of JT data remain largely unknown. Partially addressing these concerns here, we
4 provided data on the likely effects of two key methodological decisions. We found that imposing
5 time constraints leads to substantially decreased scores, whereas modality did not strongly
6 influence outcomes. Regarding the other methodological characteristics that we examined (such
7 as the balance between grammatical/acceptable and ungrammatical/unacceptable conditions,
8 asking participants to locate or correct an unacceptable feature, providing scaled versus
9 dichotomous response options), we highlighted (a) the need for empirical scrutiny to build an
10 evidence base for more informed standardization in such methodological decisions and (b)
11 improved reporting and transparency of the methodological characteristics of JTs.
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	Authors	Journal	Year	oral vs. written	timed vs. untimed
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4					
5	Abrahamsson	0	2012		
6	Abrahamsson & Hyltenstam	0	2007		
7	Abrahamsson & Hyltenstam	2	2009		
8	Abrahamsson & Hyltenstam	2	2009		
9	Akakura	6	2012		
10	Albirini & Benmamoun	18	2015		
11	Alibabae, Momenzadeh, & Abarghoui	14	2014		
12	Al-Surmi	3	2012		
13	Al-Surmi	3	2012		
14	Ammar, Lightbown, & Spada	10	2010		
15	Anderson	53	2007		
16	Anderson	53	2007		
17	Andringa, de Gloppe, & Hacquebord	2	2011		
18	Andringa & Curcic	0	2015		
19	Ard & Gass	0	1987		
20	Ard & Gass	0	1987		
21	Athanasopoulos	33	2007		
22	Au, Oh, Kightly, Jun, & Romo	39	2008		
23	Balcom	16	1997		
24	Balcom	61	1999		
25	Balcom	61	1999		
26	Balcom	61	1999		
27	Bardovi-Harlig & Dornyei	13	1998		
28	Barrios & Bernaro	40	2012		
29	Batterink & Neville	22	2013		
30	Batterink & Neville	22	2013		
31	Bautista	25	2004		
32	Bialystok	2	1979		
33	Bialystok	16	1997		
34	Bialystok & Majumder	17	1998		
35	Bialystok & Majumder	17	1998		
36	Bialystok & Miller	18	1999	*	
37	Bialystok & Miller	18	1999	*	
38	Bianchi	18	2013		
39	Birdsong	38	1992		
40	Birdsong	38	1992		
41	Bley-Vroman, Felix, & Ioup	16	1988		
42	Bley-Vroman & Yoshinaga	16	2000		
43	Bordag & Pechmann	18	2007		
44	Bordag & Pechmann	18	2007		
45	Borgonovo, Bruhn de Garavito, &	0	2015		
46	Bowles	0	2011		^
47	Bowles	0	2011		^
48	Bruhn de Garavito & Valenzuela	18	2008		
49	Bylund, Abrahamsson, & Hyltenstam	0	2012		
50	Bylund, Abrahamsson, & Hyltenstam	0	2012		
51	Cho	27	2016		
52	Cho & Slabakova	1	2015		
53	Choi & Clark	19	2006		
54	Chung	16	2012		
55	Chung	16	2012		
56	Clahsen, Martzoukou, & Stavrakaki	16	2010	*	
57	Clahsen, Martzoukou, & Stavrakaki	16	2010		
58	Clahsen et al.	16	2013		
59	Conradie	16	2006		
60	Conradie	16	2006		

