Best practice guidance for linear mixed-effects models in psychological science

Lotte Meteyard[1*]

Robert A.I. Davies[2]

Word count: 14,692 (including tables)

Keywords: linear mixed effects models, hierarchical models, multilevel models.

Affiliations

[1*] *Corresponding author*
School of Psychology & Clinical Language Sciences,
University of Reading, Berkshire, UK, RG6 6AL.
l.meteyard@reading.ac.uk

[2]
Department of Psychology,
Lancaster University, Lancaster, UK, LA1 4YF.

THIS MANUSCRIPT IS A PRE-PRINT AND HAS NOT UNDERGONE PEER REVIEW

Acknowledgements

This work was supported by a British Academy Skill Acquisition Grant SQ120069 and University of Reading 2020 Research Fellowship to LM. We would like to thank all the students, researchers and colleagues who have discussed mixed models with the authors over the last few years, and those who responded to the survey. Our thanks to Elizabeth Volke who helped LM prepare the survey and collate results whilst visiting the School of Psychology & Clinical Language Sciences, University of Reading, on an Erasmus studentship.

Abstract

The use of Linear Mixed Effects Models (LMMs) is set to dominate statistical analyses in psychological science and may become the default approach to analyzing quantitative data. The rapid growth in adoption of LMMs has been matched by a proliferation of differences in practice. Unless this diversity is recognized, and checked, the field shall reap enormous difficulties in the future when attempts are made to consolidate or synthesize research findings. The proposed article examines the diversity in modeling practice by two methods – a survey of researchers (n=163) and a quasi-systematic review of papers using LMMs (n=400). The survey reveals substantive concerns among psychologists using or planning to use LMMs and an absence of agreed standards. The review of papers complement the survey, showing variation in how the models are built, how effects are evaluated and, most worryingly, how models are reported. Using this data as our departure point, we present a set of best practice guidance for reporting LMMS. It is the authors' intention that the paper supports a step-change in the reporting of LMMS across the psychological sciences, preventing a future void in which data reported today cannot be transparently understood and used tomorrow.

1.0 Introduction

Linear Mixed-effects Models (LMMs) have become increasingly popular as a data analysis method in the psychological sciences. They are also known as hierarchical or multilevel models (Snijders & Bosker, 2011). LMMs are warranted when data are collected according to a multi-stage sampling or repeated measures design. A researcher may undertake a multi-stage sampling of observations when collecting data about the behavior or attributes of participants recruited, e.g., as students from a sample of classes in a school or schools, or as patients from a sample of clinics in a region or regions. A researcher may undertake a repeated measures investigation, e.g., when collecting data on the outcome of an experimental manipulation when all participants experience the manipulated conditions, or when collecting data on responses to stimuli when all participants are presented with all stimuli. Such investigations are common in experimental psychology. These designs yield data-sets that typically have a multilevel structure. Participant-level observations, e.g., a person's measured skill level or score, can be grouped within the classes, clinics or groups from which the participants are recruited. Trial-level observations, e.g., the latency of response to a stimulus word, can be grouped by the participants tested or the stimuli presented (Baayen, Davidson, & Bates, 2008). This hierarchical structure in the data is associated with a hierarchical structure in the error variance of the outcome observations. That is, we expect that the responses to stimuli from a given participant will be correlated, or that responses from children in the same class or geographical region will be correlated, or that responses to the same stimulus item across participants will be correlated. LMMs allow this grouping in the data to be explicitly modelled.

LMMs have grown very popular in modern Psychology because they enable researchers to estimate effects while properly taking into account this error variance structure. From under 100 Pubmed citations in 2003, the number of articles referring

to LMMs rose to just under 700 by 2013 (see Figure 1), the starting year in our systematic review of LMM practice (reported following). This popularity is associated with an increasing awareness of the need to use LMMs. However, that growth in popularity is also associated with a diversity among approaches that will incubate future difficulties. In simple terms, variation in current reporting standards will make meta-analysis or systematic review of findings near impossible.

The present article examines the diversity in modeling practice and outlines the features of a reproducible approach in using and reporting mixed-effects models.

In an experimental design common to different fields of Psychology (Memory, Wright & Villalba, 2012; Social Psychology, Judd, Westfall, & Kenny, 2012; Psycholinguistics, Baayen et al., 2008), a sample of participants is asked to respond to a sample of stimuli. Often, all participants see all stimuli so that there are multiple observations for each participant, and also for each stimulus. Historically, the dominant approach to such data has been to aggregate the observations. For example, in Psycholinguistics, a researcher would calculate the mean latency of response for each participant, by averaging over the RTs of each stimulus (to get an average RT for a condition or set of stimuli, e.g. nouns vs. verbs by-participant). In a complementary fashion, average RTs for each stimulus would be generated by averaging across the RTs of participants (i.e., average values by-items). The averaging of responses by-participants or by-items would be done within conditions, e.g., generating a mean RT for related primes and unrelated primes. The means of the by-participants or by-items latencies would be compared using Analysis of Variance (ANOVA) in, respectively, by-participants (F1 or F_s) or by-items (F2 or F_i) analyses. If s/he was seeking to correlate the average latency of responses by-items with variables indexing stimulus properties, or by-participants with variables indexing participant attributes, s/he would use multiple regression to estimate the effects of item or participant attributes on the averaged latencies. A series of analyses dating back over 50 years have shown that these approaches suffer

important limitations (Baayen et al., 2008; Clark, 1973; Coleman, 1964; Raaijmakers Schrijnemakers, & Gremmen, 1999).

As Clark (1973; after Coleman, 1964) noted, researchers seeking to estimate experimental effects must do so in analyses that account for random differences in outcome values both between participants and between items. The random differences can include by-participants or by-items deviations from the average outcome (e.g., fast or slow responding participants, items eliciting fast or slow responses), or from the average slopes of the experimental effects (e.g., individual differences in the strength of an experimental effect). The presence of random differences in the intercept or in the slope of the experimental effect between-items meant, Clark (1973) noted, that the at-the-time common practice of using only by-subjects' ANOVAs to test differences between conditions in mean outcomes was likely to be associated with an increased risk of committing a Type I error. Such errors arise in Null Hypothesis Significance Testing (NHST) if a researcher rejects the null hypothesis when the difference between, say, mean outcomes per conditions are actually consistent with random variation. Ignoring random variation in outcomes among stimulus items can mean that significant effects are observed and interpreted as experimental effects, when they are in fact due to uncontrolled variation amongst items (e.g., effects seen in by-participant average RTs are in fact driven by a 'fast' or 'slow' item influencing the means). This was termed the language-as-fixed-effect fallacy.

Clark's (1973) remedy was to perform F1 and F2 and then combine them into a quasi-F ratio (minF') that afforded a test of the experimental effect incorporating both by-participants and by-items error terms. Analyses have shown that minF' analyses perform well in the sense that Type I errors are committed at a rate corresponding to the nominal .05 or .01 significance threshold (Baayen et al., 2008; Barr, Levy, Scheepers, & Tily, 2013). However, such analyses suffer from two critical limitations. Firstly, use of the approach is restricted to situations where data have

been collected in a balanced fashion across the cells of the experimental design. Most researchers know that balanced data collection is rare. Experimenters can make mistakes and observations are missed or lost. Participants make errors and null responses may be recorded. Perhaps critically, in practice, Raaijmakers et al. (1999) showed how the use of minF' declined and was replaced by the reporting of separate F1 and F2 analyses, despite the associated risk of elevated Type I error rates (see also Baayen et al., 2008).

The minF', F1 and F2 analyses are also restricted to situations where data have been collected according to a factorial design. That is, comparing outcomes recorded for different levels of a categorical factor or different conditions of an experimental manipulation. However, researchers often seek to examine the relationships between continuous outcome and continuous experimental variables. Cohen (1983) demonstrated that the cost of dichotomizing continuous variables is to substantially reduce the sensitivity of analyses. This may be especially important where the relationship between outcome and experimental variables cannot be assumed to take a monotonic function (Cohen, Cohen, Aiken, & West, 2003). In such circumstances, researchers have tended to estimate the effects of continuous experimental variables using multiple regression, e.g., predicting by-item mean reading response latencies from a set of predictors capturing different word properties (Balota et al., 2004). However, Lorch and Myers (1990) demonstrated that such by-items regression analyses reverse the language-as-fixed-effect problem by failing to take into account random between-participants differences. They recommended that the researcher conduct a two-step analysis, firstly, conducting a regression analysis separately for each participant, e.g., predicting a participant's response latencies from variables indexing stimulus properties and then, secondly, conducting an analysis of the per-participant coefficients estimates. This approach, sometimes known as slopes-as-outcomes analysis, has been widely used (e.g., Burstein, 1981; see Kreft & de Leeuw, 1998, for a discussion) but it does not take

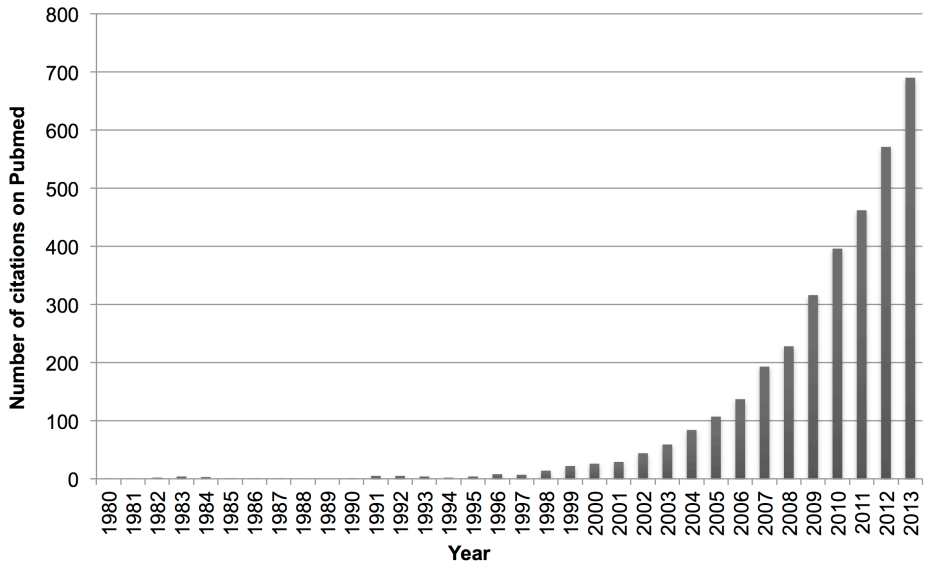
into account variation between participants in the uncertainty about coefficients estimates (e.g., if one participant has fewer observations than another). That is, in a two-step analysis it is not possible to distinguish variation between per-participant coefficients and error variance (Snijders & Bosker, 2011). As well as avoiding the language-as-a-fixed-effect-fallacy, LMMs are also a solution to the limitations of slopes-as-outcomes analyses.

Introductions to LMMs (e.g., Snijders & Bosker, 2012) often discuss random differences between sampling units (e.g., between participants) either as error variance that must be controlled, or as phenomena of scientific interest (e.g., Baayen et al., 2008, Bolker et al., 2009, Gelman & Hill, 2007; Kliegl et al., 2011). Either way, LMMs allow this variation to be modeled by the experimenter as random effects. This means specifying that the measured outcome deviates, per sampling unit, from the average of the data set (random intercepts) or from the average slope of the experimental or covariate effect of interest (random slopes). For example, modeling participants with intercepts that vary from the grand mean intercept (e.g.. for RTs, modeling 'fast' or 'slow' participants), or participants with slopes (experimental effects) that vary from the average slope (e.g., for RTs, modeling participants that show stronger than average or weaker than average experimental effects). Random intercepts and random slopes can tell us how much of the overall error variance is accounted for by variation between sampling units, e.g., the *differences* in overall RT across participants or items. They can also tell us what the modelled difference is for a given sampling unit, e.g., by how much does each participant's overall RT differ from the grand mean RT?

It is worth highlighting that if these systematic differences in hierarchically structured data-sets are not properly accounted for, then false positive results become worryingly high (e.g., a Type I error rate as high as 80%: Aarts et al., 2014; see also Clark 1973; Rietveld & van Hout, 2007) and the power of summary statistics (e.g., means) to detect experimental effects is reduced (Aarts et al., 2014). More

generally, an analysis that fails to account for potential differences between sampling units in the slopes of experimental variables can mis-estimate the robustness of observed effects (Gelman, 2014). For example, one third of participants may show an effect in a positive direction and half show an effect in a negative direction. If this variation is not captured, the estimated direction of the average effect across all participants can be misleading (Jaeger, Graff, Croft, & Pontillo, 2011). Given these numerous analytic advantages, LMMs have been rapidly adopted, particularly in subject areas such as psycholinguistics (Baayen, 2008; Baayen et al., 2008).

