Does Preliminary Model Checking Help With Subsequent Inference? A Review And A New Result

M. Iqbal Shamsudheen and Christian Hennig

1, 2 Department of Statistical Science, University College London, Gower Street, London, WC1E 6BT, United Kingdom,
E-mail: iqbal.shamsudheen.14@ucl.ac.uk,
2 Dipartimento di Scienze Statistiche, Universita di Bologna, Via delle belle Arti, 41, 40126 Bologna, Italy,
E-mail: christian.hennig@unibo.it

Summary

Statistical methods are based on model assumptions, and it is statistical folklore that a method’s model assumptions should be checked before applying it. We review literature that investigated combined test procedures, in which model assumptions are checked first. Then, in case that the model assumption is passed, a test based on the model assumption is run, and otherwise a test with less strong assumptions. Much literature is surprisingly critical of this approach, owing also to the observation that conditionally on passing a model misspecification test, the model assumptions are automatically violated (“misspecification paradox”). We also review controversial views on the role of model checking in statistics, and literature investigating empirically to what extent model assumptions are checked in practice. We suspect that the benefit of preliminary model checking is currently underestimated, and we present a general setup not yet investigated in the literature, in which we can show that preliminary model checking is advantageous.

Key words: Misspecification testing; Hypothesis test; Goodness of fit; Combined procedure; Misspecification paradox.

1 Introduction

Statistical methods are based on model assumptions, and it is statistical folklore that a method’s model assumptions should be checked before applying it. Yet there is surprisingly little agreement in the literature about how to do this. Model checking is ignored in much applied work. As will be seen later, several authors who investigated the statistical characteristics of running model checks before applying a model-based method comment rather critically on it. So is it sound ad-
vice to check model assumptions first? We shed some light on the issue by reviewing literature in which such an approach is investigated. We cannot attempt to achieve completeness, because the amount of literature on certain specific problems that belong to this scope is quite large, see, e.g., Bancroft and Han (1977) for what was already available at that time (if with somewhat limited scope). We therefore have to restrict our focus and will concentrate on the problem of two-stage testing, i.e., hypothesis testing conditionally on the result of preliminary tests of model assumptions. More work exists on estimation after preliminary testing, for overviews see Giles and Giles (1993), Chatfield (1995), Saleh (2006). Some work investigating current practice regarding model checking is also reviewed. The issue is connected to some recent discussions regarding the foundations of statistics, on which we reflect. We also present a new theoretical result concerning a combined procedure in which a final model-based or an alternative test (we focus on tests as an example for more general methods of inference) is chosen depending on whether a misspecification test passes or rejects the model assumption. A situation is constructed in which the combined procedure improves on the unconditional use of any of the two final analyses.

To fix terminology, we assume a situation in which a researcher is interested in using a “main test” for testing a main hypothesis that is of substantial interest. There is a “model-based constrained (MC) test” involving certain model assumptions available for this. We will call “misspecification (MS) test” a test with the null hypothesis that a certain model assumption holds. We assume that this is not of primary interest, but rather only done in order to assess the validity of the model-based test, which is only carried out in case that the MS test does not reject (or “passes”) the model assumption. In case that the MS test rejects the model assumption, there may or may not be an “alternative unconstrained (AU) test” that the researcher applies, which does not rely on the rejected model assumption, in order to test the main hypothesis. A “combined procedure” consists of the complete decision rule involving MS test, MC test and AU test (if specified). We generally assume that the MS test is carried out on the same data as the main test. Some of the issues discussed below can be avoided by checking the model on independent data, however such data may not be available, or this approach may not be preferred for reasons of potential waste of information and lack of power (in case the “independent” data are obtained by splitting the available dataset, see Chatfield (1995) for a discussion of this). In any case it would leave open the question whether the data used for MS testing are really independent of the data used for the main test, and whether they do really follow the same model.

Note that the main hypothesis may originally have been defined in terms of the tested model, and may require a re-definition in case that this is rejected (for example, a hypothesis on the center of a Gaussian distribution may translate into a hypothesis about a mean, median, or mode in a more general nonparametric family of models). We acknowledge that researchers may in fact apply more than one MS test to check various model assumptions, and may have more than one alternative model or test available depending on the results of these (see, e.g., Spanos (2018)). For the sake of simplicity, and because there is hardly any literature investigating the performance of more complex combined procedures, we mostly stick to the situation in which only one MS test is performed.