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Conroy & Cupples	0	2010
Cook	4	2003
Cormier et al.	55	2012
Coughlin & Tremblay	17	2013
Cox	1	2015
Cox & Sanz	4	2015
Cuza	27	2012
Cuza & Frank	56	2011
Cuza & Frank	16	2015
Cuza et al	27	2012
Cuza, Perez-Leroux, & Sanchez	0	2012
Davis & Kaplan	1	1998
deGraff	0	1997
deGraff	0	1997
DeJong	0	2005
DeKeyser	0	2000
Dekeyser, Alfi-Shabtay, & Ravid	17	2010
Dekeyser, Alfi-Shabtay, & Ravid	17	2010
Dekydspotter, Sprouse, & Anderson	53	1999
Della Putta	0	2016
Derwing, Rossiter, & Ehrensberger-	10	2002
Derwing, Rossiter, & Ehrensberger-	10	2002
Doughty	0	1991
Doughty	0	1991
Duffield & Matsuo	0	2009
Duffield & White	16	1999
Dugarova	16	2014
Dussias & Pinar	16	2010
Ehrich & Meuter	50	2009
Ellis	0	1991
Ellis	0	1991
Ellis	0	2005
Ellis	0	2005
Ellis	1	2006
Ellis	1	2006
Ellis, Loewen, & Erlam	0	2006
Erçertin & Alptekin	17	2013
Erçertin & Alptekin	17	2013
Erlam & Loewen	20	2010
Falk & Bardel	16	2010
Fallah, Jabbari, & Fazilatfar	16	2015
Felsner, Roberts, Marinis, & Gross	17	2003
Felser, Sato, & Bertenshaw	18	2009
Felser, Sato, & Bertenshaw	18	2009
Finney	16	1997
Flege & Liu	0	2001

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3	Flege, Yen-Komshian, & Liu	39	1999
4	Fotos	13	1994
5	Fotos & Ellis	13	1991
6	Gabriele	0	2009
7	Gabriele	0	2009
8	Gabriele	0	2009
9	Gabriele & Canales	28	2011
10	Gao	21	2014
11	Gao	21	2014
12	Garcia-Mayo & Slabakova	27	2014
13	Gass	2	1979
14	Gass	13	1983
15	Giancasparo, Halloran, & Iverson	18	2015
16	Godfroid et al.	0	2015
17	Godfroid et al.	0	2015
18	Godfroid et al.	0	2015
19	Goo	0	2012
20	Gor et al	65	2007
21	Gor et al	65	2007
22	Granena	1	2014
23	Granena	1	2014
24	Grey, Cox, Serafini, & Sanz	8	2015
25	Grey, Williams, & Rebuschat	0	2014
26	Grey, Williams, & Rebuschat	0	2014
27	Grey, Williams, & Rebuschat	0	2014
28	Guijarro-Fuentes	18	2012
29	Guijarro-Fuentes & Larranaga	27	2011
30	Guijarro-Fuentes & Larranaga	27	2011
31	Guion & Flege	17	2000
32	Gutierrez	61	2012
33	Gutierrez	61	2012
34	Han & Ellis	6	1998
35	Han & Ellis	6	1998
36	Han & Ellis	6	1998
37	Hara	16	2007
38	Hawkins	16	1987
39	Hawkins & Chan	16	1997
40	Hawkins & Chan	16	1997
41	Hawkins, Towel, & Bazergui	16	1993
42	Hedgcock & Lefkowitz	30	1993
43	Hermas	63	2010
44	Hermas	63	2010
45	Hermas	63	2010
46	Hermas	63	2010
47	Hermas	63	2010
48	Hermas	63	2014
49	Hermas	63	2014
50	Hermas	63	2014
51	Hermas	63	2014
52	Hermas	63	2014
53	Hermas	63	2014
54	Hermas	63	2014
55	Hermas	27	2015
56	Hermas	27	2015
57	Hermas	27	2015
58	Hopp	16	2005
59	Hopp	16	2006
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Hopp	18	2009
Hopp	28	2010
Hopp	28	2010
Hu & Liu	16	2007
Huang	34	2014
Hulk	16	1991
Hwang & Lardiere	16	2013
Hwang & Lardiere	16	2013
Hylstenstam, Bylund, Abrahamsson, &	18	2009
Hylstenstam, Bylund, Abrahamsson, &	18	2009
Hylstenstam, Bylund, Abrahamsson, &	18	2009
Indrarathne & Kormos	0	2016
Inagaki	0	2001
Inagaki	0	2001
Ionin & Montrul	2	2010
Ionin & Wexler	16	2002
Ioup et al.	0	1994
Isabelli-Garcia	30	2010
Ivanov	16	2012
Izumi	2	2003
Izumi, Bigelow, Fujiwara, & Fearnow	0	2010
Izumi & Lakshmanan	16	1998
Jabbari & Salimi	48	2015
Jackson & Roberts	17	2010
Jia & Aaronson	17	2003
Jia, Aaronson, & Wu	17	2002 *
Jia, Aaronson, & Wu	17	2002
Johnson	2	1992
Johnson & Newport	66	1989
Johnson & Newport	55	1991
Johnson & Newport	55	1991
Johnson, Shenkman, Newport, & Medin	39	1996
Juffs	16	1998
Juffs	68	2004
Juffs	16	2005
Juffs & Harrington	2	1996
Jung	6	2016
Kadia	13	1988
Kadia	13	1988
Kang	4	2009
Khatib & Nikouee	32	2012
Kovelman, Shalinsky, Berens, & Petitto	35	2008
Kovelman, Shalinsky, Berens, & Petitto	35	2008
Kupferborg & Olshtain	10	1996
Kupisch	18	2012
Kweon & Bley-Vroman	16	2011