Figure 1 Title: Number of Pubmed citations for 'Linear Mixed Models' by year



1.1 So what is the problem?

The problem for researchers is that there are multiple analytic decisions to be made when using LMMs. This issue is not new to the advent of LMMs. Simmons, Nelson, and Simonsohn (2011) demonstrated the decisive impact on results of 'researcher degrees of freedom'. Silberzahn & Uhlmann (2015) likewise showed that the same data can reasonably be analysed in a variety of different ways by different research groups. Neither demonstration depended on the use of LMMs. The

proliferation of alternate findings that arise from variation in choices at each point in a sequence of analytic decisions is crystalized by Gelman and Loken (2014) in the metaphor 'the garden of forking paths'. Multiple analytic steps make variation in observed results more likely, even when reasonable assumptions and decisions have been made at each step (Gelman & Loken, 2014; Silberzahn et al.). Thus, this paper reports on the use of LMMs in the context of ongoing concerns regarding statistical best practices across the cognitive and neurosciences (e.g. Carp, 2012a, 2012b; Chabris et al., 2012; Cumming, 2013a, 2013b; Ioannidis, 2005; Kriegeskorte et al., 2009; Lieberman & Cunningham, 2009; Pashler & Wagenmakers, 2012; Simmons, Nelson & Simonsohn, 2011; Vul et al., 2009). As we shall report, the decisions that researchers must make when conducting LMMs appear to be associated with a heightened sense of uncertainty and insecurity.

Replicability and reproducibility are critical for scientific progress, so the way in which researchers have implemented LMM analysis must be entirely transparent. The same issues have arisen elsewhere, for example, following the rapid expansion in neuroimaging studies in which complex analyses with multiple analytic steps are the norm (Carp, 2012a; Carp, 2012b; Poldrack & Gorgolewski, 2014; Wager, Lindquist, & Kaplan, 2007). The key issue is that hidden variation in practice has a direct and damaging impact on our ability to aggregate data and accumulate knowledge. The rapid adoption of LMMs has not been complemented by the adoption of common standards for how they are applied and, critically, how they are reported (e.g., Barr et al., 2013). This is despite the fact that many excellent introductory texts are available (e.g., Baayen, Davidson & Bates, 2008; Baayen, 2008; Bates, 2007; Bolker et al., 2009; Bryk & Raudenbush, 1992; Gelman & Hill, 2007; Goldstein, 2011; Hox, 2010; Judd, Westfall, & Kenny, 2012; Kreft & de Leeuw, 1998; Pinheiro & Bates, 2000; Snijders & Bosker, 2011; see also Appendix 3). There is some divergence in recommendations (e.g., Barr et al., 2013 vs. Bates, Kliegl, Vasishth & Baayen, 2015) but otherwise the literature is highly consistent in terms of

recommendations for best practice. There are also a number of tutorials for different disciplines which include examples and technical descriptions of software use (Baayen, 2008; Brauer & Curtin, 2018; Brysbaert, 2007; Chang & Lane, 2016; Cunnings, 2012; Field, Miles & Field, 2009; Field & Wright, 2011; Jaeger, 2008; Kliegl, 2014; Magezi, 2015; Murayama, Sakaki, Yan, & Smith, 2014; Rasbah et al, 2000; Rabe-Hesketh & Skrondal, 2012; Schluter, 2015; Th. Gries, 2015; Tremblay & Newman, 2015; West & Galecki, 2011; Winter, 2013). The real concern is not what we are advised to do, but what we actually do and what we publish (e.g., Raaijmakers et al., 1999; Raaijmakers, 2003).

1.2 Present study

If adoption of an analytic method is accompanied by varied practice that is not well documented, how can results from this first wave of work be understood, replicated, or aggregated for use in meta-analysis? In the present article, we will examine the diversity in practices adopted by different researchers when reporting LMMs, and the uncertainty that that diversity appears to engender. We completed a survey of current LMM practice in psychology. This consisted of two parts, a questionnaire sent out to researchers and a review of papers that used LMM analyses. To anticipate the results presented below, we found widespread concern and uncertainty about the implementation of LMMs alongside a range of reporting practices that frequently omitted key information. The survey demonstrates the assimilation of a data analysis method in our discipline in 'real time'. To address the concerns raised by the survey, we present a set of best practice guidance. We will discuss the challenges in using mixed models and present a set of guidelines for clear and unambiguous reporting of mixed model analysis.

2.0 Questionnaire

2.1 Method

2.1.1. Participants

163 individuals completed the questionnaire: 94 females, 63 males and 6 who did not disclose their gender. Mean age was 36 years (standard deviation, SD = 9.26, range = 23-72). Just under 40% of respondents reported their discipline as Psycholinguistics, 16% Linguistics, 11% Psychology, 5% Cognitive Science/Psychology, 4% Language Acquisition and 3% Neuroscience; 15% of individuals reported more than one discipline. A number of other disciplines were reported by individuals (e.g., Anthropology, Clinical Psychology, Reading). Mean number of years working in a given discipline was 10.38 (SD = 8.16, range = 0.5-30). Data on academic position and institution can be found in Table 1.

Table 1: Reported position and institution type for questionnaire respondents (% of total)

Position	**%**	**Institution**	**%**
Undergraduate	0	University UK	25.77
Postgraduate MSc	1.23	University Other	59.51
Postgraduate PhD	24.54	Research Institute UK	0.61
Postdoctoral researcher	26.38	Research Institute Other	9.82
Lecturer/Assistant Professor	24.54	Institution Other	4.29
Reader/Senior Lecturer /Associate Professor	11.04		
Professor	9.20		
Other	3.07		

2.1.2 Design and procedure

A qualitative questionnaire was used, with both open and closed questions (see Appendix 1 for the full questionnaire). Ethical approval for the study was granted

by the University of Reading School of Psychology & Clinical Language Sciences Research Ethics Committee. The online questionnaire was implemented in LimeSurvey (LimeSurvey Project Team & Schmitz, 2015). Individuals were invited to complete the questionnaire via email lists and personal emails to academic contacts of the authors, with a request to forward on to any interested parties. All responses to the questionnaire were anonymous. The questionnaire began with a brief introduction to the study. Consent was provided by checking a box to indicate agreement with a statement of informed consent.

The questionnaire elicited answers to questions focusing on the use and reporting of Linear Mixed-effects models. Questions on demographic information elicited information on participants' discipline, the length of time they had spent practicing in that discipline, their academic position and institution type. Open-ended questions then asked participants to report the main challenges they had encountered in using mixed-models, and what concerns they had in the use of these models for their own research or, more generally, for research in their discipline. Participants were then asked to report if they had already used mixed-models for analysis. If they responded yes, they answered questions on how frequently they used mixed-models, what training they had undertaken or what resources they had used to support their implementation, and what software they used. Participants were asked to report a typical formula for models they used in analysis, and their typical practice in reporting results. They were then asked if they had compared the result of "more traditional analysis" to those of LMMs, and, if they had, what differences (if any) had been noted. For participants who had not yet used mixed-models, they were asked to report if they planned to use them and the nature of their interest in LMMs. Finally, all participants answered three questions on reporting practices, their preferences for format of reporting results and views on sharing data and analysis code.

A period of approximately one month was allowed for responses to be collected. Data collection was stopped once we had reached the current sample size, as the sign-up rate to complete the questionnaire had slowed. The sample size was judged adequate for our purposes (frequency and thematic analysis of question responses) and we judged that a substantial increase in numbers was unlikely if we left more time.

2.1.3 Analysis

The complete data can be found at osf.io/bfq39 – Files – Mixed models survey results_analysis.xlsx. For closed questions, the percentage of responses falling into a given category were calculated. For open-ended questions, thematic analysis was completed to identify the most common responses across individuals (Braun & Clarke, 2006). Individual responses to each question (e.g., challenges to using LMEs) were collated as rows in a spreadsheet and given a thematic label to code the response (e.g., software, convergence, lack of standard procedures etc.). Responses were then reviewed and sorted, combining responses that fell into the same thematic label. We were interested in reporting the most common responses, so the total number of responses that fell into a given theme were counted as a percentage of the total responses to that question. For questions where categorical responses were made (e.g., reporting software used, listing training and resources), we generated lists of unique responses and the frequency (% of total) with which each one was reported. The results of the above analyses are presented together.

2.2 Results

2.2.1 Usage of mixed-models

The great majority of respondents (91%) had used mixed-models for data

analysis. The mean year of first using mixed-models was 2010 (SD = 3.94 years, range = 1980 – 2014). We asked respondents to estimate how often they used mixed-models, the mean was 64% of data analyzed (SD = 31%, range = 0-100).

2.2.2 Training & software

A variety of methods had been used to learn mixed-models. The majority of respondents had attended a workshop, course or training event to learn mixed-models (68%), 30% had learnt from colleagues, 21% from internet resources, 12% using specific books or papers, 10% self-taught and 9% learning from a statistics advisor or mentor. Appendix 2 provides a comprehensive list of the specific authors, papers, books, websites and other resources used by respondents. Readers may find this useful for their own training needs.

The majority of individuals used the statistical programming language and environment, R (90%) (R Core Team, 2017), with 20% mentioning the lme4 package (Bates, Maechler, Bolker, & Walker, 2015). Other named R packages were gamm4 (Wood & Scheipl, 2016), languageR (Baayen, 2013), lmerTest (Kuznetsova, Brockhoff & Christensen, 2016), mgcv (e.g., Wood, 2011), and nlme (Pinheiro et al., 2016). The next most frequently used software was SPSS (8%; IBM Corp, 2013). A number of other software applications were named by one or two people: MLwiN (Rasbash et al, 2009), Matlab (Matlab, 2012), Mplus (Muthén & Muthén, 2011), Stan (Stan Development Team, 2016), JASP (JASP Team, 2016), S-PLUS (e.g. Venables, 2014), SAS and ESS (Rossini et al., 2004).

2.2.3 Planned use

For individuals who had not yet used mixed-models (15 respondents), 11 were planning to use them and five were not. For those planning to use them, reasons included exploration of a larger number of predictor variables (5 responses), to look at change over time or longitudinal data (2 responses) and for better

statistical practice (e.g., control of individual differences, inclusion of random effects; 2 responses).

2.2.4 Challenges to using mixed-models

The most frequently reported concern was a lack of consensus or established, standardized procedures (26% of respondents; e.g. "it's quite difficult… to understand what standard practice is"). Related to this were responses that described a lack of training or clear guidelines for analysis, interpretation or reporting results (13%; e.g., "minimal training/knowledge available in my lab", "Presentation of data for publication") and the relative novelty of the analysis (7%; "it is relatively new so recommended practices are in development and not always fully agreed upon"). A number of responses highlighted a lack of knowledge (18%; e.g., "I do not know enough about them", "some reviewers request these models but researchers are not all skilled in these techniques", "complex math behind it not easy to grasp", "not enough people who know it"). A broad challenge in applying conceptual knowledge was seen in responses covering difficulties in selecting or specifying models (25%; e.g., "Model specification - knowing what to include and what not to include"), models which fail to converge or in which assumptions are violated (14%; e.g., "How to deal with models that fail to converge"), understanding and interpreting random effects structures (16%; e.g., "Determining what constitutes an appropriate slope term in the random effects structure"), identifying interactions (4%; "Working out significant interactions") or interpreting results generally (7%; "difficulty in interpreting the results"). Other specific points included models being overly flexible or complex (e.g., "The potential complexity of the models that goes substantially beyond standard procedures", "Mixed models are so flexible that it can be difficult to establish what is the best suited model for a given analysis") and challenges in checking and communicating model reliability (e.g., "Knowing how to test whether a model violates

the assumptions of the specific model type being used"). The most frequently reported concerns are reported in Table 2.

