Intuitions about the Reference of Proper Names: A Meta-Analysis

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Abstract

The finding that intuitions about the reference of proper names vary cross-culturally (Machery et al., 2004) was one of the early milestones in experimental philosophy. Many follow-up studies investigated the scope and magnitude of such cross-cultural effects, but our paper provides the first systematic meta-analysis of studies replicating Machery et al. 2004. In the light of our results, we assess the existence and significance of cross-cultural effects for intuitions about the reference of proper names.

Supplements: All material, including data and analysis code, is available on the OSF project page: <https://osf.io/et86f/>.

Keywords— semantic intuitions, theory of reference, proper names, cross-cultural psychology, meta-analysis, experimental philosophy

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1 Introduction

Most people who have heard of the Italian mathematician Giuseppe Peano credit him with inventing the standard axioms of arithmetic, and this is all they associate with the name. Yet, this is false: the axioms were invented in 1888 by the German mathematician Richard Dedekind and Peano published a simplified version one year later. If people identify Peano only by the description “the inventor of the standard axioms of arithmetic”, to whom are they referring when they use the name “Peano”? To Peano or to Dedekind? More generally, what kind of meaning must people associate to a proper name like “Peano” in order to be competent users of that name?

In philosophy of language, there are two main classes of theories about the meaning of proper names: descriptivist theories and causal-historical theories. According to descriptivist theories (Frege, 1892; Russell, 1905; Searle, 1958), proper names have definite descriptions as their meaning. The idea is that a proper name can refer to a person only via the *descriptive properties that users of the name associate with it*. Thus, people who identify Peano only by the description “the inventor of the standard axioms of arithmetic” would actually refer to Dedekind when they use the name “Peano”. After all, it is Dedekind who uniquely satisfies that description.

According to causal-historical theories (Kripke, 1980), proper names do not imply any descriptive property of the individuals to which they refer. Proper names would refer directly to their bearers without being essentially associated with any descriptive properties of an individual. People processing a proper name like “Peano” will certainly rely on some mental representations of certain descriptive properties, but these representations play no role in determining the meaning of “Peano”. Instead, what is crucial for determining the meaning of a proper name is its *causal history*. All uses of the name that causally derive from an original act of baptism refer to the individual originally baptized with that name. Even if people falsely associate the description “the inventor of the standard axioms of arithmetic” with Peano, the proper name “Peano” actually refers to Peano, because of the existence of a relevant causal chain between the original act of baptism for Giuseppe Peano, and people’s usage of the name “Peano”.

One influential argument for why proper names cannot be semantically equivalent to definite descriptions is that the referent of a proper name ‘is stipulated to be a single object whether we are speaking of the actual world or a counterfactual situation (Kripke 1980, p. 21). In contrast, definite descriptions can refer to different individuals in counterfactual situations.

Kripke illustrates this argument with an example analogous to the Peano case above (Kripke 1980, pp. 83 ff.). Suppose that the only description you associate with the name “Gödel” is “the mathematician who proved the incompleteness of arithmetic”. Suppose that you find out that a certain Mr Schmidt rather than Gödel actually proved the incompleteness of arithmetic. If the name “Gödel” is semantically equivalent to the

definite description “the mathematician who proved the incompleteness of arithmetic” then you are committed to the conclusion that the name “Gödel” actually refers to Mr Schmidt. But, of course, this conclusion is false, which suggests that descriptivist theories of proper names are false.

In a landmark study, Machery et al. (2004) used exactly this Gödel case, and similar vignettes with a binary response variable (the “Jonah case”), to explore the question of whether Kripke’s and other Anglophone speakers’ judgments about the referents of proper names are cross-culturally robust. Machery et al. found that Westerners tend to have intuitions in line with Kripke’s causal-historical theory, while East Asians’ intuitions tend to agree with the descriptivist theory. On the basis of this finding, Machery et al. reasoned that if people’s judgements about the meaning of a proper name are systematically influenced by irrelevant demographic variables like culture, then Kripke’s and other Anglophone speakers’ semantic judgements cannot provide us with reliable evidence in support of general theories of the meaning of proper names.

Several follow-up studies have extended and probed the original result and embedded it into a broader methodological discourse about the use of intuitions in particular cases to test and revise philosophical theories (e.g., Machery et al., 2009; Lam, 2010; Sytsma and Livengood, 2011; Machery, 2017; Cova et al., 2018). Machery et al.’s effect has been found to show significant variation both within and across studies, but no convincing explanation has been offered for when and why we should expect cross-cultural variation in semantic intuitions. It might be that scenarios like the Peano or Gödel cases above are not the right tools for eliciting people’s semantic intuitions. Or, it might be that for making semantic judgements people have access to multiple cognitive strategies, which include both causal-historical and descriptivist factors, and shift between them depending on one’s ease to take the perspective of the speaker, background knowledge, audience, and purpose of communication (Genone and Lombrozo, 2012). For all these reasons, a meta-analysis is needed to summarize the research on the original effect, as well as for indicating directions for further research on cross-cultural variation in the use of proper names.

Proper names are in fact multifaceted and have also been approached by several other disciplines, including linguistics, psychology, neuroscience, and anthropology. Research in linguistics agrees that proper names are mainly used to identify individuals uniquely, but also to activate frames of knowledge associated with certain proper names (Marmaridou 1989). These referential and connotative uses of proper names would depend on the communicative intentions the speaker want to convey to the hearer, the kind of frame of knowledge the interlocutors share when using the name, and on the specific viewpoint or perspective prompted by grammatical, lexical and constructional choices within a discourse (Dancygier, 2009; Dancygier and Vandelanotte, 2017; Marmaridou, 2000; Sophia and Marmaridou, 1989).

Cognitive psychology has been focusing on the difference between the informa-

tion processing of proper names and common nouns. Much of this research provides evidence that the two are processed differently. For instance, people from different cultures can decide more quickly if a word is a proper name as opposed to a common noun (Müller, 2010) and retrieval of meaning from memory has been found to be more demanding for proper names than common nouns (Proverbio et al., 2009; Wang et al., 2016). Explanations of this pattern of differences appeal to the idea that proper names are purely arbitrary referential expressions, whereas multiple semantic connections would be active during the processing of a noun's meaning (Valentine et al., 1996). Additional evidence for the processing difference between proper names and common nouns comes from neuropsychological studies, which have shown a double dissociation between retrieval of proper names and retrieval of common nouns. Across linguistic communities, some neurological patients can competently retrieve the meaning of common nouns from memory, but are unable to associate proper names to pictures of famous people; other patients cannot name any object, but can name familiar or famous people (Yen, 2006; Semenza, 2006).