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3	Lan, Sung, Cheng, & Chang	7	2015
4	Larson-Hall	16	2008
5	Leon, Tsung, & Tse, Shum, & Ki	27	2012
6	Leon, Tsung, & Tse, Shum, & Ki	27	2012
7	Leon, Tsung, & Tse, Shum, & Ki	27	2012
8	Leung	18	2005
9	Leung	18	2005
10	Li	8	2013
11	Li, Ellis, & Zhu	29	2016
12	Lightbown & Spada	10	2000
13	Lightbown & Spada	10	2000
14	Lightbown & Spada	10	2000
15	Loewen & Erlam	9	2006
16	Loewen & Erlam	9	2006
17	Loewen & Erlam	9	2006
18	Lozano	69	2002
19	Lozano	69	2002
20	Lozano	16	2006
21	Lozano	16	2006
22	Mai & Yuan	16	2016
23	Mai & Yuan	16	2016
24	Mandell	16	1999
25	Marian, Blumenfeld, & Kaushanskaya	37	2007
26	Marsden	16	2008
27	Marsden	16	2008
28	Marsden	58	2009
29	Marsden & Chen	2	2011
30	Masny	10	1997
31	Masny	10	1997
32	Masny & d'Anglejan	34	1985
33	Masny & d'Anglejan	34	1985
34	Mayberry & Lock	47	2003
35	McDonald	17	2000
36	McDonald	17	2000
37	McDonald	39	2006
38	McDonald & Roussel	18	2010
39	McManus	4	2013
40	McManus & Marsden	0	2016
41	McManus & Marsden	0	2016
42	McManus & Marsden	0	2016
43	Meunier & Littre	8	2013
44	Meunier & Littre	8	2013
45	Monteiro	7	2014
46	Monteiro	7	2014
47	Monteiro	7	2014
48	Montrul	16	1999
49	Montrul	27	2006
50	Montrul	27	2006
51	Montrul	27	2006
52	Montrul	17	2010
53	Montrul	16	2010
54	Montrul	16	2010
55	Montrul	18	2014
56	Montrul & Bowles	18	2009
57	Montrul & Bowles	18	2009
58	Montrul & Bowles	18	2009
59	Montrul & Bowles	56	2010
60	Montrul et al.	18	2014