Table 2: Most frequently reported challenges and concerns in using LMMs*

Reported challenge	**%**
Lack of standardized procedures	26
Selecting and specifying models	25
Researcher reports lack of knowledge	18
Understanding and interpreting random effects	14
Lack of training/guidelines for analysis, interpretation and reporting	13
Use of new and unfamiliar software	12
General concern over use of LMM for own analysis	75
Reporting results	15
Model selection	14
Learning and understanding analysis	14
Lack of established standards	11
General concern over use of LMMs for discipline	73
Lack of standards	23
LMMs used when not fully understood	23
Misuse of models	17
Reporting is inconsistent and lacks detail	17
Peer review of LMMs is not robust	10

*identified by thematic analysis

Technical challenges were highlighted, specifically the use of new or unfamiliar software (12%; e.g., "software package (R) I was unfamiliar with") and the reliability of analysis code (e.g., "Some of the code might also not be reliable. For

example, people reported differences when running the same analysis in different versions of the same software"). A number of individuals reported specific difficulties with model coding and fitting (e.g., coding of categorical variables, setting up contrasts, structuring data appropriately, forward and backward model fitting and post-hoc analyses).

A number of responses reflected unease at the shift from traditional factorial designs and ANOVA or other inferential statistical tests (e.g., "[lack of] convincing evidence that mixed models provide information above and beyond F1 and F2 tests"). For example, susceptibility to p-value manipulation or difficulties in establishing p-values (4%; “too many people still believe that we are fishing for p-values if we do not use classical anovas"), knowing how to map models onto study design (4%; "Knowing when it's appropriate to use them", "to understand the influence on future study designs"), difficulties with small samples, sparse data and calculating effect sizes or power.

2.2.5 Concerns using mixed models for own data and in the wider discipline

Around 75% of respondents had concerns over using mixed-models in their own data analysis. For these respondents, the most salient concerns were reporting results (15%; e.g., "Do you report your model selection criteria and if so, in what level of detail… perhaps several models fail to converge before you arrive at one that does?"), selecting the right model (14%; e.g., "model selection"), learning how to do the analysis and fully understanding it (14%; e.g., "I do not have enough knowledge to correctly apply the technique"), a lack of established standards (11%; e.g., “the lack of standardized methods is a problem"), models that do not converge (9%; e.g., "How to deal with convergence issues") and the review process when submitting mixed-model analysis for publication (9%; e.g., “experimental psychology reviewers are often suspicious of them”). Other concerns broadly reflected those already

identified as challenges above. See Table 2 for the most frequently reported concerns.

Around 73% of respondents had concerns over the use of mixed-models in their discipline or field. Here, the key concerns were a lack of standards (23%, e.g., "lack of established standards"), use of models without their being fully understood (23%; e.g., "Overzealous use of random effects without thinking about what they mean"), frank misuse of models (17%; e.g., "Misapplication of mixed models by those not at the forefront of this area"), reports of model fitting being inconsistent and not detailed enough (17%; e.g., "not describing the analysis in enough detail"), a lack of familiarity and understanding of the models (10%; e.g., "lack of knowledge about their implementation") and the review process not being robust (10%; e.g., “Reviewers often can’t evaluate the analyses”). Additional concerns were over researchers being able to misuse the flexibility of mixed-models (5%; e.g., "increased ‘researcher degrees of freedom’ ") or “p-hack” the data (3%; e.g., "It's easier to p-hack than an ANOVA"), and the breadth of approaches to making decisions during model fitting (4%; e.g., "The variety of approaches people take for deciding on model structure"). There was also concern over why mixed-models were deemed better than factorial ANOVA approaches (3%; e.g., "Why are they privileged over simpler methods?") and that it was difficult to compare them against these traditional approaches (2%; e.g. "less accessible to readers/reviewers without experience… than traditional analyses"). See Table 2.

2.2.6 Current practice

For respondents who were currently using mixed-models, 70% did not specify variance-covariance structures for mixed models. We asked people to provide a typical model formula from their analyses. Two individuals stated that they used SPSS, and therefore did not specify model formulae. Of those who did provide an example, only three explicitly mentioned model comparison and model checking. See

Table 5 for a summary of random effects from model examples. 100% specified random intercepts for subjects/participants and 92% specified random intercepts for items/stimulus materials or trials. Random slopes to allow the effect of interest to vary across subjects and/or items were less common (62%).

Table 3: Current practice

Current practice	**%**
Do you specify variance-covariance structures?	
Yes	30
No	70
Random Effect structures from model examples:	
Random intercepts for subjects	100
Random intercepts for stimuli/trials	92
Random slopes for effect to vary across subjects	62
Comparison to factorial analysis (ANOVA)	
Do you compare LMMs to factorial analysis?	
Yes	61
No	24
N/A	15
Were results comparable?	
Yes	33
No	46
N/A	21
How do LMMs compare to factorial analysis?*	
LMM are better fit to data	28
Largely comparable	26
LMMs are more sensitive/less conservative	16
LMMs are more conservative	8

*identified by thematic analysis

When included, random slopes were often qualified on the basis of experimental design and only included when appropriate for the data structure (e.g., random slopes for within-subject factors; Barr et al., 2013). Where multiple predictor factors were included, interactions between factors for random slopes were typically included. It is notable that some respondents stated that they did not include interaction terms for random slopes, excluded these first if the model failed to converge, or simplified random effects until the model converged. Some removed the modeling of correlations between random effects for the same reason. See Table 3.

2.2.7 Comparison to traditional approaches

Around 61% of respondents had compared the results of mixed-model analyses to the results of more traditional analyses (i.e. ANOVA or other factorial inferential statistics; 15% responded N/A). Of those, 33% reported that results had been comparable, 46% reported that results were not comparable and 21% responded with N/A. An open question asked for respondents' evaluation of this comparison. The most frequent response was that results were comparable (26%; e.g., "Largely methods correspond to each other"). A number of responses identified that mixed-models were preferred or gave a better, more detailed fit to the data (28%; e.g., "I think we got a better fit for our data using LMEs instead of the traditional ANOVAs/Regression models"). However, it is not clear whether results were comparable in terms of the size of numeric effects or coefficients. Responses instead focused on whether results were significant. Mixed-models were reported to be more sensitive/less conservative, demonstrating significance for small effects (16%; e.g., "differences can occur if effects are just above or below p=.05", "mixed models seems less conservative than for example (repeated measures) anova"). However, mixed-models were also found to be more conservative, depending on how the random effects structures were specified (8%; e.g., "Mixed models are typically more conservative, but not always"). Traditional F1/F2 tests were sometimes

used to confirm or interpret effects in the mixed-models (4%; "I look if both analyses point to the same effects of the experimental manipulations") and in one instance F1/F2 tests were reported to be “much easier and less time-consuming” than mixed-models. See Table 3.

2.2.8 Reporting & Preferred reporting

Respondents were asked for their typical practice when reporting models, this question was multiple choice and a summary of responses is given in Table 4. The vast majority reported p-values and model fitting (88% and 80% respectively), but other options were chosen much less often: model likelihood was reported by 50% of respondents, confidence intervals by 37%, specification/reporting of model iterations by 36% and F-tests between models by 31%.

Table 4: Current practice in reporting mixed models (% total*)

What is reported	% Yes	% No
p-values	88	12
Model fitting	80	20
Likelihood	49	51
Confidence intervals	37	63
Iterated models	36	64
F-tests	31	69

*ordered by frequency of response high to low, rounded up to nearest %; 147 responses

We asked respondents to state how they preferred models to be reported (in the text, as a table or as a partial effects plot). The majority were in favour of a table (53%), followed by written information in the text (19%) and then plots (15%). The main reasons for selecting tables were ease of reading and clarity. Written text could provide details and facilitate interpretation. Plots were deemed important for more complex models and to visualize the model structure. Some individuals stated that reporting format should depend on the data and model complexity (7%). See Table 4.

2.2.9 Sharing of Code and Data

We asked respondents to state whether they would share data and code, with 70% responding that they would share both (e.g., “Yes. Science should be open in its practice”). Table 5 summarizes the responses. Some respondents specified that they would share data only after publication, on request, after an embargo or when they had finished using it (9%; e.g., “I would be willing to share data on personal request”). Reasons for sharing included being open and transparent or a duty to share work that had been publicly funded (e.g., “yes, always. No-brainer: tax-payer-funded scientist”). A number of respondents identified a general benefit to the field and to improve standards. For example, to contribute to meta-analysis or further data exploration (e.g., “… to facilitate additional research and replication of previous results. This data would also be extremely helpful for meta-analyses and for future research to be able run power analyses based on previous findings”). Analytic rigour was also mentioned, for example having a more open discussion about how models are used, checking model fitting, correcting errors, and having more experienced people look at the data (e.g., “We definitely need transparency and standards here because most of us are not statisticians”). Around 3% would not share data and 3% were unsure. Reasons included not wanting to be ‘scooped’, being unsure if data sharing was allowed on ethical grounds. One respondent asked “Why should I share my data?”.

Table 5: Sharing of code and data*

Would you share data and code?	**%**
Share both data and code	70
Share code	15
Specified sharing of data after publication	9
Would not share either	3
Unsure	3
Would you like access to data and code?	%
Access to both	74%
Access to both but unlikely to use it	6
Access to code	9
Did not want access to either	3
Did not want access to code	3
Did not want access to data	2
Unsure	2

*Identified from thematic analysis

Around 15% responded that they would share code, with no statement about data sharing. Reasons for sharing code included it being good practice and good for learning, as well as comparing analyses (e.g., “Good practice, other researchers can look at what you did and learn something, or point out errors”, “I think it is helpful to share code. This will hopefully lead to a more open discussion of the choices we all make when doing this type of analyses”). Two individuals stated that they would not share code due to their inexperience. A few respondents mentioned difficulty in sharing code that could often be ‘messy’ and that it would be time consuming to prepare code for publication.

We asked respondents to state if they would like to access data and code in published reports. Around 74% would like access to both, with a further 6% specifying yes but that they would be unlikely to use it. Reasons for accessing were

broadly similar to those identified above, with mention of transparency, improved standards, for learning, for meta-analysis, analytic rigour and checking reported data. Some respondents reported that current data sharing practices were already sufficient (e.g., sharing data on request, depositing in centralised archives, e.g., “Doesn't have to be in published reports. Can be in a database accessed via the publisher or institute”), or that this was a wider issue and not specific to mixed-models (e.g., “I don't see the access to data and code being a mixed effects specific issue. This is for any paper, regardless of the statistical technique used”). A smaller number specified that they would like access to code (9%) with no statement about data. Some respondents qualified that data and code should be part of supplementary materials or a linked document, rather than in the publication itself. Finally, a few people did not want access to code (3%), data (2%) or both (3%), or they were unsure (2%). See Table 5.

2.3 Discussion

Most respondents had concerns over the use of LMMs in their own analyses and in their discipline more widely. Concerns were driven by the perceived complexity of LMMs, with responses detailing a lack of knowledge (own knowledge, that of reviewers or other researchers). Our interpretation is that this knowledge deficit (perceived or real) drives the other concerns. Namely, difficulties in learning and understanding the analysis process and difficulties in building, selecting and interpreting LMMs. For some, these difficulties are compounded by having to learn about new software applications (for an overview of software applications and their comparability see McCoach et al., 2018). Respondents were concerned by not knowing what to report or how to report results from LMMs. This point feeds into reports of LMMs being received skeptically by reviewers as inconsistent formatting

and presentation of analyses will exacerbate difficulties in the review process. Given that reviewers are sampled from the community of active researchers, lack of knowledge in reviewers was also a concern. A complement to this was that some respondents stated that LMMs were overly flexible and more prone to p-hacking than traditional factorial ANOVA. At present, we are using a method of analysis that the community feels is not well understood, not clearly reported and not robustly reviewed. Little wonder that it is seen as overly flexible and yet another way of fishing for results.

The survey showed that whilst most researchers report p-values for model coefficients and some detail of model fitting for LMMs, fewer provide details of iterated models, F-tests or Likelihood comparisons between different models. This means that, in general, the number of decisions being made during model fitting and the process of model selection is not transparently reported in manuscripts. This lack of transparency should not be seen as deliberate obfuscation: most respondents were willing to share analysis code and data, and felt that it was important to do so. The multiple decision points during LMM analysis (building in fixed effects and random effects, model comparison and selection) are a legitimate concern. Multiple analytic steps can produce different results for the same data set (Gelman & Loken, 2013; Silberzahn & Uhlmann, 2015) and give the impression of unprincipled flexibility. The rapid uptake of LMMs has been driven, in part, by the need to explicitly account for both subject- and item-related random error variance (Locker, Hoffman & Boviard, 2007; Baayen, Davidson & Bates, 2008; Brysbaert, 2007) and part of the anxiety over model building arises when one moves from factorial ANOVA into LMMs (Boisgontier & Cheval, 2016).