For anthropologists, the meaning of proper names depends on both general and local principles governing naming practices across cultures (Vom Bruck and Bodenhorn, 2006). Proper names constitute a universal language class (Bright, 2003), and across naming systems proper names have been found to play differentiation and categorization functions: they identify their bearers; differentiating them from other individuals; and they function as devices for social classification in terms of parental, economic, ethnic or geographical group. Focusing on these two functions in sixty cultures around the world, Alford (1988) noticed that the functions of proper names could enter into conflict, since the more a proper name differentiates its bearer from other individuals, the less social information it carries. The specific level of categorization of a proper name would depend on factors such as the size of a social community, the presence of naming taboos, and the level of social mobility (Alford, 1988).

Now, we find noteworthy that the body of empirical research produced in philosophy has not been related to these anthropological studies of proper names, or, more surprisingly, to relevant results from linguistics, cognitive psychology and neuroscience. Linguists and cognitive psychologists have flagged the influence of the classic philosophical theories on psychological issues (Sophia and Marmaridou, 1989; Valentine et al., 1996, p.12, p.19). Specifically, in cognitive psychology, researchers have debated whether a proper name is linked to the individual or whether biographical features mediate between names and individuals. The focus of cognitive psychologists is more on mental representations than semantics. Nonetheless, as their debates resonate with the descriptivist and causal historical views, we believe that the relatively recent experimental work in philosophy can fruitfully inform them.

This meta-analysis aims to provide a basis of literature that could be useful for researchers addressing those questions, bridging the gap between philosophy of language,

linguistics, psychology, and anthropology. Establishing whether judgements about the meaning of proper names vary cross-culturally between communities in East Asia and in Western countries (in a way that fits descriptivist and causal-historical theories respectively) impacts on psychology and anthropology. For psychology, the cross-cultural variation would constitute a challenge, since their research characterizes the processing of a person name as relatively independent from the descriptive resources that competent speakers associate with the name. For anthropologists, the cross-cultural variation would provide a general benchmark for studying the interplay of the categorisation and differentiation functions of proper names across cultures.

Central to our analysis are the questions of whether Westerners' and East Asians' intuitions about the reference of person names are in line with either the causal-historical or the descriptivist theory (Hypotheses 1a and 2a), and how the two populations compare in this respect (Hypothesis 3a).¹ We evaluate these hypotheses on the basis of a set of empirical studies, which rely on probes similar to the Gödel and Peano cases we have described above. To answer these questions, we test three hypotheses:

Hypothesis 1a The majority of Westerners have causal-historical intuitions.

Hypothesis 2a The majority of East Asians have descriptivist intuitions.

Hypothesis 3a Westerners are more likely than East Asians to have causal-historical intuitions.

There are two types of vignettes in the literature we aggregate: first, the *Gödel probe*, which is structurally identical to our introductory Peano/Dedekind example, and the *Jonah probe*, where the properties associated with a proper name gradually shift over time. In the Gödel probe both the causal-historical and the descriptivist theory single out a specific referent of the proper name, while in the Jonah probe the proper name does not refer at all according to the descriptivist account. Since both probes follow the same experimental design (exposure to vignette, binary response, two theories with different predictions, etc.), the results can be aggregated to test hypotheses 1a–3a. Moreover, since the Gödel probe has particular significance in the philosophical literature (going back to Kripke, 1980), and since the original study found a notable effect of cross-cultural variation only for Gödel probes, but not for Jonah probes (Machery et al., 2004), we also test the following three hypotheses:

Hypothesis 1b The majority of Westerners have causal-historical intuitions in the Gödel probes.

Hypothesis 2b The majority of East Asians have descriptivist intuitions in Gödel probes.

¹Compare the following quote from the original paper (Machery et al., 2004, p.B5): “W[esterners] would be more likely to respond in accordance with causal-historical accounts of reference, while E[ast] A[sian]s would be more likely to respond in accordance with descriptivist accounts of reference.”

Hypothesis 3b Westerners are more likely than East Asians to have causal-historical intuitions in Gödel probes.

It is worth noting that some studies differ in the samples of Westerners and East Asians. Machery et al. (2004) tested participants in the United States and Hong Kong. Follow-up studies use for example Dutch participants (Cova et al., 2018) or French participants (Machery et al., 2009). We note these differences in detail in Section 3.1. Nonetheless, we formulated the hypotheses in a general form as a comparison between Westerners and East Asians for two reasons. First, we intended to study the effect as reported in Machery et al. (2004) and hence we followed their design. Second, we did not have theoretical reasons to exclude e.g., Dutch or French from the group of Westerners or e.g., Japanese from the group of East Asians.

Testing these six hypotheses identifies the extent to which intuitions about the reference of proper names vary with cultural context, and it informs future research on the degree of reliability of semantic intuitions.

2 Material and Methods

2.1 Data sources and searches

We conducted a comprehensive literature search of studies on cross-cultural semantic intuitions. To find replications of the original experiment, we started with the Google Scholar list of all papers citing Machery et al. 2004 and checked whether they contained experimental data. Our search was aided by a list of known replications that we obtained from Prof. E. Machery via e-mail. We used this search strategy because any replication of Machery et al. 2004 would, in virtue of its aims and scope, refer to the original paper.

2.2 Study selection

Studies were eligible if they were published or publicly available in English, and used a design sufficiently similar to the original study. Specifically, eligible studies had to contain results from experiments featuring East Asian and/or Western participants with one or more binary-choice probe. The binary-choice answer to such probes had to correspond to the descriptivist and the causal-historical theory of reference, respectively.