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3	Montrul & Ionin	18	2010	
4	Montrul & Ionin	8	2012	
5	Montrul & Ionin	8	2012	
6	Montrul & Ionin	8	2012	
7	Morgan et al.	23	2002	
8	Morgan-Short et al.	18	2014	
9	Murphy	16	1997	*
10	Murphy	16	1997	*
11	Murphy	16	1997	
12	Murphy	16	1997	
13	Murphy	16	1997	
14	Nabei & Swain	10	2002	
15	Nagano	59	2015	
16	Nagano	59	2015	
17	Nagano	59	2015	
18	Neubauer & Clahsen	0	2009	
19	Oh	16	2010	
20	Omaki & Schulz	0	2011	
21	Omaki & Schulz	0	2011	
22	Paap & Liu	43	2014	
23	Pae et al.	34	2013	
24	Papadopoulou & Clahsen	0	2003	
25	Papadopoulou & Clahsen	0	2003	
26	Papadopoulou & Clahsen	0	2003	
27	Papadopolou et al.	16	2010	
28	Paradis	2	2010	
29	Paribakht & Bingham Wesche	54	1993	
30	Paribakht & Bingham Wesche	54	1993	
31	Park & Starr	63	2015	
32	Parodi & Tsimpli	16	2005	
33	Parodi & Tsimpli	16	2005	
34	Parodi & Tsimpli	16	2005	
35	Parodi & Tsimpli	16	2005	
36	Perpiñán	18	2015	
37	Pfenniger	62	2014	
38	Pires & Rothman	27	2009	
39	Polomska	16	1988	
40	Potowski, Jegerski, & Morgan-Short	2	2009	
41	Potowski, Jegerski, & Morgan-Short	2	2009	
42	Prentza	53	2014	
43	Rassaei	8	2014	
44	Rebuschat & Williams	17	2012	
45	Rebuschat & Williams	17	2012	
46	Rebuschat & Williams	17	2012	
47	Reinders	6	2009	
48	Renou	30	2000	*
49	Renou	30	2000	*
50	Renou	10	2001	*
51	Renou	10	2001	*
52	Renou	10	2001	*
53	Revesz	2	2012	
54	Rinke & Flores	18	2014	
55	Robenalt & Goldberg	2	2016	
56	Robenalt & Goldberg	2	2016	
57	Roberts & Felsner	17	2011	
58	Roberts, Gullberg, & Indefrey	0	2008	
59	Roberts & Liszka	16	2013	
60	Roberts & Liszka	16	2013	

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3	Robinson	0	1996	
4	Robinson	0	1997	
5	Robinson	0	2005	
6	Robinson	0	2005	
7	Robinson & Ha	0	1993	
8	Robinson & Ha	0	1993	
9	Rogers	13	2015	
10	Rogers, Revesz, & Rebuschat	17	2016	
11	Rothman	27	2007	
12	Rothman	27	2009	
13	Rothman	27	2009	
14	Rothman & Cabrelli Amaro	16	2010	
15	Rothman & Cabrelli Amaro	16	2010	
16	Rothman & Cabrelli Amaro	16	2010	
17	Rothman & Iverson	64	2007	
18	Rothman & Iverson	0	2013	
19	Rothman & Iverson	0	2013	
20	Sabourin, Stowe, & de Haan	16	2006	