Although ANOVA and LMM share a common origin in the general linear model, they are very different in terms of execution. In LMMs, the analysis process is similar to regression (Bickel, 2007). A model equation for the data is specified and reliable analysis requires larger data sets (e.g., trial level data or large samples of

individuals, Baayen, 2008; Luke, 2016; Maas & Hox, 2004; 2005; Pinheiro & Bates, 2000; Westfall, Kenny & Judd, 2014). Nested models may be compared or 'built' to find the best fit to the data. The process feels notably different to producing a set of summary statistics (e.g., averaging responses to all items for a subject), which are then put through a factorial analysis (such as ANOVA). Here, a typically limited set of main effects and interactions are specified from experimental design and then analysed 'once'. As noted in the Introduction, language research developed a tradition of ANOVA by subjects (F1) and by items (F2). We argue that this shift from ANOVA to LMM is part cause of the widespread concern reported by researchers. Survey responses reflected this uneasy shift. For respondents who had compared LMMs to ANOVA, a third found comparable results but nearly half found results that were not comparable. For those who had compared the two analyses, LMMs were reported to be a better fit to the data, but could be both more or less conservative especially when effects were marginally significant under ANOVA. ANOVA or other factorial analyses were sometimes used to interpret the results from LMMs. That is, individuals were using an analysis they felt comfortable with to interpret an analysis they did not fully understand. It is worth noting that LMMs are not a new level of complexity for statistics in cognitive science (e.g. structural equation modelling, Bowen & Guo, 2011; growth curve modelling, Nagin & Odgers, 2010), especially when compared against advances in brain imaging analysis and computational modelling. However, the perceived complexity of LMM analysis is demonstrated in our survey by responses repeatedly referring to a lack of knowledge and established standards.

The survey data clearly demonstrates that researchers are uncomfortable with the use of LMMs. This is despite a number of excellent texts (see Appendix 2, and references given above in the Introduction) and an explosion of online tutorials and support materials. However, the key problem is not what we are supposed to do, but what we actually do and how it is communicated. To that end, we completed a

review of published papers using LMM analysis. This was the purpose of the second part of our study.

3.0 Review of current practice in use and reporting of LMMs

The objective was to review current practice in the use and reporting of LMM/GLMMs in linguistics, psychology, cognitive science and neuroscience. This complements the survey by adding objective data on how LMMs are used and reported. If we see substantial variation in the application and reporting of LMMs, particularly where it conflicts with transparency, then the need for best-practice guidelines is warranted. Variation in analysis choices will also speak to current critiques of statistical analysis in psychology – in particular, researcher degrees of freedom in completing analyses and how 'significance' is established (Simmons et al., 2011; Silberzahn & Uhlmann, 2015; Gelman & Loken 2014).

3.1 Method

We completed a review of published papers using LMM analysis, taking a sample rather than exhaustively searching all papers. This approach was chosen to make the review manageable. To start, the first author used Google Scholar to find papers citing Baayen et al. (2008), chosen as it is widely as a seminal article whose publication was instrumental to the increased uptake of LMM analysis (e.g., it has now been cited over 3500 times). To keep the review contemporary papers were chosen from a four-year period spanning 2013, 2014, 2015 and 2016. Papers had to

be in the field of language research, psychology or neuroscience (judged on the basis of title, topic and journal). From each year, the first 100 citations fitting the above criteria were extracted from Google Scholar, when limited by year, giving 400 papers in total. The first search was completed on 30th May 2017, giving a total of 3524 citations for Baayen et al (2008) with 2360 citations between 2013-2016. Therefore, we sample ~17% of the papers fitting our criteria, published in that four-year period. Sixteen papers were excluded as they did not contain an LMM analysis (e.g., citing Baayen et al. in the context of a review, or when referring to possible methods). One paper was not accessible. Three papers were initially reviewed to establish the criteria for classifying papers, with an excel spreadsheet created with a series of drop-down menus for classification. To check coding and classifications, the second author looked at one reported model from 80 papers (20% of the total papers coded; 20 papers from each year). Initial agreement was 77%, with differences resolved by discussion. The spreadsheet with all the data and classifications from the review can be found here (https://osf.io/bfq39/; Files – Baayen Papers Rev with coding check.xlsx). Classification criteria are summarized in Table 6, and a fuller description of these can be found in Appendix 3.

Table 6: Classification criteria for review and associated data table

Criteria	Options	Data Table
Field / Topic	Psychology, Linguistics & Phonetics, Neuroscience, Psycholinguistics.	
Model Type	LMM, GLMM, LMM & GLMM, GAMM, Other.	A4.1
Approach	ANOVA testing for fixed effects via LRTs/model comparison ANOVA testing with random effects of interest Regression with random effects control for subject / item variance Regression with multiple predictors and control variables Regression with random effects of interest Repeated measures / control for hierarchical sampling Repeated measures with random effects of interest	A4.2

Model Comparison	LRTs. AIC/BIC, LRTs & AIC/BIC, Descriptive	A4.3
Statement on model selection	What detail is given by the authors on how different models have been compared, or a final model selected.	A4.4
Convergance / Random Effect simplification	What detail is given by the authors of any convergence issues and what was done to deal with this (e.g. model simplification)	
Model equation	Yes reported, not reported, given for some and not others	A4.5
Dependent variable	RT, Errors / Categorical variable, RT & Errors, eye movement data, brain imaging data, other.	
Fixed Effects 1	IV, IV & Control variables	A4.6
Fixed Effects 2	Main effects, main effects & interactions	
Random Effect approach (if mentioned)	LRTs; LRT & AIC/BIC; LRTs/AIC for slopes; Maximal structure; LRTs backwards from maximal, LRTs upwards from minimal; LRTs against null.	A4.7
Random Effect Intercepts modelled	Subject, Item/other, Subject & Item/other, Subject, item & other, Item & other	
Random Effect Slopes modelled	FE over subject, FE over item/other, FE over subject & items/other, FE over subject with interactions, FE over items/other with interactions, FE over subject & items/other with interactions	
Random Effect covariances modelled	Yes reported as modelled, no not modelled, unclear whether modelled or not.	
Reporting Format	Text only Text & Tables Text, tables & figures Table & Figures Text & Figures Figures Tables	A4.8
Reporting Fixed Effects	Coefficients Coefficients, t & p Coefficients, SE/CI Coefficients, SE/CI, t/z Coefficients, SE/CI, t/z & p Coefficients, SE/CI, p Coefficients, p t/z, p p *Additional note if condition means reported, not coefficients*.	A4.9
Reporting Random Effects	Variance, variance & covariance, or not reported.	A4.10
Model fit reported	R2, model estimate correlation with data, R2 & est. correlations, AIC/BIC, Log Likelihood, other (define), no.	A4.11

P values (if mentioned)	Assume t > 1.96 / 2 MCMC LRTs F tests Sattherwaite Kenward-Rogers	A4.12
Appendices for full reporting (if mentioned)	Yes.	A4.13

3.3 Results

The complete data set can be found at osf.io/bfq39 and tables with counts in Appendix 4. Here we will summarise the data by walking through the stages of LMM analysis: model selection, evaluating significance and reporting results.

3.3.1 Model Selection

Across the four years, the majority of papers used LMM (n=193). GLMM (n=88) or a combination of both LMM and GLMM (n=95) was also common. General Additive Models (GAMs) were rare in our sampled papers (n=5; see Table A4.1 in Appendix 3).

Models were used in a number of different ways. The classic use of LMMs for hierarchical sampling designs was present relatively infrequently (n=26), which may be a result of the sampling process. That is, the Baayen et al. (2008) paper presents LMMs as a method to control for by participant and by item variation in cognitive science research whereas, in educational and organisational research, LMMs have been used for a number of years to address questions concerning data recorded according to hierarchical sampling designs (Gelman & Hill, 2007; Scherbaum & Ferreter, 2009; Snijders, 2005). The majority of papers used LMMs as a variant on regression with random effects controlling for participant and item variation (n=272) but a number also used LMMs as a replacement for ANOVA (n=61). It was relatively

rare for studies to look at the random effects as data of interest (n=13; see Table A4.2).

Model comparisons for fixed effects were not present in all manuscripts (typically present in ~50-60 papers in each year; Table A4.3). When they were present, the majority reported Likelihood Ratio Tests (LRTs, n=129), with fewer reporting Akaike Information Criterion or Bayesian Information Criterion (AIC/BIC, n=12) or a combination of LRTs and AIC/BIC (n=20). Some papers described the model comparison process but did not provide data for the comparisons themselves (n=54). Reporting the model selection process was infrequent (typically present in ~20-25 papers in each year; Table A4.4) and a wide variety of practices were present. Manuscripts reported "best fit" models following LRTs or AIC/BIC comparisons (n=23) or minimal model approaches in which models were simplified by removing fixed or random effects that were not significant (n=31). Models were also selected by moving from maximal to minimal models (n=6) or minimal to maximal models (n=8), or specifically mentioned backwards fitting (n=7). Model comparisons for random effects were also not present in all manuscripts (Table A4.5). The numbers that did test for the inclusion of random effects increased over time (2013 = 16, 2014 = 33, 2015 = 43, 2016 = 42). When a specific approach was reported, there was a clear preference for using a maximal random effects structure (Barr et al., 2013; n=86), followed in frequency by a preference for using Likelihood Ratio Tests to determine random effects structures (LRTs, n=25). Less common was a combination of starting with a maximal structure and then using LRTs to simplify (n=11) or starting with a minimal structure and using LRTs to add more complex random effects (n=7).

Reporting of convergence issues was increasingly common over the four-year period (2013 = 2, 2014 = 8, 2015 = 14, 2016 = 21; Table A4.4), and a variety of methods were reported for dealing with this. Models were simplified by removing slopes (n=9), removing correlations between intercepts and slopes (n=2), removing both slopes

and correlations (n=4), including slopes with the largest variance or removing those with the smallest variance (n=6). Some manuscripts reported the "fullest model that converged" without specific detail on how simplification took place (n=14).

Fixed effect predictors were most often modelled as main effects and interactions (n=287) as compared to main effects alone (n=94), the inclusion of control variables was also common (n=109; Table A4.6). Concerning random effect variance terms, the vast majority of models included random intercepts for both participants and items (n=277; Table A4.5), with a good number that included intercepts for participants only (n=64). Random slopes were present in around half the papers (2013 = 41, 2014 = 50, 2015 = 67, 2016 = 58; Table A4.7). Most commonly, random slope terms were included to capture variation in fixed effect predictors varying as main effects over participants (n=78) or over both participants and items (n=94). It was less common to include the variation of fixed effect interactions as slopes over subjects and/or items (n=36). Where random slopes were modelled, it was rare for manuscripts to explicitly report whether correlations or covariances between intercepts and slopes had been modelled (~10-15 papers per year) and this information was often unclear or difficult to judge (n=63).

A simple way to report the structure of a model is to provide the model equation (Table A4.7); this was given in a minority of papers with a clear increase over time (2013 = 7, 2014 = 6, 2015 = 26, 2016 = 22, total n = 61). However, the majority of papers did not provide this information (n=317).

3.3.2 Evaluating significance

From the manuscripts reviewed, we classified 10 different combinations or approaches to evaluating significance for fixed effects (see Table A4.8). It is worth noting that only around half the papers reported the method used (n = 207), so we can assume that researchers employed methods that were defaults for software packages. The main methods reported were MCMC bootstrapping procedures

available in R (n=71), assuming t was normally distributed and taking t>1.96 or t>2 as significant (n=52), or taking p values for fixed effects from significant changes in Likelihood Ratio Tests when comparing models with and without the fixed effect of interest (n=40). Other options for evaluating significance were using specific corrections for calculating degrees of freedom (e.g., Sattherwaite, n=20; number of observations – fixed effects n=2), or using F Tests calculated over the model output (n=23).

It was very rare for measures of model fit to be reported (Table A4.9), with most papers not providing this information (n=330). When model fit information was provided, it was most often the Log Likelihood and/or AIC/BIC value (n=35), which are informative relative to another model of the same data. R^2 was provided in a few cases (n=8).