2.3 Data extraction

The eligible studies were classified by two teams, each of which included two authors of this paper. For each study, team members independently extracted the name of the first author, the year of publication, and the data of the probe responses. Per study and probe, they independently extracted data on the type of the probe (e.g., Gödel, Jonah, etc.), the number of Western and East Asian participants, total sample size, the number

of causal-historical responses (per subgroup and in total), and deviations from the original design, such as language of the probe, phrasing of the question, and phrasing of the answers. Disagreements were resolved by consensus. ²

2.4 Data synthesis and analysis

We carried out a confirmatory (Section 3.2) and an exploratory analysis (Section 3.3), as well as a quality appraisal (Section 3.4 and Appendix C). A confirmatory analysis assessed the overall meta-analytic evidence and its generalisability for the main hypotheses tested in Machery et al. 2004 (Hypotheses 1a–3a) and the meta-analytic evidence for the particular Gödel probes (Hypotheses 1b–3b) for which Machery et al. 2004 reported positive results. Specifically, for Hypotheses 1b–3b, only Gödel probes were used, which did not deviate from the design of the original study (i.e., direct replications). The analyses were conducted on the level of the individual probes within a study, because not all studies had the same (number of) probes. We used a multilevel random effects (RE) model to capture the hierarchical structure (i.e., probes within studies) of the data and the inter-study dependencies between probes via the ‘metafor’ package in R (Viechtbauer, 2010).

In total, we performed six confirmatory meta-analyses, one per hypothesis. Specifically, we calculated summary Relative Risk ratios (RR)³ for the Causal/Historical responses to probes that compared Westerners to East Asians (Hypotheses 3a and 3b) and summary proportion estimates of Causal/Historical response for single cultural group responses (Hypotheses 1a/b, 2a/b) with a restricted maximum-likelihood heterogeneity estimator⁴ to model the random effects. The extent of heterogeneity between probes was assessed by the I^2 measure (indicating the percentage of total variance due to between-probe variance; Higgins et al., 2003; Ioannidis et al., 2007). In addition to the 95% confidence intervals (CIs) for the unknown parameters (i.e., the proportion of causal-historical responses and the RR between populations), we calculated the 95% prediction intervals (PI). Unlike CIs, which are based on compatibility of the observed data with an unknown parameter value, PIs predict the distribution of future data points by taking into account inter-study variability. Therefore, they are better suited to express a plausible range of values for the next conducted study, and to predict whether effects are likely to replicate (Higgins et al., 2009; Riley et al., 2011).

The exploratory analysis focused on the observed variance in effect-size between

²The full data spreadsheet is available online in the additional materials file.

³Relative Risks ratios are calculated as the quotient of the proportions of an event in two different groups. If A and B are the two groups and p_A and p_B are the proportions of the event E in the groups, then the relative risk ratio is defined as $RR_E(A, B) = p_A / p_B$. Relative Risk is a well-probed measures of association between categorical data that is easy to interpret and using a different effect-size measure like Odds Ratio does not change the inference (Higgins and Green, 2011).

⁴This estimator is approximately unbiased and efficient (Viechtbauer, 2005; Raudenbush, 2009), and is the software package’s default (Viechtbauer, 2010).

the various probes; that is, it aimed to identify factors that would explain why studies often deliver heterogeneous results. Specifically, we repeated the tests performed for Hypotheses 1a–3a with a meta-regression analysis where deviations from the original design were included as moderator variables. We ignored the hierarchical structure of the data because we were interested in explaining variance by the general design factors.⁵

3 Results

3.1 Studies and data

Via Google Scholar, we identified 482 records published in 2017 or earlier that cite Machery et al. 2004. Using the search criteria described in Section 2.1, we ended up with 15 potential studies. Four studies were added in addition to our initial search results, resulting in a total of 19 studies.⁶

Eventually, from this set of 19 studies, 13 were included and 6 were excluded. Reasons for exclusion were the following: [1] missing data, [2] culturally mixed samples, and [3] structure of the data (i.e., data from non-binary questions, compare Section 2.2). See Appendix A for the list of excluded papers and their reason for exclusion. In addition, the Indian sample from Machery 2009 was excluded, because they are considered to be an South-Asian instead of East-Asian. Combined, these studies tested 61 probes on 4691 participants, who produced a total 8959 binary responses. Of these probes, 35 tested both Western and East Asian samples [median sample size: 181]; 15 probes tested Western samples [median sample size: 60]; and 11 probes tested East Asian samples [median sample size: 211]. Table 1 provides an overview of the number of probes per study and their characteristics. Most Western samples were from the United States of America. Exceptions were ‘Machery, 2009’ (France), ‘Cova, 2019’ (The Netherlands), and ‘Colombo, n.p.’ (The Netherlands, USA, England, Germany, and Italy). In general, the East-Asian samples were from Hong Kong. Exceptions were ‘Machery, 2009’ (Mongolia), ‘Sytsman, 2015’ (Japan), ‘Kazaki, 2017’ (Japan), ‘Izumi, 2018’ (Japan), and ‘Colombo, n.p.’ (Hong Kong and China). We identified ten factors on which probes could deviate from the original design (see Appendix B), from the language in which probes were presented to the phrasing of the question that participants were asked.⁷

In particular, the studies in our dataset did not always use the same phrasing for

⁵Taking account of hierarchical structures allows one to assess the associations between level, but ignoring this structure has a negligible effect on the main effect estimates (O’ Mara, 2008).

⁶The first author of the original study, Prof. Machery, suggested to add the original results reported in Machery et al. (2015) and a forthcoming by Izumi et al. (2018). In addition, we included the results of our replication attempt reported in Cova et al. (2018) and an online replication attempt carried out via Qualtrics, whose details are also available online at BLINDED.

⁷The complete list of the design deviation factors and their values is given in the data extraction plan, which is available online in the additional materials file.

eliciting a judgment on causal-historical versus descriptivist intuitions. Most studies followed the original design by Machery et al. (2004) and asked participants the following question: Who is the person that the vignette character John is “talking about” when using the proper name “Peano”? By contrast, Sytsma and Livengood (2011) suggest rephrasing the question in the following way: Who would you *take* is the person that John is talking about when using the proper name “Peano”? (this modification is known as the “clarified narrator’s perspective”, see also Section 3.3), and Machery et al. (2009) asked for the *truth value* of sentences such as “Peano discovered the incompleteness of mathematics”. Similarly, answers to the binary question were sometimes phrased as descriptions (e.g., in the original study) and sometimes as bare noun phrases without further explanations (e.g., in Lam, 2010). For the purpose of the meta-analysis, we treated all these dependent variables as answering the same question, independently of the exact phrasing. However, for testing Hypothesis 1b-3b, we focused on direct replications of the original Gödel probe and excluded variations of the design such as Sytsma and Livengood’s “clarified narrator’s perspective”. These different ways of analyzing the data allow us to answer two different questions: first, the question of the replicability and internal validity of the original effect observed by Machery et al., and second, the generality and external validity of the observed effect to a wider hypothesis about the reference of proper names.