21	Sagarra & Herschensohn	28	2010	
22	Sagarra & Herschensohn	2	2011	
23	Sajjadi & Tahririan	4	2001	
24	Salaberry	4	2013	
25	Santoro	16	2007	
26	Sanz et al	2	2009	
27	Schachter	16	1976	
28	Schachter	16	1990	
29	Schachter & Yip	0	1990	
30	Schulz	16	2011	
31	Schulz	16	2011	
32	Schmid, Gilbers, & Nota	16	2014	
33	Shirai & Kuruno	2	1998	
34	Slabakova	16	1999	
35	Slabakova	16	2003	
36	Slabakova	16	2006	
37	Slabakova	16	2006	
38	Slavoff & Johnson	0	1995	
39	Snape	18	2008	
40	Soler	59	2014	
41	Soler	27	2015	
42	Soler	27	2015	
43	Sopata	18	2005	
44	Spada, Shiu, & Tomita	2	2015	*
45	Spada, Shiu, & Tomita	2	2015	*
46	Spinner	2	2013	
47	Spinner	2	2013	
48	Stafford	1	2013	
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3	Stafford, Bowden, & Sanz	2	2011
4	Stringer et al.	16	2011
5	Svalberg	25	1998
6	Tagarelli	0	2016
7	Tanner, McLaughlin, Herschensohn, &	18	2013
8	Tolentino & Tokowicz	2	2014
9	Tolentino & Tokowicz	2	2014
10	Torito	18	2001
11	Toth	0	2000
12	Toth	2	2006
13	Toth	2	2008
14	Toth & Guijarro-Fuentes	17	2013
15	Towell, Hawkins, & Bazergui	0	1993
16	Trahey	16	1996
17	Trahey & White	0	1993
18	Tremblay	16	2006
19	Tsang	27	2015
20	Uziel	16	1993
21	Vafae, Suzuki, & Kachisnke	0	2016
22	Vafae, Suzuki, & Kachisnke	0	2016
23	Vulchonova, Talcott, Vulchanov, &	43	2012
24	Westergard et al.	27	2016
25	White	2	1985
26	White	0	1987
27	White	0	1987
28	White	0	1987
29	White	58	1990
30	White	58	1990
31	White et al.	2	1997
32	White et al.	2	1997
33	White et al.	2	1997
34	White et al.	59	2012
35	White & Genesse	16	1996
36	White & Ranta	10	2002
37	Whong-Barr & Shwartz	0	2002
38	Winitz	8	1996
39	Xu	16	2014
40	Yalcin & Spada	0	2016
41	Yalcin & Spada	0	2016
42	Yang & Givon	0	1997
43	Yang & Li	45	2012
44	Yeganeh, Ghoreyshi, & Darabi	31	2013
45	Yen-Konshian, Robbins, & Flege	17	2001
46	Yuan	2	1995
47	Yuan	18	2007
48	Yuan	16	2007
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Yuan	16	2010
Yuan	27	2013
Yuan	16	2014
Yuan & Dugarova	0	2012
Yuan & Zhao	27	2010
Zhang	8	2012
Zhang	21	2015
Zhang	21	2015
Zhang	0	2015
Zhang	0	2015
Zhang	17	2015
Zhao	16	2014
Zhao	16	2014
Zyzik	16	2008