3.3.3 Reporting results

Manuscripts typically used text, tables and figures to report model output (n=151) although other options were evenly split over text and tables (n=85) and text and figures (n=94), with a reasonable number only reporting model output in the text (n=52; Table A4.10). This means that a substantial number of papers do not provide a summary of model output in a table, as you would expect for an analysis derived from regression.

When reporting fixed effects, we saw every possible variation in what information was provided (Table A4.11). The majority of studies reported fixed effect coefficients, standard errors or confidence intervals, a test statistic (t/z) and a p value (n=128), it was also common to report the coefficients and the standard error or confidence intervals with a test statistic but no p value (n=52) or a p value but no test statistic (n=39). However, it was also common to report the coefficients without standard errors or confidence intervals (n=73), or to report no coefficients and provide only a test statistic or a p value (n=43).

When reporting random effects (Table A4.12), practice was less variable but this was principally because most studies did not report random effects at all (n=304), with 51 papers reporting variances and 23 reporting variances and correlations or covariances.

A small number of papers used appendices to provide a complete report on model selection, fitting and code used for analysis (n=25, Table A4.13).

3.4 Discussion

The variation in practice evident from the review of papers mirrors the uncertainty reported by surveyed researchers. Naturally, some of the variation will be attributable to what is appropriate to the data and the hypotheses (e.g., the use of LMMs or GLMMs, the modelling of main effects only or interactions). What concerns us is the evidence for unnecessary or arbitrary variation in the use and reporting of LMMs. Because it is arbitrary, this variation will make analyses difficult to parse and it will incubate an irreducible difficulty (given low rates of data or code sharing) for the aggregation or summary of psychological findings. This difficulty will, necessarily, impede the development of theoretical accounts or practical applications.

Prior to completing this work, we hypothesized that models were being used in different ways by the research community – as an alternative to multiple regression or as an alternative to ANOVA. We found some support for this, the vast majority of models (70%) were framed as regression analyses, but around 15% were framed as ANOVA analysis. We also found other approaches, for example, whether the random effects were reported as data of interest, or whether the study was explicitly controlling for a hierarchical sampling procedure. For model building, around 56% of the papers reported some form of model comparison process (although not always giving details or output). For model selection, 24% provide

explicit detail on the approach taken for fixed effects and around 35% provided detail on how the random effects structure had been chosen. We have a situation in which LMM analyses are being framed in a variety of ways and then often reported with a lack of detail on how the final model was reached. The review of papers clearly reflects the diversity of practice and a lack of transparency and detail in reporting. This makes the diversity confusing rather than a source of information. In this context, it is not surprising that researchers report confusion and a lack of knowledge.

Of particular interest was the variation in how significance was established. Around half the papers reported a specific method, meaning that half did not. We can assume that they used default options available in whatever software they were using. Of those reported, we found 10 different methods for testing significance. Depending on the study (e.g., confirmatory hypothesis testing or data exploration) researchers will have different needs for their analysis (Cummings, 2012). When used as a replacement for ANOVA or ANCOVA, we anticipated that what researchers want, and what they want to report, is something similar to an F test that provides a p-value for the main effect or the interaction effect. This can be achieved by setting up a random effects model and then adding main effects or interactions as fixed effects, testing to see if these significantly improve model fit (e.g., Frisson et al., 2014; Trueswell et al., 2013). Alternatively, LMM effects can be evaluated by employing an ANOVA to get F-tests for predictors. Here, the ANOVA summarises the variation across levels of a predictor, and therefore how much variation in the outcome that predictor accounts for (e.g., if there is zero variation across experimental conditions, that manipulation does not change the outcome; Gelman & Hill, 2007). It is interesting to note that Gelman and Hill (2007) suggested the latter use of ANOVA not as a final analysis step in establishing significance, but as a tool for data exploration to inform which predictors are interesting when building models.

We found 63 papers that evaluated significance by using F tests or model comparison (~30% of the papers that reported a specific method of testing significance). However, it was not the case that LMMs framed as ANOVA always used this method for evaluating significance: such cases were evenly split across analyses framed as ANOVA (n=30) and those framed as regression (n=31, see Table A4.14). Where the analysis was framed as regression, we expected that it would draw on the power of LMMs to account for nested sampling groups (e.g., geographic or genealogical relationships between different languages, Jaeger, Graff, Croft, & Pontillo, 2011), modelling the influence of individual differences (e.g., such as age, Davies et al., 2017), change over time in repeated measures data (e.g., Walls & Schafer, 2006), or accounting for multiple predictor variables (Baayen & Milin, 2010; Davies et al., 2017). What researchers want and what they want to report here is more similar to regression, exploring model building and comparison (e.g., Goldhammer et al., 2014), coefficients for predictor variables, and possibly an exploration of the random effect structures. The vast majority of manuscripts were framed as regression and of those that provided explicit detail on how p-values had been computed (n=141), the vast majority reported the significance of coefficients (n=122) in line with what you would expect from a regression. Interestingly, it was almost never the case that papers reported both (n=2). That is, reporting both whether a coefficient was significant *and* whether the inclusion of that predictor improved model fit.

4.0 General Discussion

In our introduction, we raised significant concerns about the variation in use and reporting of LMMs, and the impact of this variation on our capacity to understand what researchers have done, and consequently on future efforts to aggregate evidence (meta-analysis, replication, review). The survey responses showed significant concern over the lack of consistent reporting standards, and this was strongly supported by the literature review.

We observe that it is the reporting of models that is the principle point of failure. We saw that model equations were very rarely reported though this is a simple means to communicate the precise structure of both fixed and random effects. Whilst most papers did use a table to report the model output, there were still a number that did not. Thus, papers using LMM analysis do not always provide a complete summary of the model results. Fixed effect coefficients were not always reported with standard errors or confidence intervals, though these statistics are essential data for meta-analysis and power analysis. Random effects were hardly reported at all. This means that the reporting of LMMs often ignores the key reason for using the analysis in the first place: an explicit accounting for the variance associated with groupings (sampling units) in the data.

Random effect variances are essential for computing the power of LMM analyses (Westfall, Kenny & Judd, 2014) and are therefore absolutely critical for researchers who are planning to use LMMs. How will we know what to include in power calculations if we do not have some estimates of random effect variances from published papers? From a conceptual point of view, random effect variances and covariances allow us to see just how much of the variance in the data can be attributed to, for example, individual variation in predicted effects or to the correlation between overall performance (e.g., fast or slow participants) and the predicted effects (e.g., do fast participants always show a smaller effect?). If we care about

psychological mechanisms, this is very valuable data that is simply not being reported.

In the survey, the need for common standards was also raised for other aspects of LMM analysis, including model building, model comparison, model selection and interpretation of results. There are varying ways to build any statistical model, for example, in linear regression (e.g., stepwise, simultaneous etc.) and so there are varying ways to build an LMM. There is no one approach that will suit all circumstances, therefore researchers should report *fully* the process they took and provide a rationale. A common core of information needs to be reported for all uses of LMMs since they are underpinned by the same analytic engine.

The impact of researcher degrees of freedom in analysis choices is well documented. Gelman and Loken (2016) argued that analysis choices must be decided prior to data collection. Otherwise researchers erroneously combine analysis decision making with inferences about the data (i.e. analysis choices are conditional on the data themselves).

The more complex the analysis pipeline, the greater the possible number of analyses, and the greater the likelihood of widespread but undocumented variation in practice. Carp (2012b) documented over 6500 analysis pipelines for fMRI data and consequent variability in results, for example, for peak activation locations. However, some activations were stable across different analyses. And, more recently, studies have identified how the variation in data preparation and model building can be harnessed to clarify the stability or sensitivity of effects estimates to decision making choices at the dataset construction or at the analysis stages. Steegen et al. (2016) described multiverse analysis, in which all possible data sets are constructed from a sampling of the alternative ways in which raw data can be prepared for analysis (e.g., with variations on outlier exclusion, variable coding, exclusion of participants) and the analysis of interest is then performed across these data sets. P value plots then show how reliable effects are across these different data sets – showing the

robustness of results, and potential holes in theory or measurement. Patel, Burford and Ioannidis (2015) describe the "vibration of effects" or VoE which shows the variation in effect estimates across different models. This is particularly applicable to analysis of observational data sets in which there are many ways to specify models, and many possible variables or covariates of interest. VoE analysis shows how the influence of a variable changes across models and as more covariates are included (adjustment variables). Acknowledging the relative complexity of LMM analysis and the impact of researcher degrees of freedom, it would be natural to ignore a complex problem as insoluble. But the problem that we observe to be evident, here, as arbitrary variation in reporting and analytic practice is *not* insoluble. All the papers cited in this discussion offer a similar solution: when multiple analyses (and models) are possible, best practice is to report them all and to examine the variation in estimates as a test of the robustness of effects.

In the following sections, we present short discussions and recommendations for practice for the key areas highlighted by the survey and review results. Simmons et al. (2011) recommended that researchers should decide on stopping rules in advance (i.e. sample size), list all collected variables, report all conditions, report results with vs. without exclusions, and results with and without covariates. Our recommendations offer complementary guidance that will serve to likewise strengthen analytic and reporting practice. We offer, in Table 7, advice concerning best practice in reporting LMMs.

4.1 Preparation for using LMMs

A number of researchers are moving from using factorial designs analysed with ANOVA to factorial designs analysed with LMMs. As well as the issues we have already outlined, this is problematic for another reason: power. Researchers need to

plan specifically to power a study for a regression-like analysis (i.e. many stimuli, many data points) and the sample of experimental stimuli or trial types needs to be carefully considered. The sample size is important for power to detect experimental or observed effects (see below), but also because the computational engine (most often, maximum likelihood estimation) for LMMs assumes a large sample size (Maas & Hox, 2004; 2005). It will surprise no-one that power analysis for LMMs is complicated. This is principally because the hierarchies or groups identified in the data introduce multiple levels of sampling (Scherbaum & Ferreter, 2009; Snijders & Bosker, 1993; Snijders, 2005). There is the lowest level of sampling (the individual data points, say n=400) but also the groups (e.g. n = 20 participants, n = 20 items, and so on) and analysis may include effects or interactions within and across grouping levels. There is also the issue of what is being estimated. For LMMs, we can consider the power to accurately estimate fixed effect coefficients, random effect variances, averages for particular sampling units or interactions across those units (Scherbaum & Ferreter, 2009; Snijders, 2005).

Power in LMMs does not increase simply as the total sample increases. Data points within a grouping (e.g., trials for a given participant) may be more or less correlated. If this correlation (i.e. the intra-class correlation for a given grouping) is high, adding more individual data points for a grouping does not add more information (Scherbaum & Ferreter, 2009). In other words, if the responses across trials from a particular participant are highly correlated, the stronger explanatory factor is the participant, not the individual trials. Getting the participant to do more trials does not increase power. This also means that accurate power estimation for LMMs requires us to estimate or know the variation within and between sampling units, e.g. the intra-class correlation for trials within subjects (Snijders & Bosker, 1993; Scherbaum & Ferreter, 2009).

The general recommendation is to have as many sampling units as possible, since this is the main limitation on power (Snijders, 2005). Small or sparse numbers

of sampling units will mean that estimates are less reliable (e.g., underestimated standard errors, increased risk of false positive results; Bell et al, 2010; Maas & Hox, 2004; 2005). When looking across a range of simulation studies, Scherbaum & Ferreter (2009) concluded that increasing numbers for the sampling units is the best way to improve power (this held for the accuracy of estimating fixed effect coefficients, random effect variances and cross-level interactions). Small numbers are typically defined as fewer than 30 to 50 (Bell, Morgan, Kromery & Ferron, 2010; Maas & Hox, 2005; 2006). For psychological research, this means 30-50 participants, and 30-50 items or trials for each of those participants (i.e. a total sample of 900-2500 data points; Scherbaum & Ferreter, 2009). In line with this, Brysbaert & Stevens (2018) recommend a minimum of 40 participants and 40 items (1600 data points) to properly power psychological research studies, assuming typical effect sizes of 0.3-0.4.