3.2 Meta-Analytic Estimates

The results per hypothesis are displayed as forest plots in Figures 1 and 2⁸ and summarized below. The analyses provide confirmatory evidence for Hypotheses 1a, 2b, 3a and 3b in the sense that the confidence interval for the unknown parameter consists only of values in the expected direction, and excludes the hypothesis of zero effect. Forest plots for all six hypotheses are given in Figure 1 and 2. Below is a precise statement of our results.

Hypothesis 1a The summary proportion of Causal-Historical probe responses in Westerners was 0.58 [95% CI: 0.52–0.62] with large observed heterogeneity [$I^2 = 82.10\%$] and a prediction interval that included zero-effect [95% PI: 0.35–0.77].

Hypothesis 1b The summary proportion of Causal/Historical responses to Gödel probes in Westerners (in the original experimental design) was 0.55 [95% CI: 0.50–0.59] with moderate observed heterogeneity [$I^2 = 21.89\%$, 95% PI: 0.35–0.77].

Hypothesis 2a The summary proportion of Causal-Historical probe responses in East

⁸These plots show the distribution of effect-sizes (location, confidence intervals, weight in the meta-analysis) of the probes that were used for the analysis, their summary meta-analytic effect-size (diamond at the bottom), and its confidence interval (width of the diamond). The forest plots for Hypothesis 1a and 2 can be found in the online supplementary files.

Table 1: Overview of the Studies and Their Characteristics.

First author	Year	Study Type	Study Design	Number of probes	Sample Size	Design Deviations
Machery	2004	Comparison	Within-subjects	4 (Gödel, Jonah)	72	No (original study)
Lam	2009	Comparison	Within-subjects	3 (Shakespeare, Julius)	69	Yes (2 deviations)
Machery	2009	Comparison	Between-subjects	2 (Gödel, Shakespeare)	144	Yes (2 deviations)
Machery	2010	Asians only	Single probe	1 (Gödel)	19	Yes (1 deviation)
		Comparison	Single probe	1 (Gödel)	153	Yes (1 deviation)
Sytsma	2011	Westerners only	Within-subjects	3 (Gödel)	35	Yes (1 deviation)
		Westerners only	Between-subjects	3 (Gödel)	223	Yes (1 deviation)
Beebe	2015	Westerners only	Between-subjects	6 (Satyricon)	304	Yes (1 deviation)
		Westerners only	Between-subjects	3 (Satyricon)	180	Yes (2 deviations)
		Comparison	Within-subjects	4 (Satyricon)	177	Yes (1 deviation)
Machery	2015	Asians only	Single probe	1 (Gödel)	65	Yes (2 deviations)
		Comparison	Within-subjects	2 (Gödel)	129	Yes (2 deviations)
Sytsma	2015	Comparison	Between-subjects	3 (Gödel)	621	Yes (2 deviations)
Beebe	2016	Comparison	Within-subjects	4 (Gödel, Jonah)	207	Yes (1 deviation)
	2016	Comparison	Within-subjects	4 (Gödel, Jonah)	406	Yes (1 deviation)
Kazaki	2017	Asians only	Within-subjects	2 (Gödel)	231	Yes (3 deviations)
Izumi	2018	Asians only	Between-subjects	7 (Gödel)	1283	Yes (2 deviations)
Cova	2018	Comparison	Within-subjects	4 (Gödel, Jonah)	181	No (only sample deviation)
Colombo	n.p.	Comparison	Within-subjects	4 (Gödel, Jonah)	192	No (only sample deviation)

Figure 1: Forest plot for Hypotheses 3a, showing distribution of effect sizes and confidence intervals.

Figure 2: Forest plot for Hypotheses 1b, 2b and 3b, respectively, showing distribution of effect sizes and confidence intervals.

Asians was 0.50 [95% CI: 0.41–0.59] with large observed heterogeneity [$I^2 = 87.50\%$] and prediction interval [95% PI: 0.19–0.80].

Hypothesis 2b The summary proportion of Causal-Historical to Gödel probes in East Asians (in the original experimental design) was 0.36 [95% CI: 0.32–0.41] with large observed heterogeneity [$I^2 = 52.59\%$], but a prediction interval that excluded the zero-effect value [95% PI: 0.26–0.48].

Hypothesis 3a The summary RR of Causal-Historical probe responses in Westerners versus East Asians was 1.18 [95% CI: 1.09–1.27] with moderate-to-large observed heterogeneity [$I^2 = 47.42\%$] and a prediction interval that included zero-effect [95% PI: 0.96–1.44].

Hypothesis 3b The summary RR of Causal-Historical responses to Gödel probes in Westerners versus East Asians (in the original experimental design) was 1.36 [95% CI: 1.20–1.54] with moderate observed heterogeneity [$I^2 = 30.08\%$] and a prediction interval that excluded the zero-effect value [95% PI: 1.03–1.80].

In addition, we wanted to make sure that these results did not depend on how we modelled the structure of the data. Most studies included in the meta-analysis contain several probes. Some studies used one sample of participants to test several probes (i.e., repeated measure design), while other studies used separate samples of participants to test each probe individually (i.e., between-subject design). Our analyses take into account the hierarchical structure of probes nested in studies, but they neglect the potential dependencies between probes in studies with repeated measures design.

To take account of these differences, we ran two additional sets of analyses. For the first analyses, we used a flat data structure (i.e., a regular RE meta-analysis). For the second analyses, we also used a regular RE meta-analysis, but included the average effect size and standard error of probe clusters with shared repeated measure design. For instance, if a study contained one sample of (Western) participants (and/or one sample of Asian participants) that responded to four probes (e.g., the two Gödel and two Jonah probes of the original study by Machery et al. 2004), then only the average of their response was included in the analyses. The results of these analyses were similar to those of the analyses with the hierarchical data structure, thereby warranting the same conclusion (code and results of the these analyses can be found in the additional materials file).