For Peer Review

Appendix A

*List of 69 Coded Features with Reliability Estimates***Publication Information**

1. *Authors*
2. *E-mail*
3. *Journal*
4. *Year of publication*

Study Context

5. *context* ($\kappa = .757$; 92% agreement) – are participants learning the target language for a class or just for the study? (0 = classroom learner, 1 = learned for study, 2 = non-classroom learners of the language)
6. *shot_dev* ($\kappa = .946$; 95% agreement) – are participants tested for knowledge at a specific moment in time or to measure development over time (0 = one shot on “static Knowledge”, 1 = developmental)
7. *effect* (83% agreement, too few observations to determine κ) – If developmental, was the GJT used to measure effectiveness of intervention/treatment? (0=not used as an outcome measure, 1=yes used as an outcome measure)
8. *delay_ptest* (100% agreement) – If used as outcome measure, was a delayed post-test conducted? (0 = no, 1 = yes)
9. *comp_prof* ($\kappa = .838$) (94% agreement) – Was data collected and compared across proficiency levels? (0 = no, 1 = yes)
10. *L1* ($\kappa = .940$) (95% agreement) – What was the first language of participants?

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4 11. *L1s_Comp* (limited observations) – If multiple L1s amongst the learners (between-
5 subject), were their results compared (i.e., differences across L1 groups)? (0 = no, 1 =
6 yes)
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10 12. *L2_if_L3* (limited observations) – If target language was a third language, what was the
11 second language of participants?
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14 13. *TL* ($\kappa = .862$) (89% agreement) – What was the target language of the study?
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17 14. *SL_FL* ($\kappa = .738$) (84% agreement) – What was the learning context of the study? (0 =
18 SL context, 1 = FL context, 2 = not real, a priori learners (e.g. a new, real language but
19 first exposure), 3 = artificial language, 999 = not reported)
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23 15. *htg* ($\kappa = .849$, 98% agreement) – Were the participant heritage language learners (0 = no,
24 1 = yes)
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27 16. *beg* – ($\kappa = .423$, 76% agreement) Were participants at beginner proficiency as reported by
28 author (or up to 2 years of study) (0 = no, 1 = yes, 2 = not reported)
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32 17. *int* – ($\kappa = .671$, 81% agreement) Were participants at intermediate proficiency as reported
33 by author (0 = no, 1 = yes, 2 = not reported)
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37 18. *adv* – ($\kappa = .639$, 79% agreement) Were participants at advanced proficiency as reported
38 by author (or up to 2 years of study) (0 = no, 1 = yes, 2 = not reported)
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42 19. *unint* ($\kappa = .632$, 94% agreement) – Was the proficiency level uninterpretable from the
43 description provided? (0 = no, 1 = yes)
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47 20. *profJust* ($\kappa = .651$, 79% agreement) – How did the authors justify their proficiency
48 labels? (0=no justification (opinion), 1=assumed from educational/intititutional level,
49 2=standardised proficiency test (IELTS, TOEFL, ACTFL, A-level, CEFR, etc.), 3=
50 other)
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3 21. *age* ($\kappa = .774$, 94% agreement) – What was the age range of participants? (0 = child: 0-1,
4 1 = teen: 13-17, 2 = adult 18+, 3 = multiple, 4 = not reported)
5
6
7
8 22. *inst* ($\kappa = .811$, 90% agreement) – What was the institutional level of participants? (0 =
9 elementary/primary (up to 13'ish), 1 = secondary (14-18'ish), 2 = tertiary
10 college/university, 3 = language institute, 4 = not classroom learners/users, 999 = not
11 reported)
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17 23. *NS_ctrl* ($\kappa = .822$, 92% agreement) – Were NNS GJTs compared to a NS "control"
18 group? (0 = no, 1 = yes)
19
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22 24. *N_L2* (Cronbach's Alpha = .995) – What was the sample size of the L2 participants?
23
24 25. *N_NS* (Cronbach's Alpha = .960) – What was the sample size of the native speaker
25 control group?
26
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29