Westfall, Kenny & Judd (2014) present a comprehensive analysis of power for crossed designs (where a group of participants respond to the same stimuli set; see also Brysbaert & Stevens, 2018). If we take a typical factorial experiment in psychology with 30 participants responding to 30 stimuli, power for a small effect size (0.2) is around 0.25 and for a medium effect size (0.5) is around 0.8 (see Figure 2 in Westfall, Kenny & Judd, 2014). To achieve a power of 0.95 for this number of participants and stimuli, you need a minimum effect size of around 0.6. Recall that 0.4 is a typical effect size for psychological studies (Brysbaert & Stevens, 2018). Adding more participants alone does not remedy this problem (Luke, 2016), as power asymptotes due to the variation in stimuli (Westfall, Kenny & Judd, 2014). This links back to the issue identified above: the higher level groupings (sampling units) in the data influence variation (responses for the same participant are correlated, responses for the same items are correlated) so ideally, the numbers for all sampling units should be increased. Ultimately, *this may change the design of the study*. Initial plans may have been to split a variable for a factorial design (e.g. high and low

frequency words). However, a sufficiently powered experiment would analyse frequency as a continuous variable, requiring many more stimuli to do so (see also Baayen, 2010; Cohen, 1983; Cohen, Cohen, Aiken, & West, 2003). Appendix 2 lists packages available for LMM power analysis.

It is our view that some issues with convergence are likely caused by researchers using LMMs to analyse relatively small sets of data. With smaller sets of data, it is less likely that a viable solution can be found to fit the proposed model to the data set. It is worth highlighting again that literature on mixed models defines 'small' as 50 or fewer sampling units (Bell et al., 2010; Maas & Hox, 2004; 2005). A researcher may be interested in the effect of frequency, testing this with 10 high frequency and 10 low frequency words. In an ANOVA, the participant average RT for the high and low frequency words would be calculated. In an LMM, this would be the coefficient for frequency (i.e. the average difference between frequency conditions across all participants and items). However, a random effect may also be fit to model how this effect *differs for each participant* (i.e. variation in the slope of frequency across participants). In this case, the model only has available 20 data points per participant (10 high and 10 low) and this may simply be insufficient data to complete the computation (Bates, Kliegl, Vasishth & Baayen, 2015). With more complex random effect structures (e.g., Barr et al., 2013) and perhaps no change in how researchers plan experiments, it is no wonder that convergence issues have become increasingly common. In short, plan to collect data for as many stimuli and as many participants as possible. Wherever possible, do not dichotomize continuous variables.

During analysis, researchers should check whether the assumptions of LMMs have been met. Researchers may choose to centre or standardize fixed effect predictors to manage collinearity and the scale on which different variables are measured; this can also help with interpretation of coefficients (see Field & Wright, 2011, for a nice summary). For LMMs, we take the same assumptions as for regression (linearity, random distribution of residuals, homoscedasticy; Maas & Hox,

2004; 2005). LMMs are used because the independence assumption is violated, that is, we know that data is grouped in some way, so observations from those groups are correlated. For LMMs, we assume that residual errors are normally distributed and random effects (variance associated with groupings) are normally distributed (Crawley, 2012: Field & Wright, 2011; Pinheiro & Bates, 2000; Snijders & Bosker, 2011). The simplest way to check these assumptions is to plot residuals and plot random effects. Note again that it is argued LMMs assume a large sample of data (Maas & Hox, 2004; 2005). If the assumptions of LMMs are not met, for example, due to violations of normality or small samples, standard errors can be underestimated and this can lead to false positive results (Maas & Hox, 2004). It has been shown that non-normally distributed random effects do not substantially effect the estimation of fixed effect coefficients but do affect the reliability of the variance estimates for the random effects themselves (Maas & Hox, 2004).

4.2 Selecting Random Effects

Gelman & Hill (2007) note that the definition of fixed and random effects varies across the literature and this makes it difficult to know when to used ‘fixed’ or ‘random’ elements during modelling. Random effects *vary* across sampling units and are expressed as standard deviations or variances. The variance-covariance structure of the model can be specified to model random effects as independent or related (e.g., successive trials within subjects will be correlated) and whether they have the same or different variances (Field & Wright, 2011). Researchers may choose to set these options for conceptual reasons (e.g., repeated measures designs imply autocorrelation between data points closer in time) or because it improves model fit.

It has been recommended that a maximal random effects structure (intercepts, slopes and interactions for subjects and items) should be used for confirmatory hypothesis testing (i.e. an experimental context, under the assumption that a set of predictor variables have been pre-defined; Barr et al, 2013). In this case, random effects are modelled for *all* experimental conditions and their interactions (e.g., how the effect of a given experimental condition changes across participants). This has been contrasted with exploratory analyses or data from observational studies, which justify the chosen random effects by testing different random effect structures and selecting those that improve model fit (Linck & Cunnings, 2015; Magezi, 2015).

Highly complex (sometimes called maximal) random effect structures may prevent the model from converging because the random effects specified in the model are not present in the data (Bates, Kliegl, Vasishth & Baayen, 2015; Matuschek et al, 2017). Convergence problems are mentioned in recent papers (Barr et al., 2013; Brauer & Curtin, 2018; Luke, 2016; Matuscheck et al., 2017) and they present another critical point of variation in analysis (see Eager & Roy, 2017). Decisions about how to handle convergence problems intersect with decisions about what target fixed and random effects are included in the model. Solutions to convergence problems may include the simplification of model structure (Matuscheck et al, 2017), using Principal Components Analysis to determine the most meaningful slopes (Bates et al., 2015), switching to alternate optimization algorithms (see comments by Bolker, 2015), or indeed to alternate programming languages or approaches (Bayes estimation, Eager & Roy, 2017). Brauer and Curtin (2017) provide a comprehensive list of options (n=20) to check through and possibly implement when convergence issues arise.

Matuscheck et al. (2017) demonstrated that models are more sensitive (in the detection of fixed effects) if random effects are specified according to whether LRT model comparisons warrant their inclusion, that is, according to whether or not the

random effect improved model fit. Whilst many saw the Barr et al. (2013) recommendations as a one-size-fits-all approach, the conclusion that we take from the literature is that random effects should be specified to the extent that they are defensible, justifiable and warranted by the data (i.e. from LRT or other model comparisons). Matuschek et al. (2017) contend that we cannot know in advance whether a random effect structure is *supported* by the data, and that in the long run, fitting models with random effects selected for better model fit means that the researcher can effectively manage both Type I and Type II error rates.

The focus and discussion about random effects reflects the novelty of this requirement for psychological research, and the conceptual and computational challenges involved: what effects can be specified? (Barr et al., 2013); what effects allow a model to converge? (Eager & Roy, 2017). Concerns with any one approach mandating how fixed and random effects are specified (e.g., led by experimental design and irrespective of data) are that it can lead to problems with convergence (Eager & Roy, 2017) and reduced sensitivity of analysis (increased Type II error rate; Bates et al., 2015; Matuscheck et al, 2017). More broadly however, it reflects a general point about model specification and selection – why should we want to build all models in the same way?

4.3 Model comparison and model selection

The process of model selection and comparison was an area of concern in the survey, and the focus of limited reporting practice in the literature reviewed. As highlighted above, the temptation (and the reality reflected in the literature) is for researchers to adhere to a prominent set of recommendations as *the* way to complete analysis. There is a widespread inertia that comes from putting data through factorial ANOVA and testing all possible main effects and interactions

(whether or not that is actually appropriate), and we believe that gives rise to a false belief that LMMS can be treated in the same way. LMMs are a *modelling* approach. Thus, as in comparable *modelling* approaches (e.g., growth curve modelling, structural equation modelling) there should be a clear statement of the criteria used when selecting model parameters (both fixed and random) and these should be principally driven by the research questions. In cases where multiple models are used or generated, these should be *comprehensively* reported. Multiple models can also then be used a way to test the reliability of effects. In an era of online publication, it is straightforward for appendices and supplementary materials to house this information. The provision of analysis scripts is one way this information can be easily provided.

In the context of testing data from an experimental design (e.g., the kind of factorial design that would traditionally be analysed using an ANOVA), it is sensible for the model to begin with the experimental conditions (see, e.g., Barr et al, 2013). Note that this parallels LMMs used with observational data (e.g., children in classes, classes in schools, schools in regions) where the simplest structure reflects these nested sampling groups (Gelman & Hill, 2007). There are numerous methods available for stepping through model comparisons, for example, by starting with a simple model and adding in more parameters, starting with a maximal or complex model and removing parameters, adding in control parameters first and then the predictors or effects of interest, and so on.

We think it would be misguided to identify an approach that is universally 'correct', since (as with much in statistics) whatever approach is used, it needs to be clearly justified. We should ask the question "What do you want to know from the data?" and "Given that, why have you taken the approach you have taken?". The result of presuming that one approach is correct may be that when LMM papers go through the review process, reviewers request an alternative modeling procedure. It would be more productive, for the field, if we acknowledge that the approach we take

is one choice given alternatives. The provision of analysis scripts and data with publication are a straightforward means to repeat or modify analyses if researchers (and reviewers) so wish. With the increasing use of pre-registration, researchers will specify in advance the modelling approach they will use. This may include an actual model to be fit, but at minimum it should include the dependent variable(s), fixed effects, covariates, the method for establishing the random effects structure and the method by which model selection will take place (e.g. simple to complex, covariates first etc.). To be truly comprehensive it should also have an a-priori power analysis (see section 4.1); this alone would mean the model (or alternative models) are well specified beforehand.

There are different methods available for model comparison (AIC, BIC, LRT) and these methods are not replacements for each other, i.e. they should be applied in different situations (Aho et al., 2014). In a situation where model comparison is necessary, the researcher has the option of using LRTs, AIC and BIC. Likelihood Ratio Tests (LRTs) apply when models are *nested* (i.e. all the terms of the smaller model appear in the larger model) and the models are being compared in a pairwise fashion (Aho et al., 2014). In the case of non-nested models, situations in which a large number of models are possible, or in cases where the 'null' model is of interest as a plausible model rather than something to be rejected, AIC and BIC are available. Aho et al. (2014) summarise that AIC (Akaike Information Criterion) is appropriate in situations where the 'reality' is that a very complex model has produced that data and we can never realistically expect to know or model all parameters in our statistical models. The assumption is that all the models are essentially wrong but predictive accuracy is valued, such as in weather forecasting. Alternatively, BIC (Bayesian Information Criterion) is appropriate when a relatively simple process has generated the data, and as our sample size increases we can expect to find the true model and confirm or falsify alternatives (i.e. a limited number of hypotheses are presented and one of these will be correct). In practice AIC and

BIC may provide the same conclusions about which model is best. Note that BIC lends itself to much of the work that is done in experimental psychology (controlled experiments, specified null, desire to find out which model is correct). It is an open question whether the situation is experimental psychology is in truth more akin to the first scenario.

A related issue is whether or not there needs to be corrections for multiple comparisons when multiple models are being compared using LRTs. If a complex model is being built and LRTs are being used at each step to judge the inclusion or exclusion of a particular factor, should there be an adjustment to the alpha level to reflect the volume of comparisons being made? It is our understanding that there is a risk of Type I errors occurring here. For any NHST procedure the simple fact of having a cut-off value for significance carries an associated risk of false-positives. We do not have an answer for this issue, but considered it worth raising.

4.4 Testing the significance of fixed effects

Computing degrees of freedom for NHST inferential tests in LMMs is a non-trivial problem (Baayen at al., 2008; Bates, 2006; Luke, 2016). For models with a hierarchical structure it is not clear how to define the denominator degrees of freedom (e.g., by number of observations, number of participants, number of random effects). As Luke (2016) notes, researchers may prefer to use model comparison with LRTs to evaluate the significance of a fixed effect predictor as this method does not require any computation or approximation for degrees of freedom. However, LRTs are only appropriate when the models to be compared are nested and have been fit with maximum likelihood (Luke, 2016; Pinheiro & Bates, 2000). The lme4 package in R (Bates et al, 2015) provides a summary guide to how p-values can be obtained for fitted models (search for help("pvalues") when lme4 is installed), with a

number of different options for confidence intervals, model comparison and two named methods for computing degrees of freedom (Kenward-Roger, Sattherwaite). Clearly, one reason why multiple methods for computing p-values appear in the literature is because there are a variety of software applications being used and these may or may not come with default computations that are available, but whose procedures are essentially hidden. There are two approaches to this problem.

The first potential approach is to choose the least worst method for obtaining p values. Luke (2016) used simulations to compare different methods for computing significance in LMMs. LRTs and t>1.96 (i.e. interpreting t as z) were anti-conservative, especially for small samples of participants and items (e.g., 12 to 36) and critically, this issue was independent of the total number of observations (you cannot compensate for small numbers of participants with large numbers of items). In our review of papers, LRTs and t-as-z approaches were the most common to be named in published manuscripts. Luke (2016) reports that the best performing methods were the Satterthwaite and Kenward-Rogers approximations when applied to models estimated with REML, these methods were also relatively robust across different samples sizes. Following Luke (2016), we recommend the use of these methods when p-values are needed for fixed effects.