3.3 Results of exploratory analyses

The width of the confidence and prediction intervals in the hypotheses tests (see Section 3.2) point to high observed inter-study variance of participants' responses. By means of a meta-analytic regression, we explored whether this variance could be explained by

specific deviations in study design, such as language of the participants (see Appendix B for a list of these deviations and their descriptions). Concretely, we fitted three generalized meta-analytic regressions with the Knapp and Hartung method⁹ (Knapp and Hartung, 2003; Viechtbauer, 2010) for the data used to test hypotheses 1a, 2a, and 3a with the identified design deviation factors as predictors and log-transformed RR (hypothesis 3a data) or Odds Ratio (hypotheses 1a and 2a data) as outcome. We fixed the sampling variance to zero, because we were only considering variance between probe outcomes. This choice is acceptable, since we are not estimating an overall effect-size weighted by the precision of each datum in the analysis. However, we did include the sample size in the model as a predictor.

For all three analyses, much of the variance was explained by the model¹⁰. About 47% of the variance in the probe responses by Westerners could be accounted for by deviations in study design [$R^2 = 0.47$, $F(18,30) = 3.34$, $p = 0.002$]. For probe responses by East Asians, about 40% of the variance could be accounted for [$R^2 = 0.40$, $F(23,23) = 2.338$, $p = 0.02$]. In the comparison in probe responses between Westerners and East Asians, about 28% of the variance could be accounted for by deviations in study design [$R^2 = 0.28$, $F(18,15) = 1.70$, $p = 0.15$]¹¹. Although the relatively large number of predictors in relation to the size of the dataset made the results unsuited for drawing strong conclusions with certainty, a noteworthy result was that none of the design deviations, which include participant nationality and different types of probes, made a statistically significant contribution ($p < 0.05$) to all of the models.

In addition, we separately analyzed studies that used an alternative formulation of the binary-response question in the Gödel probes: the “clarified narrator’s perspective” (e.g., Sytsma and Livengood, 2011). Based on existing literature, this formulation was expected to be less ambiguous in eliciting participants’ semantic intuitions. We found six such probes in our data set, of which three compared the causal-historical and descriptivist responses between Westerners and East Asians; one tested East Asians only; and two tested Westerners only. To test the summary effect-size of these data, the same kind of analyses were used as for testing H1b-H3b (see Section 2.4). The results are summarized in Table 2. The proportion of causal-historical intuitions is higher for both Westerners and East Asians in the clarified narrator’s perspective than in the set of all Gödel probes. In particular, for Westerners, the effect—as measured by the proportion of causal-historical responses—is more pronounced in the “clarified narrator’s perspective” while it is slightly smaller for East Asians and the comparison of East Asians and

⁹This method was used because with this method the individual coefficients are based on the t-distribution and the omnibus test statistic uses an F-distribution. In other words, this method allows for interpretation of the result similar to a regular linear regression.

¹⁰Because of the fixed sampling-variance, the analysis is identical to a generalized linear model with R^2 equal to adjusted- R^2 .

¹¹The models do not have the same number of predictors, because of group specific variables (e.g., language of participant group) and redundant dummy predictors were left out.

Westerners. These comparisons are relative to the outcomes of the tests of H1b–H3b in Section 3.2.

Table 2: Results of the analysis for probes with the clarified narrator’s perspective proposed by Sytsma and Livengood (2011).

Analysis	Effect Type	Effect Size	95% CI	<i>P</i> value	<i>I</i> ²	95% PI
Westerners	proportion	0.66	0.60–0.72	$p < 0.0001$	3.27%	0.56–0.75
East Asians	proportion	0.39	0.28–0.52	$p < 0.10$	68.10%	0.20–0.62
Comparison	RR	1.31	1.17–1.47	$p < 0.0001$	0.0%	1.17–1.47

3.4 Evaluation of the study’s results and methods

Evaluation of the results of the included papers indicate absence of small-study effects and presence of evidential value, but high inter-study variance. The *p*-curves are right-skewed (see Figures 3 and 4), which indicates that the set of studies contains evidential value (Simonsohn et al., 2014).¹² Figure 5 shows the funnel plots (Sterne et al., 2011) for Hypothesis 1a–3a, and Figure 6 shows the corresponding funnel plots for Hypothesis 1b–3b. Small to large studies show symmetrical distributions around the summary effect-sizes (see Figure 5 and 6), but for Hypothesis 1a and 2a, inter-study variance is high even among studies with high precision (i.e., large sample size, low standard error). This indicates that inter-study variance does not diminish with increasing precision as it should. No such effect is observed when cross-cultural variations are directly studied, that is, in Hypothesis 3a/b. For a quality appraisal of the methods employed in the experiments, see Appendix C.

4 Discussion

All in all, our meta-analysis supports the hypothesis that cross-cultural factors affect semantic intuitions about proper names (Machery et al., 2004). For four out of the six tested hypotheses, the meta-analytic confidence interval for the summary effect size does not include the zero effect value. Neither do specific analysis tools aimed at detecting publication bias or QRPs (e.g., funnel plots, *p*-curves) provide evidence of systematic suppression of negative results. The wide prediction intervals point to high inter-study variability of the data, which cannot be consistently explained by the studies’ differences in experimental design.

Our study cannot test the overall scope of cross-cultural factors in eliciting intuitions about the reference of proper names, since the meta-analysis is restricted to a very specific set of probes. In addition, some aspects of the meta-analysis are limited by the

¹²It should be noted that the reliability of *p*-curves is negatively related with the heterogeneity between the included results, which is quite large in the case of our meta-analyses

Note: The observed p -curve includes 17 statistically significant ($p < .05$) results, of which 15 are $p < .025$. There were 35 additional results entered but excluded from p -curve because they were $p > .05$.

Note: The observed p -curve includes 21 statistically significant ($p < .05$) results, of which 15 are $p < .025$. There were 26 additional results entered but excluded from p -curve because they were $p > .05$.

Note: The observed p -curve includes 16 statistically significant ($p < .05$) results, of which 11 are $p < .025$. There were 19 additional results entered but excluded from p -curve because they were $p > .05$.

Figure 3: P-Curve plots for Hypotheses 1a, 2a and 3a, respectively.

Note: The observed p -curve includes 12 statistically significant ($p < .05$) results, of which 10 are $p < .025$. There were 18 additional results entered but excluded from p -curve because they were $p > .05$.

Note: The observed p -curve includes 19 statistically significant ($p < .05$) results, of which 15 are $p < .025$. There were 14 additional results entered but excluded from p -curve because they were $p > .05$.

Note: The observed p -curve includes 12 statistically significant ($p < .05$) results, of which 9 are $p < .025$. There were 10 additional results entered but excluded from p -curve because they were $p > .05$.

Figure 4: P-Curve plots for Hypotheses 1b, 2b and 3b, respectively.