Furthermore, chances are that informal approaches are used far more often, i.e., researchers may do some informal or formal model assumption checking, and may only decide how to proceed knowing the outcome of the model check, rather than using a combined procedure that was well defined in advance. Such a behavior obviously cannot be formally investigated. Formally defined combined procedures may serve as some kind of “formal model” for this course of action,
informing us to some extent about advantages and disadvantages, while acknowledging that they cannot cover all the options open to a real researcher. On the other hand, there is a demand for fully formally defined procedures that allow a researcher to avoid supposedly subjective decisions. The simple rule “use MS test to check whether the model assumption for the MC test is fulfilled; if the assumption is passed, apply MC test, otherwise apply AU test” looks appealing to many (as one of the authors knows from many years of experience as a statistical advisor).

Section 2 introduces MS testing for model checking. Section 3 formally introduces a combined procedure in which an MS test is used to decide between an MC and an AU main test. Section 4 reviews the controversial discussion of the role of model checking and testing in statistics. Section 5 reviews work that investigated the use of model checking and misspecification tests in practical statistics. Section 6 runs through the literature that investigated the impact of misspecification testing and the performance of combined procedures in various scenarios. In Section 7 we present a new result that formalizes a situation in which a combined procedure can be better than both the MC and the AU test. Section 8 provides the conclusion.

2 Misspecification testing and checking model assumptions

Even though two of the most prominent statisticians that introduced the idea of statistical hypothesis testing, Fisher and Neyman, had differences, one thing they agreed on is that checking the assumptions comprising the statistical model in order to ensure its adequacy is essential. Fisher (1922) stated:

> For empirical as the specification of the hypothetical population may be, this empiricism is cleared of its dangers if we can apply a rigorous and objective test of the adequacy with which the proposed population represents the whole of the available facts. Once a statistic, suitable for applying such a test, has been chosen, the exact form of its distribution in random samples must be investigated, in order that we may evaluate the probability that a worse fit should be obtained from a random sample of a population of the type considered.

Neyman (1952) outlined the construction of a mathematical model in which he emphasized testing the assumptions of the model by observation and if the assumptions are satisfied, then the model “may be used for deductions concerning phenomena to be observed in the future”.

The idea of MS testing came about as early as the early 20th century when Pearson introduced the Pearson’s goodness of fit chi-square test. This is an MS test for the adequacy of a distributional assumption (the term “goodness of fit test” has a longer history than the term “misspecification test” but we will use the latter term here). The term misspecification was only coined as late as Fisher (1961) for the selection of exogenous variables in economic models. Spanos (1999) used the term extensively and discussed many aspects of the role of MS testing for specifying and validating statistical models, and how to proceed when certain statistical assumptions are violated. We do not attempt to review model misspecification testing exhaustively; for this see Spanos (2018) and the references given there. However, here are a few examples:

> Testing assumptions regarding distributional shape: The probably oldest test of a given distributional shape is Pearson’s chi-square test. There are also tests based on the empirical cumulative distribution function such as the Kolmogorov’s test and the Cramer-Von Mises statistic. These
tests quantify the difference of the distances between the observed data and the hypothesized distribution. Another family of tests are those based on ordered samples. An example of tests of this kind include the Shapiro-Wilk test for testing normality. One can also carry out an MS test based on the moments using properties of the skewness and kurtosis coefficients, for instance the skewness-kurtosis test given by Fisher (1930).

Testing independence assumptions: Some non-parametric tests can be used to test the independence assumption, for example the runs test first presented in Wald and Wolfowitz (1940). Another approach is moment based tests, for example see Box and Pierce (1970) and Ljung and Box (1978).

Testing homogeneity of variance assumptions: Some early non-parametric tests for the homogeneity assumption were based on the signs of the differences, for example as proposed by Mann (1945) and Daniels (1950). Examples of parametric tests include the $\chi^2$-test, the $F$-test and the Levene’s test (see Levene, 1960).

2.1 Graphical methods for checking model assumptions

Informal graphical assessments such as certain scatter plots for independence, others for constant variance and normal quantile-quantile plots for the adequacy of the Gaussian model are often recommended to check the assumptions of a particular main test, for example, a Student’s $t$-test; testing the validity of a regression model by way of residual plots is treated in many textbooks.