The second option for dealing with p-values is to abandon them all together, in line with a now substantial body of work arguing for a change in how NHST and frequentist statistics are used. For example, reporting means or coefficient estimates and confidence intervals but not p values (Cumming, 2013a; 2013b). It is possible to abandon NHST all-together and fit models according to Bayesian inferential methods. Here, multiple models may be tested and pooled, rather than being compared. Another advantage of Bayesian methods is that the use of priors can resolve issues with model convergence (Eager & Roy, 2017). It is beyond the scope of this article to cover this in detail, but there are now several excellent texts available (McElreath, 2015; Kruschke, 2014; Eager & Roy, 2017). Regardless of whether

models are fit with frequentist or Bayesian methods, reporting of the modelling process needs to be entirely transparent.

4.5 Best practice

The standard for publication should be that other researchers can reproduce the study itself, as well as the study's results on the basis of the reported method, analysis approach and data (if available) (e.g., Open Science Collaboration, 2015). It is our judgment that many of the issues identified arise because of 'under-reporting' – that is, insufficient information provided in publications on the analysis steps (Gelman & Loken, 2013; Silberzahn & Uhlmann, 2015; Simmons et al., 2011) and for LMMs more specifically, incomplete reporting of model results. Following the summary above, Table 7 provides best practice guidance for the reporting of LMMs. For specific guidance on Generalised Linear Mixed-effects Models (GLMMs), we refer readers to Bolker et al., (2009). We have been asked what to do about the extensive documentation required by what we see as best practice, comprehensive, reporting. The simple solution is for researchers to share their data analysis scripts with publication. Scripts show exactly what decisions have been taken and exactly how models were selected and compared. When provided with data, they allow any other researcher to replicate entirely the reported results. Knowing in advance that an analysis script will be shared on publication will likely make researchers more systematic and attentive to their code and annotations in the first place. It should also encourage more supportive discussion (rather than criticism) around analysis processes and best practice methods, and give the less experienced an easy way to learn from experts.

Table 7: Best practice guidance for reporting LMMs

Issue	Recommendation
The model	
Assumptions of LMM	Report what data cleaning has been completed, outlier/data removal, transformations (e.g., centering or standardizing variables) or other changes prior to or following analysis (e.g., Baayen & Milin, 2015). Report the sample size entered into model in terms of total number of data points and various sampling units (e.g., number of participants, number of items, number of other groups specified as random effects). Report whether models meet assumptions for LMMs.
Selection of fixed and random effects.	Fixed effects and covariates are specified from explicitly stated research questions and/or hypotheses. Random effects are explicitly specified according to sampling units (e.g., participants, items), the data structure (e.g., repeated measures) and anticipated interactions between fixed effects and sampling units (e.g., intercepts only or intercepts and slopes).
Model comparison*	A clear statement of the methods by which models are compared/selected; e.g., simple to complex, covariates first, random effects first, fixed effects first etc. Report comparison method (LRT, AIC, BIC) and justify the choice (Aho et al., 2014). A complete report of all models compared (e.g., in appendices/supplementary data/analysis scripts) with model equations and the result of comparisons. For an

	example table reporting model comparisons see Appendix Table A5.1.
Convergence issues	If models fail to converge, simplification of models is comprehensively reported (see section 4.2 above), to include each model equation that did or did not converge and a rationale for a) the simplification method used and b) the final model reported. This may be most easily presented in an analysis script.
The results	
Model*	Provide equation(s) that transparently define the reported model(s). An elegant way to do this is providing the model equation with the table that reports the model output (see Appendix Table A5.2).
Model output*	Final model(s) reported in a table that includes all parameter estimates for fixed effects (coefficients, standard errors and/or confidence intervals, associated test statistics and p values if used), random effects (standard deviation and/or variance for each random effect, correlations/covariances if modelled) and some measure of model fit (e.g. R-squared, correlation between fitted values and data) (see Appendix Table A5.2).
Data and code	Share coding script used to complete the analysis. Wherever possible share data that generated the reported results.

* Example tables here are adapted from the excellent examples in Stevenson et al., 2013 (Table 2), Goldhammer et al., 2014 (Table 1) and Li et al., 2014.

5.0 Conclusion

We completed a survey of current practice and a review of published papers for LMMs. Concerns raised in the survey were broadly corroborated by data from a review of published papers. In response to this, we have reviewed current guidelines for the implementation and reporting of LMMs, and provided a summary of best practice. The survey highlighted that many researchers felt they had a lack of knowledge, or were unable to properly deal with the complexity of LMMs. We hope this paper has gone some way to remedying this deficit (perceived or real), and encouraging researchers to spend time preparing analyses in a such a way that fully transparent reporting is painless.

6.0 References

Aarts, E., Verhage, M., Veenvliet, J. V., Dolan, C. V., & van der Sluis, S. (2014). A solution to dependency: using multilevel analysis to accommodate nested data. Nature neuroscience, 17(4), 491-496.

Aho, K., Derryberry, DW & Peterson, T. (2014) Model selection for ecologists: the worldview of AIC and BIC, Ecology, 95(3), 631-636.

Baayen, R. H. (2008). Analyzing linguistic data: A practical introduction to statistics using R. Cambridge University Press.

Baayen, R. H. (2010). A real experiment is a factorial experiment? The Mental Lexicon, 5(1), 149-157.

Baayen, R. H., Davidson, D. J., & Bates, D. M. (2008). Mixed-effects modeling with crossed random effects for subjects and items. Journal of Memory and Language, 59(4), 390-412.

Baayen, R.H. (2013). languageR: Data sets and functions with "Analyzing Linguistic Data: A practical introduction to statistics". R package version 1.4.1. http://CRAN.R-project.org/package=languageR

Baayen, R. H., & Milin, P. (2015). Analyzing reaction times. International Journal of Psychological Research, 3(2), 12-28.

Balota, D. A., Cortese, M. J., Sergent-Marshall, S. D., Spieler, D. H., & Yap, M. J. (2004). Visual word recognition of single-syllable words. Journal of Experimental Psychology: General, 133(2), 283.

Barr, D. J., Levy, R., Scheepers, C., & Tily, H. J. (2013). Random effects structure for confirmatory hypothesis testing: Keep it maximal. Journal of Memory and Language, 68(3), 255-278.

Bates, D.M. (2006) [R] lmer, p-values and all that. Post on the R-help mailing list, May 19th, available at: https://stat.ethz.ch/pipermail/r-help/2006-May/094765.html

Bates, D. M. (2007). Linear mixed model implementation in lme4. Manuscript, university of Wisconsin - Madison, January 2007.

Bates, D., Maechler, M., Bolker, B. & Walker, S. (2015). Fitting Linear Mixed-Effects Models Using lme4. Journal of Statistical Software, 67(1), 1-48. doi:10.18637/jss.v067.i01.

Bates, D., Kliegl, R., Vasishth, S., & Baayen, H. (2015). Parsimonious mixed models. arXiv preprint arXiv:1506.04967.

Bell, B.A., Morgan, G.B., Kromery, J.D. & Ferron, J.M. (2010) The Impact of Small Cluster Size on Multilevel Models: A Monte Carlo Examination of Two-Level Models with Binary and Continuous Predictors. JSM.

Bickel, R. (2007). Multilevel analysis for applied research: It's just regression!. Guilford Press.

Boisgontier, M. P., & Cheval, B. (2016). The anova to mixed model transition. Neuroscience & Biobehavioral Reviews, 68, 1004-1005.

Bolker, B. M., Brooks, M. E., Clark, C. J., Geange, S. W., Poulsen, J. R., Stevens, M. H. H., & White, J. S. S. (2009). Generalized linear mixed models: a practical guide for ecology and evolution. Trends in Ecology & Evolution, 24, 127-135.

Bolker, B. (2015, March 14). GLMM. Retrieved August 01, 2016, from http://glmm.wikidot.com/faq

Bowen, N. K., & Guo, S. (2011). Structural equation modeling. Oxford University Press.

Brauer, M., & Curtin, J. J. (2017, November 27). Linear Mixed-Effects Models and the Analysis of Nonindependent Data: A Unified Framework to Analyze Categorical

and Continuous Independent Variables that Vary Within-Subjects and/or Within-Items. Psychological Methods.

Braun, V., & Clarke, V. (2006). Using thematic analysis in psychology. Qualitative Research in Psychology, 3(2), 77-101.

Bryk, A. S., & Raudenbush, S. W. (1992). Hierarchical linear models: applications and data analysis methods. London, UK: Sage.

Brysbaert, M. (2007). The language-as-fixed-effect-fallacy: Some simple SPSS solutions to a complex problem. London: Royal Holloway, University of London.

Burstein, L., Miller, M.D., & Linn, R.L. (1981). The Use of Within-Group Slopes as Indices of Group Outcomes. Center for the Study of Evaluation, Graduate School of Education, UCLA, Los Angeles California. CSE Report 171.

Carp, J. (2012a). The secret lives of experiments: methods reporting in the fMRI literature. Neuroimage, 63(1), 289-300.

Carp, J. (2012b). On the plurality of (methodological) worlds: estimating the analytic flexibility of fMRI experiments. Frontiers in Neuroscience, 6, 149.

Chabris, C.F., Hebert, B.M., Benjamin, D.J., Beauchamp, J., Cesarini, D., van der Loos, M., Johannesson, M., Magnusson, P.K.E., Lichtenstein, P., Atwood, C.S., Freese, J., Hauser, T.S., Hauser, R.M., Christakis, N. & Laibson, D. (2012). Most reported genetic associations with general intelligence are probably false positives. Psychological Science, 1(23), 1314-1323.

Chang, Y-H. & Lane, D.M. (2016) Generalizing across stimuli as well as subjects: A non-mathematical tutorial on mixed-effects models. The Quantitative Methods for Psychology, 12, no. 3 (2016) 201-219. 10.20982/tqmp.12.3.p201 .

Clark, H. H. (1973). The language-as-fixed-effect fallacy: A critique of language statistics in psychological research. Journal of verbal learning and verbal behavior, 12(4), 335-359.

Cohen, J. (1983). The Cost of Dichotomization. Applied Psychological Measurement 7(3), pp249-253

Cohen, J., Cohen, P., West, S. G., & Aiken, L. S. (2003). Applied multiple correlation/regression analysis for the behavioral sciences. UK: Taylor & Francis.

Coleman, E. B. (1964). Generalizing to a language population. Psychological Reports, 14(1), 219-226.

Crawley, M. J. (2012). The R book. John Wiley & Sons.

Cumming, G. (2013a). Understanding the new statistics: Effect sizes, confidence intervals, and meta-analysis. Routledge.

Cumming, G. (2013b). The new statistics why and how. Psychological science, 0956797613504966.

Cunnings, I. (2012) An overview of mixed-effects statistical models for second language researchers. Second Language Research, 28 (3), 369-382.

Davies, R. A. I., Arnell, R., Birchenough, J., Grimmond, D., & Houlson, S. (2017). Reading Through the Life Span: Individual Differences in Psycholinguistic Effects. Journal of Experimental Psychology: Learning, Memory, and Cognition.

Eager, C., & Roy, J. (2017). Mixed effects models are sometimes terrible. arXiv preprint arXiv:1701.04858.

Field, A., Miles, Z. & Field, Z. (2009). Discovering statistics using R. Sage publications, London.

Field, A. & Wright, D.B. (2011) A Primer on Using Multilevel Models in Clinical and Experimental Psychopathology Research. Journal of Experimental Psychopathology, 2(2), 271-293.

Frisson, S., Koole, H., Hughes, L., Olson, A., & Wheeldon, L. (2014). Competition between orthographically and phonologically similar words during sentence reading: Evidence from eye movements. Journal of Memory and Language, 73, 148-173.

Gelman, A. (2014). The Connection Between Varying Treatment Effects and the Crisis of Unreplicable Research: A Bayesian Perspective. Journal of Management, DOI: 0149206314525208.

Gelman, A., & Hill, J. (2007). Data Analysis Using Regression and Multilevel/Hierarchical Models. Cambridge University Press, Cambridge, UK.

Gelman, A., & Loken, E. (2013). The garden of forking paths: Why multiple comparisons can be a problem, even when there is no “fishing expedition” or “p-hacking” and the research hypothesis was posited ahead of time. Department of Statistics, Columbia University.