Figure 5: Funnel plots for Hypotheses 1a (upper left figure), 2a (upper right figure) and 3a (bottom figure). x-axis: observed effect size; y-axis: precision of study as measured by standard error.

Figure 6: Funnel plots for Hypotheses 1b (upper left figure), 2b (upper right figure) and 3b (bottom figure). x-axis: observed effect size; y-axis: precision of study as measured by standard error.

methodological design of the studies and the small number of studies with respect to the differences in design between the studies. The use of a few vignettes with binary responses per study with unknown dependencies forced us to analyse the data on the level of individual probes (vignettes) and may have contributed to the large heterogeneity. However, three findings of general interest should be noted.

First, there is a notable difference between the confidence intervals (CIs) for the unknown parameter and the prediction intervals (PIs) for the next observation: only two of the six PIs do not contain zero effect. The clearest difference between the CIs and the PIs is perhaps visible in the meta-analysis of Hypothesis 1a, 1b and 2a: the 95% PIs are too wide to make a theoretically meaningful prediction. The reason is that PIs take into account how the variability in the data transfers to the expected value of future data points. In a case where individual studies scatter over a wide range of points, like in the case of Hypothesis 1a, 1b, and 2a, the PIs will be considerably wider than the CIs. The same remarks apply, though with a less pronounced effect, to the other three hypotheses. For these reasons, it is not surprising that a recent replication attempt, included in Cova et al. (2018), has not reproduced the original effect: although there is, on average, a cross-cultural effect in the predicted direction, the inter-study variance is too high to reliably predict a result in the vicinity of the meta-analytic mean. The funnel plots of Figure 5 and 6 confirm this diagnosis: since variation in the observed proportions of causal-historical responses is extremely high, there seems to be a lot of “noise” in the data.

Second, the clarified narrator’s perspective, suggested by Sytsma and Livengood (2011), seems to push causal-historical intuitions: it increases the tendency of participants to give causal-historical responses across the board. At the same time, it reduces the cross-cultural difference between Westerners and East Asians. However, due to the small sample size for this type of probe, our finding here should be treated with caution.

Third and last, our meta-analytic regression found statistically significant dependencies of the results on variations in experimental design. However, we could not identify predictor variables that uniformly explained inter-study variance for Westerners, East Asians and the comparison of both populations. Moreover, there are interesting dependencies on the particular probes. For example, for the population of East Asians, the tendency to give descriptivist answers is almost exclusively driven by Gödel-type probes (meta-analytic estimate of 64% as opposed to 50% overall). This shows a high sensitivity to the particular instrument for eliciting semantic intuitions, without presence of a theoretically convincing explanation (the effect is absent for Westerners). More generally, it is possible that the large amount of heterogeneity and lack of uniform explanation is (partly) due to methodological deficiencies (see Appendix C). We therefore suggest that future research on cross-cultural differences in semantic intuitions extends the range of probes in experiments, in order to avoid that substantial philosophical, psychological or anthropological conclusions depend (too) much on the specifics of a particular probe

(see also Devitt and Porot, 2018).

Given these qualifications, the take-home message of our meta-analysis can be summed up as follows: there is cross-cultural variation in intuitions about the reference of person names, but there is a high unexplained inter-study variance, compromising predictive validity; and it is not easy to disentangle this cross-cultural effect from the (random) effect of a particular study.

In relation to existing findings from other disciplines, our results are consistent with anthropologists' and linguists' work showing that there are many features of naming practices that differ across cultures and contexts of discourse (Vom Bruck and Bodenhorn, 2006). However, underlying these differences, the primary pragmatic functions of person names of referring directly to a unique individual and also marking social connections, may be stable (Alford, 1988; Sophia and Marmaridou, 1989). Also stable may be the kinds of cognitive processes involved in understanding person names, which probably recruit a number of structured representations (or frames) associated with the name (Valentine et al., 1996; Dancygier, 2009). Given the high degree of variation, both within and across experimental groups, that our meta-analysis uncovered, it is plausible that the workings of these kinds of processes are modulated by both causal and descriptive factors in different linguistic and social contexts (Genone and Lombrozo, 2012).

Due to the dependency on particular probes, we recommend that researchers interested in understanding the mechanisms underlying the processing of person names move beyond the contrast between descriptivists and causal-historical theories, and use a broader and more diverse set of vignettes in different languages and contexts. For those experimental philosophers who may be interested only in questions about philosophical methodology, our results confirm that philosophical scenarios are less reliable instruments for eliciting semantic intuitions than current philosophical practice seems to presume.

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A List of Excluded Studies

Table 3 lists all studies in our original pool that were excluded from our meta-analysis. For each study, we specify why it did not meet the inclusion criteria. Since meta-analyses are based on effect-sizes of the results of all included studies and their standard error, the same kind of effect-size must be calculable for each study in a meta-analysis. The original study used two groups, one of Westerners and one of East Asians, and binary-outcomes for each probe. Hence, the relevant kind of effect-size is the Relative Risk or Odds Ratio. Based on this kind of effect-size, we excluded studies that had outcomes with more than two values and studies or samples that did not fall in the East-Asian or Western group.

Table 3: List of excluded study on semantic intuitions.

APA reference	Exclusion reasons
Nichols, S., Pinillos, N. ., & Mallon, R. (2016). Ambiguous reference. <i>Mind</i> , 125(497), 145-175.	The design did not match. The studies in this paper did not use binary-response options for their vignettes where one was the causal-historical option and the other the descriptivist option.
Domaneschi, F., Vignolo, M., & Di Paola, S. (2017). Testing the causal theory of reference. <i>Cognition</i> , 161, 1-9.	The design did not match. The studies in this paper did not use binary-response options for their vignettes where one was the causal-historical option and the other the descriptivist option.
Genone, J., & Lombrozo, T. (2012). Concept possession, experimental semantics, and hybrid theories of reference. <i>Philosophical Psychology</i> , 25(5), 717-742.	The design did not match. The studies in this paper did not use binary-response options for their vignettes where one was the causal-historical option and the other the descriptivist option.
Grau, C., & Pury, C. L. (2014). Attitudes towards reference and replaceability. <i>Review of Philosophy and Psychology</i> , 5(2), 155-168.	The ethnicity/cultural background of the participants was mixed. The data could not be separated into Western and East Asian samples.
Islam, F. (2017) <i>Logical semantics in a cross-cultural perspective</i> . Masters thesis in English Linguistics and Language Acquisition. Supervisor: Giosu Baggio. NTNU - Trondheim, May 2017	The author did not want to share data. The Asian participants were not East-Asians, but South-Asians.
Machery, E. (2012). Expertise and intuitions about reference. <i>THEORIA. Revista de Teora, Historia y Fundamentos de la Ciencia</i> , 27(1), 37-54.	The ethnicity/cultural background of the participants was mixed. The data could not be separated into Western and East Asian samples.