30 Judgment Task Design

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33 26. *knowledge* ($\kappa = .622$, 87% agreement) – Were assumption made about type(s) of
34 knowledge being tapped? (0 = no, 1 = yes, implicit, procedural, and/or automatized, 2 =
35 yes, explicit and/or declarative, 3 = yes, 1+2, 4 = yes, but not clear whether 1 or 2)
36
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40 27. *K_targeted* (Cronbach's Alpha = .390) – Number of items in judgment task not including
41 distractors
42
43
44 28. *K-ungramm* (Cronbach's Alpha = .946) – Number of K-targeted items that were
45 ungrammatical
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49 29. *K_distractors* (Cronbach's Alpha = .656) – Number of items in judgment task not
50 including distractors
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54 30. *break* ($\kappa = .571$, 94% agreement) – Were participants offered breaks during the task? (0 =
55 no, 1 = yes, 2 = not reported)
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3 31. *mode* ($\kappa = .686$, 84% agreement) – What modality was the judgment task delivered in? (0
4 = aural, 1 = written, 2 = both, 3 = not reported, 4 = visual, 5 = half aural/half written)
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8 32. *format* ($\kappa = .595$, 71% agreement) – How were item responses recorded? (0 =
9 paper/pencil, 1 = computer-based, 2 = unreported, 3 = spoken)
10
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12 33. *#_of_features* (Cronbach's Alpha = .798) – How many grammatical features were
13 targeted in the judgment task?
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15
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17 34. *morphosyn_type* ($\kappa = .780$, 81% agreement) – What were the morphosyntactical target(s)
18 of the judgment task?
19
20
21 35. *incidental* ($\kappa = .625$, 85% agreement) – Was the morphosyntactic target incidental to the
22 overall goal of the study? (0 = No, 1 = Yes)
23
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26 36. *timelimit* ($\kappa = .647$, 79% agreement) – Was a limit given for how long the participant had
27 to complete an item? (0 = no, 1 = yes, 2 = not reported)
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31 37. *time_calc* ($\kappa = .449$, 67% agreement) – If time limit used, was time calculated based on
32 (0 = native speaker norms, 1 = non-native norms, 2 = other, 3 = not reported)
33
34
35 38. *adjust_time* (100% agreement) – If native norms used for timing, what percentage of time
36 to complete a judgment was added? (0 = not reported, 1 = 10%, 2 = 20%, 3 = 30%. 4 =
37 1.2 x NS RT)
38
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41
42 39. *time_provided* – (100% agreement) For Timed JTs, the length of time provided to
43 respond to an individual item (in seconds).
44
45
46
47 40. *timed* ($\kappa = .438$, 68% agreement) – Was how long they took to complete an item
48 measured? (0 = no, 1 = yes, 2 = not reported)
49
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52 41. *identify* ($\kappa = .294$, 54% agreement) – Were participants asked to identify in some way
53 incorrect items? (0 = no, 1 = yes, 2 = not reported)
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3 42. *fix* ($\kappa = .294$, 54% agreement) – If asked to identify incorrect statements: do
4
5 participants...? (0 = nothing, 1 = fix the error, 2 = just indicate problem area (e.g., circle
6
7 it), 3 = both)
8
9
- 10 43. *timing* – (limited observations) If corrections were made to incorrect items, when were
11
12 they made? (0 = none/not relevant, 1 = corrections made WHILE making judgments, 2 =
13
14 corrections made afterwards, 3 = both)
15
16
- 17 44. *InCorrCorrection* – (limited observations) If corrections were made, what did researchers
18
19 do with INCORRECT corrections? (0 = not reported, 1 = marked correct, 2 = marked
20
21 incorrect, 3 = removed from analyses, 4 = errors analyzed, 5 = Partial Scoring Used)
22
23
- 24 45. *CB_forms* ($\kappa = .109$, 50% agreement) – Were 2 counter-balanced but otherwise identical
25
26 forms of the JT used? (0 = no, 1 = yes, 2 = not reported)
27
28
- 29 46. *item_rand* ($\kappa = .399$, 66% agreement) – Were Items randomized across participants? (0 =
30
31 no, 1 = yes, 999 = not reported)
32
- 33 47. *bal_corr* ($\kappa = .614$, 74% agreement) – Were the number of correct and incorrect items
34
35 balanced? (0 = equal, 1 = unequal, 2 = not reported)
36
37
- 38 48. *context?* ($\kappa = .796$, 95% agreement) – Were items presented within a larger context? (0 =
39
40 no context reported, 1 = paired with picture/video, 2 = contextual sentence/story
41
42 provided, 3 = targets embedded within larger prose [e.g., passage correction])
43
44
- 45 49. *response_type* ($\kappa = .759$, 85% agreement) – What response type was utilized? (0 =
46
47 dichotomous [correct vs. incorrect, [un]acceptable], 1 = scale of
48
49 acceptability/grammaticality, 2 = dichotomous + "I don't know"/not sure option, 3 = not
50
51 reported, 4 = other, see scale descriptor)