Goldhammer, F., Naumann, J., Stelter, A., Tóth, K., Rölke, H., & Klieme, E. (2014). The time on task effect in reading and problem solving is moderated by task difficulty and skill: Insights from a computer-based large-scale assessment. Journal of Educational Psychology, 106(3), 608.

Goldstein, H. (2011). Multilevel statistical models (Vol. 922). John Wiley & Sons.

Hox, J. (2010). Multilevel analysis: Techniques and applications. New York, NY: Routledge.

IBM Corp (2013 release). IBM SPSS Statistics for Windows, Version 22.0. Armonk, NY: IBM Corp.

Ioannidis, J. P. (2005). Why most published research findings are false. Chance, 18(4), 40-47.

Jaeger, T. F. (2008). Categorical data analysis: Away from ANOVAs (transformation or not) and towards logit mixed models. Journal of Memory and Language, 59, 434 – 446.

Jaeger, T. F., Graff, P., Croft, W., & Pontillo, D. (2011). Mixed effect models for genetic and areal dependencies in linguistic typology. Linguistic Typology, 15(2), 281-320.

JASP Team (2016). JASP (Version 0.8.0.0) [Computer software].

Judd, C. M., Westfall, J., & Kenny, D. A. (2012). Treating stimuli as a random factor in social psychology: a new and comprehensive solution to a pervasive but largely ignored problem. Journal of personality and social psychology, 103, 54.

Kliegl, R., Wei, P., Dambacher, M., Yan, M., & Zhou, X. (2011). Experimental effects and individual differences in linear mixed models: Estimating the relationship between spatial, object, and attraction effects in visual attention. Frontiers in Psychology, 1, 238.

Kliegl, R. (2014). Reduction of complexity of linear mixed models with double-bar syntax. RPubs.com/Reinhold/22193

Kreft, I.G., & de Leeuw, J. (1998). Introducing multilevel modeling. London, UK: Sage.

Kriegeskorte, N., Simmons, W. K., Bellgowan, P. S., & Baker, C. I. (2009). Circular analysis in systems neuroscience: the dangers of double dipping. Nature Neuroscience, 12(5), 535-540.

Kruschke, J. (2014). Doing Bayesian data analysis: A tutorial with R, JAGS, and Stan. Academic Press.

Kuznetsova, A., Brockhoff, P.B. & Christensen, R.H.B. (2016). lmerTest: Tests in Linear Mixed Effects Models. R package version 2.0-30. http://CRAN.R-project.org/package=lmerTest

Li, X., Bicknell, K., Liu, P., Wei, W., & Rayner, K. (2014). Reading is fundamentally similar across disparate writing systems: A systematic characterization of how words and characters influence eye movements in Chinese reading. *Journal of Experimental Psychology: General*, *143*(2), 895.

Lieberman, M. D., & Cunningham, W. A. (2009). Type I and Type II error concerns in fMRI research: re-balancing the scale. Social cognitive and affective neuroscience, 4(4), 423-428.

LimeSurvey Project Team & Schmitz, C. (2015) LimeSurvey: An Open Source survey tool /LimeSurvey Project Hamburg, Germany. URL http://www.limesurvey.org

Linck, J. A., & Cunnings, I. (2015). The Utility and Application of Mixed‐Effects Models in Second Language Research. Language Learning, 65(S1), 185-207.

Locker, L., Hoffman, L., & Bovaird, J. A. (2007). On the use of multilevel modeling as an alternative to items analysis in psycholinguistic research. Behavior Research Methods, 39(4), 723-730.

Lorch, R. F., & Myers, J. L. (1990). Regression analyses of repeated measures data in cognitive research. Journal of Experimental Psychology: Learning, Memory, and Cognition, 16(1), 149.

Luke, S. G. (2016). Evaluating significance in linear mixed-effects models in R. Behavior Research Methods, 1-9.

Maas, C.J.M. & Hox, J.J. (2004) The influence of violations of assumptions on multilevel parameter estimates and their standard errors. Computational Statistics & Data Analysis, 46, 427-440.

Maas, C.J.M. & Hox, J.J. (2005) Sufficient Sample Sizes for Multilevel Modeling. Methodology, 1(3), 86-92.

Magezi, D. A. (2015). Linear mixed-effects models for within-participant psychology experiments: an introductory tutorial and free, graphical user interface (LMMgui). Frontiers in psychology, 6.

MATLAB and Statistics Toolbox Release 2012b, The MathWorks, Inc., Natick, Massachusetts, United States.

Matuschek, H., Kliegl, R., Vasishth, S., Baayen, H., & Bates, D. (2017). Balancing Type I error and power in linear mixed models. Journal of Memory and Language, 94, 305-315.

McCoach, B.D., Rifenbark, G.G., Newton, S.D., Xiaoran, L., Kooken, J., Yomtov, D., Gambino, A.J., & Bellara, A. (2018) Does the package matter? A comparison of five common multilevel modeling software packages. Journal of Educational and Behavioral Statistics 43 (5), 594-627.

McElreath, R. (2015). Statistical rethinking: A Bayesian course with examples in R and Stan. Chapman and Hall/CRC.

Muthén, L. K., & Muthén, B. O. (2011). Mplus User's Guide. Sixth Edition. Los Angeles, CA.

Murayama, K., Sakaki, M., Yan, V. X., & Smith, G. M. (2014). Type I error inflation in the traditional by-participant analysis to metamemory accuracy: A generalized mixed-effects model perspective. Journal of Experimental Psychology: Learning, Memory, and Cognition, 40(5), 1287.

Nagin, D.S. & Odgers, C.L. (2010) Group-Based Trajectory Modeling in Clinical Research. Annual Review of Clinical Psychology 6, 109-138.

Open Science Collaboration (2015). Estimating the reproducibility of psychological science. Science, 349(6251), aac4716.

Pashler, H., & Wagenmakers, E. J. (2012). Editors' Introduction to the Special Section on Replicability in Psychological Science A Crisis of Confidence? Perspectives on Psychological Science, 7(6), 528-530.

Patel, C. J., Burford, B., & Ioannidis, J. P. (2015). Assessment of vibration of effects due to model specification can demonstrate the instability of observational associations. Journal of Clinical Epidemiology, 68(9), 1046-1058.

Pinheiro, J. C., & Bates, D. M. (2000). Theory and computational methods for linear mixed-effects models. Mixed-Effects Models in S and S-PLUS. New York, NY: Springer-Verlag.

Pinheiro, J., Bates, D., DebRoy, S., Sarkar, D. & R Core Team (2016). _nlme: Linear and Nonlinear Mixed Effects Models_. R package version 3.1-128, URL: http://CRAN.R-project.org/package=nlme.

Poldrack, R. A., & Gorgolewski, K. J. (2014). Making big data open: data sharing in neuroimaging. Nature Neuroscience, 17(11), 1510.

R Core Team (2017). R: A language and environment for statistical computing. R Foundation for Statistical Computing, Vienna, Austria. URL https://www.R-project.org/.

Raaijmakers, J. G. (2003). A further look at the" language-as-fixed-effect fallacy". Canadian Journal of Experimental Psychology/Revue canadienne de psychologie expérimentale, 57(3), 141.

Raaijmakers, J. G., Schrijnemakers, J. M., & Gremmen, F. (1999). How to deal with “the language-as-fixed-effect fallacy”: Common misconceptions and alternative solutions. Journal of Memory and Language, 41(3), 416-426.

Rasbash, J., Charlton, C., Browne, W.J., Healy, M. and Cameron, B. (2009) MLwiN Version 2.1. Centre for Multilevel Modelling, University of Bristol.
Rabe-Hesketh, S. and Skrondal, A. (2012). Multilevel and Longitudinal Modeling Using Stata (3rd Edition). College Station, TX: Stata Press.

Rasbash, J., Browne, W., Goldstein, H., Yang, M., Plewis, I., Healy, M., Woodhouse, G., Draper, D., Langford, I. & Lewis, T. (2000). A user’s guide to MLwiN. London: Institute of Education, 286.

Rietveld, T., & van Hout, R. (2007). Analysis of variance for repeated measures designs with word materials as a nested random or fixed factor. Behavior research methods, 39(4), 735-747.

Rossini, A. J., Heiberger, R. M., Sparapani, R. A., Maechler, M., & Hornik, K. (2004). Emacs speaks statistics: A multiplatform, multipackage development environment for statistical analysis. Journal of Computational and Graphical Statistics, 13(1), 247-261. URL: https://ess.r-project.org/

Scherbaum, C.A., & Ferreter, J.M. (2009) Estimating Statistical Power and Required Sample Sizes for Organisational Research Using Multilevel Modeling. Organizational Research Methods, 12(2), 347-367.

Schluter, D. (2015). Fit models to data. Retrieved August 1, 2016, from https://www.zoology.ubc.ca/~schluter/R/fit-model/

Silberzahn, R., & Uhlmann, E. L. (2015). Many hands make tight work. Nature, 526(7572), 189.

Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology undisclosed flexibility in data collection and analysis allows presenting anything as significant. Psychological science, 0956797611417632.

Snijders, T.A. (2005) Power and Sample Size in Multilevel Linear Models. In: B.S. Everitt and D.C. Howell (eds.), Encyclopedia of Statistics in Behavioral Science. Volume 3, 1570–1573. Chices- ter (etc.): Wiley.

Snijders, T.A., & Bosker, R.J. (2011). Multilevel analysis (2nd Edition). London, UK: Sage.

Snijders, T.A., & Bosker, R.J. (1993) Standard Errors and Sample Sizes for Two-Level Research. Journal of Educational Statistics, 18(3), 237-259.

Stan Development Team (2016). Stan Modeling Language Users Guide and Reference Manual, Version 2.14.0. http://mc-stan.org

Steegen, S., Tuerlinckx, F., Gelman, A., & Vanpaemel, W. (2016). Increasing transparency through a multiverse analysis. Perspectives on Psychological Science, 11(5), 702-712.

Stevenson, C. E., Hickendorff, M., Resing, W. C., Heiser, W. J., & de Boeck, P. A. (2013). Explanatory item response modeling of children's change on a dynamic test of analogical reasoning. *Intelligence*, *41*(3), 157-168.

Th. Gries, S. (2015). The most under-used statistical method in corpus linguistics: multi-level (and mixed-effects) models. Corpora, 10(1), 95-125.

Tremblay, A., & Newman, A. J. (2015). Modeling nonlinear relationships in ERP data using mixed‐effects regression with R examples. Psychophysiology, 52(1), 124-139.

Trueswell, J. C., Medina, T. N., Hafri, A., & Gleitman, L. R. (2013). Propose but verify: Fast mapping meets cross-situational word learning. Cognitive psychology, 66(1), 126-156.

Venables, W. N. 2014. S-PLUS and S. Wiley StatsRef: Statistics Reference Online.

Vul, E., Harris, C., Winkielman, P., & Pashler, H. (2009). Puzzlingly high correlations in fMRI studies of emotion, personality, and social cognition. Perspectives on psychological science, 4(3), 274-290.

Wager, T. D., Lindquist, M., & Kaplan, L. (2007). Meta-analysis of functional neuroimaging data: current and future directions. Social cognitive and affective neuroscience, 2(2), 150-158.

Walls, T. A., & Schafer, J. L. (). (2006). Models for intensive longitudinal data. New York, NY: Oxford University Press.

West, B. T., & Galecki, A. T. (2011) An overview of current software procedures for fitting linear mixed models. The American Statistician, 65(4), 274-282.

Westfall, J., Kenny, D. A., & Judd, C. M. (2014). Statistical power and optimal design in experiments in which samples of participants respond to samples of stimuli. Journal of Experimental Psychology: General, 143(5), 2020.

Winter, B. (2013). Linear models and linear mixed effects models in R with linguistic applications. arXiv preprint arXiv:1308.5499.

Wood, S.N. (2011) Fast stable restricted maximum likelihood and marginal likelihood estimation of semiparametric generalized linear models. Journal of the Royal Statistical Society (B) 73(1):3-36

Wood, S. N. & Scheipl, F. (2016). gamm4: Generalized Additive Mixed Models using 'mgcv' and 'lme4'. R package version 0.2-4. http://CRAN.R-project.org/package=gamm4

Wright, D. B., & Villalba, D. K. (2012). Memory conformity affects inaccurate memories more than accurate memories. Memory, 20(3), 254-265.