B List of experimental design deviations

Table 4 below provides an overview of experiment design deviations from Machery et al. 2004. We focused on several design factors. For each factor, we identified deviations from the original design, based on the information reported in the method sections of all studies in our dataset.

Table 4: Overview of experimental design deviation and where they are present.

Factor	Values	Description	Studies that contain this deviation
Probe	Godel1, Godel2, Jonah1, Jonah2, Shakespeare1, Julius1, Julius2, Satyricon1, Satyricon2, Satyricon3	Different vignettes were used in the studies. The original study used the Gödel and Jonah vignettes. The Shakespeare, Julius, and Satyricon vignettes are about different characters and situations, though still concern reference of proper names.	Lam, 2009 (Shakespeare1, Julius1, Julius2); Machery, 2010 (Shakespeare1); Beebe, 2015 (Satyricon1, Satyricon2, Satyricon3)
Language probe Asia	English, Chinese, Japanese	In the original study, the probes were in English for the East-Asian sample. Some studies deviated by presenting the probes in Chinese or Japanese.	Lam, 2009 (Chinese); Machery, 2010 (Chinese); Machery, 2015 (Chinese); Sytsma, 2015 (Japanese); Kazaki, 2017 (Japanese); Izumi, 2018 (Japanese)
Continued			

Factor	Values	Description	Studies that contain this deviation
Language participant West	English, French, Dutch, Mixed	In the original study, the main language of the Western sample was English (USA based). Some studies deviated by using Westerners with a different place or origin, residence, and main language.	Machery, 2009 (French); Cova, 2018 (Dutch); Colombo, n.p. (Mixed)
Language participant asia	Chinese, Mongolian, Japanese, Mixed	In the original study, the (main) language of the East-Asian sample was Chinese (Hong Kong based). Some studies deviated by using East-Asians with a different place or origin, residence, and main language.	Machery, 2009 (Mongolian); Sytsma, 2015 (Japanese); Kazaki, 2017 (Japanese); Izumi, 2018 (Japanese); Colombo, n.p. (Mixed)
Moral valence	0=original, 1=good, 2=bad	The moral valence of the vignette was altered. E.g., in the original version Gdel stole Schmidts incompleteness theorem, but in the altered version he took credit to assist Smith.	Beebe, 2015
Continued			

Factor	Values	Description	Studies that contain this deviation
Perspective probe answer	0=original, 1=protagonists perspective, 2=narrators perspective, 3=clarified narrators perspective	The phrasing of the questions and the binary-response options are altered. Specification is added to guide the participant in taking the perspective of the one narrating the vignette (2, 3). The protagonist perspective (1) is not supposed to test semantic intuitions. These probes were excluded from the analyses.	Sytsma, 2011; Beebe, 2015 (only 3, clarified narrators perspective); Machery, 2015 (only 3, clarified narrators perspective); Sytsma, 2015; Beebe, 2016 (only 3, clarified narrators perspective)
Anchoring	0=unaltered, 1=changed anchoring in the probe.	The vignette is altered in its anchoring. Specifically, in the original the protagonists learns that, for instance Gdel proved the incompleteness theorem, but further in the vignette this is said to be incorrect. In the vignettes with changed anchoring the vignette is rearranged in such a way that it starts with that Schmidt proved the incompleteness theorem.	Beebe, 2016
Continued			

Factor	Values	Description	Studies that contain this deviation
Answer phrasing	0=original, with bare noun phrases, 1=without bare noun phrases, 2=added sono-demonstrative, 3=added sono-demonstrative and anaphoric predicate, 4=added clarifying predicate.	The answers phrasing is changed in particular ways for the purpose of decreasing ambiguities. Values 1, 2, and 3 concern particular in Japanese translations responses. Value 4 concerns added predicate(s) that are supposed to add clarity.	Machery, 2009; Kazaki, 2017; Izumi, 2018
Question target	0=reference (talking about), 1=truth values (sentence true?)	The type of binary-response options that were used. The original used a reference style where the protagonist of the vignette is talking about the one bearing the name (causal-historical option) or the one with the descriptive property (descriptivist option). The other type is a true-or-false question, where one is the causal-historical option and the other the descriptivist option.	Machery, 2009
Continued			

Factor	Values	Description	Studies that contain this deviation
Asian sample	0=students from the University of Hong Kong, 1=other populations	This variable was added later, because we realized that it might be possible that the students from the University of Hong Kong might be a highly specialized group that might respond differently than those from the other university in Hong Kong, mainland China, Japan, or other parts of East-Asia.	Lam, 2009; Machery, 2009; Machery, 2010; Machery, 2015; Sytsma, 2015; Beebe, 2016; Kazaki, 2017; Cova, 2018; Izumi, 2018; Colombo, n.p.

C Detailed Quality Appraisal

In addition to the evaluation of the main results of the meta-analysis (see Section 3.4), we assessed the quality of the methods and measurements techniques of the included studies. Quality appraisal is a standard feature in systematic reviews (e.g., exhaustive meta-analysis of the literature on a particular topic or field of study; Petticrew and Roberts, 2008). In general, results of a methodological quality appraisal are used for deciding on study inclusion/exclusion (e.g., studies below a certain quality threshold are excluded), or as predictors for assessing the robustness of the results. Since no standards of quality appraisal have yet been formulated and tested for experimental philosophy research, we moved the quality appraisal to this appendix where we indicated presence or absence of methodological features of the studies without interpreting these results.

For the quality appraisal of the studies' methods, we used parts of a validated appraisal checklist for clinical non-RCT comparative studies (Khan et al., 2001; Deeks et al., 2003). In addition, we added three items concerning generalizability from the sample to the population suggested in Petticrew and Roberts 2008.