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3 50. *scale* (limited observations) – If a scale of acceptability was used, how many options
4 were on the scale (e.g., 5)?
5
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7
8 51. *scale_analysis* (limited observations) – Was the scale analyzed as described, or as
9 dichotomous? (1 = as described, 2 = dichotomous, 3 = mean score, 4 = other, 999 = not
10 reported)
11
12
13
14 52. *judg_con* ($\kappa = 1.000$, 100% agreement) – Did participants rate confidence of response? (0
15 = no, 1 = yes)
16
17
18
19 53. *judg_basis* ($\kappa = 1.000$, 100% agreement) – Did participants rate basis of judgment (e.g.,
20 intuition/feel vs. rule)? (0 = no, 1 = yes)
21
22
23
24 54. *compQ* ($\kappa = 1.000$, 100% agreement) – Were participants asked to answer comprehension
25 question after judgment? (0 = no, 1 = yes)
26
27
28
29 55. *compQ_ornt* – (limited observations) What was the learners' attention oriented to in the
30 comprehension question? (0 = meaning, 1 = more info in addition to meaning, 2 = other,
31 3 = not reported)
32
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35 56. *instructions* – (open response) What were the instructions for the JT?
36
37
38 57. *instruct_source* ($\kappa = .381$, 89% agreement) – List the instructional source for
39 *Instructions*. (0 = paraphrase, 1 = description from text, 2 = exact directions)
40
41
42 58. *gram_ungramcoding* ($\kappa = .65$, 82% agreement) – Are grammatical and ungramamtical
43 items analyzed separately? (0 = no, 1 = yes)
44
45
46
47 59. *rel* ($\kappa = .896$, 97% agreement) – Was a reliability estimate for the judgment task
48 provided? (0 = no, 1 = yes)
49
50
51 60. *rel_index* – (100% agreement) If reliability reported, which index was used? (0 = none, 1
52 = Cronbach's alpha, 2 = interrater (%), 3 = other, 4 = both, 999 = Not reported)
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3 61. *rel_est* – (100% agreement) If reported, what was the reliability estimate given?
4
5
6 62. *Other_instr* ($\kappa = .740$, 89% agreement) – Was another instrument used to triangulate data
7
8 from judgment task? (0 = no, 1 = yes)
9
10 63. *other_inst_type* – (open response) If other instrument, what kind of OTHER instrument
11
12 was used?
13
14 64. *other_inst_comp* ($\kappa = .721$, 84% agreement) – If other instrument used, did authors
15
16 attempt to correlate/compare with other instrument(s) in a statistical way? (0 = no [just
17
18 part of a battery], 1 = yes [some statistical correlation, validation, comparison made])
19
20 65. *task_avail* ($\kappa = .732$, 89% agreement) – Is the judgment task publically available? (0 =
21
22 no, 1 = yes, in the article, 2 = yes, on IRIS, 3 = yes, in the article + IRIS, 4 = yes,
23
24 somewhere else [e.g., authors' website])
25
26 66. *multi_JTs* ($\kappa = .453$, 74% agreement) – Did the study compare results of multiple
27
28 judgment tasks to test different task features/conditions? (0 = no, 1 = yes)
29
30
31 67. *name_of_test* – (open response) What terminology did the authors use to refer to their
32
33 judgment task?
34
35
36 68. *test_code* ($\kappa = .566$, 78% agreement) – What terminology did the authors use to refer to
37
38 their judgment task? (0 = No Title Provided, 1 = Grammaticality Judgement Test/Task, 2
39
40 = Acceptability Judgement Test/Task, 3 = Truth-Value Judgement Test, 4 = Forced-
41
42 Choice Elimination Task, 5 = Grammar Preference Task, 6 = Aural Grammar Test, 7 =
43
44 Questionnaire, 8 = Grammar Scan, 9 = Sentence Judgement/Interpretation Task, 10 =
45
46 Aural Judgement Task, 11 = Written Judgement Task, 12 = Sentence Completion Task,
47
48 13 = Moving Window Experiment, 14 = Story (Compatibility) Task, 15 = Aural Priming
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50 Task, 16 = Context Evaluation/Matching Task, 17 = Forced Choice Task, 18 =
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3 Explanation Task, 19 = Correction Task, 20 = Working Memory Reading Span Task, 21
4 = Picture Sentence Acceptability/Judgement) Task, 22 = Behavioral ERP Assessment, 23
5 = Plausability Rating Task, 24 = Picture Description Task, 25 = Passage Correction Task,
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10 26 = Word Order Judgement Task, 27 = Acceptabilty Rating Task, 28 = Context
11
12 Felicitousness Task, 29 = Preference Task, 30 = Picture Matching/Selection Task, 31 =
13
14 Self-Paced Reading Task)

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16
17 69. *framework* ($\kappa = .757$, 77% agreement) – What theoretical framework did the authors
18
19 employ? (0 = Unclear, 1 = Formal, 2 = Functional, 3 = Usage-Based, 4 = Skill-
20
21 Acquisition, 5 = Input Processing and Processing Instruction, 6 = Declarative/Procedural,
22
23 7 = Processability, 8 = Interactionist, 9 = Sociocultural, 10 = Complexity, 11 =
24
25 Implicit/Explicit, 12 = Working Memory, 13 = Neurolinguistic Processing, 14 =
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27 Connectionist, 15 = Neurocognitive Disorder)
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