The checklist consisted of nine particular qualities, on which each study could be scored 'yes' if the information provided in the paper describing the study indicated it possessed this quality; 'no' if there was clear indication in the paper that the study did not possess this quality; 'unknown' if insufficient information was given in the paper about this quality; or 'NA' if the quality was not applicable to the study (e.g., group matching does not apply to a study that tests only a single group). These nine items were:

1. *Sample description*: adequacy and completeness of description of the relevant participants' characteristics. We restricted ourselves to age (mean and standard deviation) and sex (percentage of males/females) because the original study reported these characteristics and the literature does not identify other relevant characteristics.
2. *Matched groups*: similarity of participant groups in terms of characteristics that may affect the outcome (including socio-economic characteristics). In this case, the Western and East Asian participants would have to be matched on age and sex ratio.
3. *Equal group treatment*: identical treatment of participant groups. In this case, Westerners and East Asians should be tested under the same experimental conditions.
4. *Experimental description*: adequate and unambiguous description of the experiment. In this case, the studies should contain information on what kind of design was used (within-subject or between-subject), how participants were sampled, and how the participants were tested.

5. *Adequate measure*: a measurement or test is adequate when its validity and reliability are reported. This can be ensured by reference to previous measurement assessments and/or argumentation supporting its validity and reliability.
6. *Relevant measure*: a measurement or test is relevant when it is considered appropriate for testing (answering) the reported hypothesis (research question). In this case, an explanation should be given why the probe and the binary question measurement are suitable for measuring intuitions about the reference of proper names (in comparison to other potential tests or measurements).
7. *No experimenter bias*: Experimenter bias is considered absent when the report indicates that the person(s) administering the test could not have influenced the outcome. In our case, we consider absence of experimenter bias when the administrator(s) was (were) not in the same room as the participants during the experiment.
8. *Random sampling*: a sample is considered a random sample (of the population) when all members of the population had an equal probability of participating. This is a very demanding requirement. However, it should be noted that it is an assumption of all inferential statistical tests. In our case, at least clear non-random sampling practices should be absent (e.g., convenience sampling).
9. *Representative samples*: a sample is considered representative when there are clear indications that its composition/distribution of the relevant characteristics is similar to that of the population. In our case, the samples of Western and East Asian participants should be demographically similar to the respective populations.

The methodological quality assessment was performed by two of the authors [NvD and MC]. Two of us independently scored the included studies using the quality appraisal checklist. The results of the assessment are reported in Figure 5 and 6. None of the studies scored affirmatively on all of the methodological qualities, and in many cases there is not enough reported on the measurement and procedure of the studies to adequately infer absence or presence of a quality.¹³

Due to the low number of high-scoring studies, it was not possible to perform a sensitivity analysis where we compare the effect-size and heterogeneity of high-scoring vs. low-scoring studies. However, it was possible to run regression analyses with the qualities as predictors. Five of the nine qualities could not be included, because none scored affirmatively. The four predictors that could be included in the analyses explained between 14.0% and 34.30% of the variance between the probes (for code and results: see the additional materials file).

¹³The original statistical analysis by Machery et al. (2004) is flawed on several levels, most egregiously in the aggregation of binary scores across probes and the use of the *t*-test for comparing the sample means across populations.

In an effort to improve the replicability, and cross-disciplinary comparability of this line of research on semantic intuitions, we encourage researchers to consider quality assessment schemes from other disciplines (e.g., in medicine), in addition to other checks on research quality, like high-powered replication studies (e.g., Cova et al., 2018), checking for statistical reporting errors (e.g., Colombo et al., 2018), and meta-science tools for the uncovering publication bias and questionable research practices (e.g., Stuart et al., 2018).

Table 5: Assessment of methods and measurement qualities 1 through 4.

First author	Year	Study No.	Sample description	Matched groups	Equal group treatment	Experimental description
Machery	2004	1	no	no	yes	yes
Lam	2009	1	no	no	yes	yes
		2	no	no	yes	yes
Machery	2009	1	yes	no	unknown	no
Machery	2010	1	no	unknown	unknown	no
		2	yes	no	NA	yes
Sytsma	2011	1	yes	NA	NA	yes
		2	yes	NA	NA	yes
		2	yes	NA	NA	yes
		3	yes	NA	NA	yes
Beebe	2015	1	yes	no	yes	yes
		2	yes	NA	NA	yes
		3	yes	NA	NA	yes
		4	yes	NA	NA	yes
Machery	2015	1	yes	no	unknown	yes
		2	yes	no	unknown	yes
Machery	2015	3	yes	NA	NA	yes
Sytsma	2015	1	yes	no	no	yes
Beebe	2016	1	yes	no	yes	yes
		2	yes	no	yes	yes
Izumi	2017	1	yes	NA	NA	yes
		2	yes	NA	NA	yes
Kazaki	2017	1	no	NA	NA	yes
Cova	2018	1	yes	no	no	yes
Colombo	n.p.	1	NA	unknown	yes	NA

Table 6: Assessment of methods and measurement qualities 6 through 9.

First Author	Publication Year	Study Number	Adequate Measurement	Relevant Measurement	No experimenter bias	Random Sampling	Representative Sample(s)
Machery	2004	1	no	unknown	unknown	unknown	no
Lam	2009	1	no	unknown	unknown	unknown	no
		2	no	unknown	unknown	unknown	no
Machery	2009	1	no	unknown	unknown	unknown	unknown
Machery	2010	1	no	unknown	unknown	unknown	no
		2	no	unknown	unknown	unknown	no
Sytsma	2011	1	no	unknown	unknown	unknown	no
		2	no	unknown	yes	unknown	unknown
		3	no	unknown	yes	unknown	no
		4	no	unknown	no	no	unknown
Beebe	2015	1	no	unknown	yes	unknown	no
		2	no	unknown	unknown	unknown	no
		3	no	unknown	yes	unknown	no
		4	no	unknown	yes	unknown	no
Machery	2015	1	no	unknown	unknown	unknown	no
		2	no	unknown	unknown	unknown	no
Machery	2015	1	no	unknown	unknown	unknown	no
Sytsma	2015	1	no	unknown	unknown	unknown	no
Beebe	2016	1	no	unknown	unknown	unknown	no
		2	no	unknown	yes	unknown	no
Izumi	2017	1	no	unknown	yes	unknown	no
		2	no	unknown	unknown	unknown	no
Kazaki	2017	1	no	unknown	yes	unknown	unknown
Cova	2018	1	no	unknown	no	unknown	no
Colombo	n.p.	1	no	unknown	yes	unknown	no