Graphical displays have the advantage that they can give the statistician more insight about the nature of potential violations of model assumptions and appropriateness of alternative models and methods than formal MS tests. On the other hand, decisions based on graphical displays do not follow prespecified formal decision rules, and therefore the statistical characteristics of combined procedures involving graphical misspecification detection cannot be investigated by theory or simulation. It is conceivable to “translate” certain decisions based on graphical displays into formal rules and analyze them, although to our knowledge this has not happened in the literature yet. It may also be seen as incorrect, because graphical displays are used for stimulating the intuition better than formal rules, so to force graphical decisions into formal rules would not appropriately reflect their actual use. However, the issues with formal MS testing as elaborated below also occur if graphical models are used to check model assumptions, even though cannot be formally analyzed.

3 Combined procedures

The general setup we are interested in here is as follows. Given is a statistical model defined by some model assumptions $\Theta$,

$$M_\Theta = \{P_\theta, \theta \in \Theta\} \subset M,$$

where $P_\theta, \theta \in \Theta$ are distributions over a space of interest, indexed by a parameter $\theta$. $M_\Theta$ is written here as a parametric model, but we are not restrictive about the nature of $\Theta$. $M_\Theta$ may even be the set of all i.i.d. models for $n$ observations, in which case $\Theta$ would be very large. However, in the literature, $M_\Theta$ is usually a standard parametric model with $\Theta \subseteq \mathbb{R}^m$ for some $m$. There is a bigger model $M$ containing distributions that do not require one or more assumptions made in $M_\Theta$, but for data from the same space.
Given some data \( z \), we want to test a parametric null hypothesis \( \theta \in \Theta_0 \), which has some suitably chosen “extension” \( M^* \subset M \) so that \( M^* \cap M_\theta = M_{\Theta_0} \), against the alternative \( \theta \notin \Theta_0 \) corresponding to \( M \setminus M^* \) in the bigger model.

In the simplest case, there are three tests involved, namely the MS test \( \Phi_{MS} \), the MC test \( \Phi_{MC} \) and the AU test \( \Phi_{AU} \). Let \( \alpha_{MS} \) be the level of \( \Phi_{MS} \), i.e., \( Q(\Phi_{MS}(z) = 1) \leq \alpha_{MS} \) for all \( Q \in M_\theta \). Let \( \alpha \) be the level of the two main tests, i.e., \( P_\theta(\Phi_{MC}(z) = 1) \leq \alpha \) for all \( P_\theta, \theta \in \Theta_0 \) and \( Q(\Phi_{AU}(z) = 1) \leq \alpha \) for all \( Q \in M^* \). To keep things general, for now we do not assume that type I error probabilities are uniformly equal to \( \alpha_{MS} \), \( \alpha \), respectively, and neither do we assume tests to be unbiased (which may not be realistic considering a big nonparametric \( M \)).

The combined test is defined as

\[
\Phi_C(z) = \begin{cases} 
\Phi_{MC}(z) : & \Phi_{MS}(z) = 0, \\
\Phi_{AU}(z) : & \Phi_{MS}(z) = 1.
\end{cases}
\]

This allows to analyze the characteristics of \( \Phi_C \), particularly its effective level (which is not guaranteed to be \( \leq \alpha \)) and power under \( P_\theta \) with \( \theta \in \Theta_0 \) or not, or under distributions from \( M^* \) or \( M \setminus M^* \). General results are often hard to obtain without making restrictive assumptions, although some exist, see Sections 6.1 and 5.4. At the very least, simulations are possible picking specific \( P_\theta \) or \( Q \in M \), and in many cases results may generalize to some extent because of invariance properties of model and test.

Also of potential interest are \( P_\theta(\Phi_C(z) = 1|\Phi_{MS}(z) = 0) \), i.e., the type I error probability (size) under \( M_{\Theta_0} \) or the power under \( M_\theta \) in case the model was in fact passed by the MS test, \( Q(\Phi_C(z) = 1|\Phi_{MS}(z) = 0) \) for \( Q \in M \setminus M_\theta \), i.e., the situation that the model \( M_\theta \) is in fact violated but was passed by the MS test, and whether \( \Phi_C \) can compete with \( \Phi_{AU} \) in case that \( \Phi_{MS}(z) = 1 \) (\( M_\theta \) rejected). These are investigated in some of the literature, see below.

For example, many researchers have found that the use of an MS test influences the size of the main test, meaning that \( P_\theta(\Phi_C(z) = 1|\Phi_{MS}(z) = 0) \) can be substantially different from \( P_\theta(\Phi_{MC}(z) = 1) \).

In Section 7 we look at the performance of \( \Phi_C \) in case there is a “hyperprobability” of having data generated from either \( P_\theta \in M_\theta \) or \( Q \in M \setminus M_\theta \); such a situation in which both satisfied and violated model assumptions can occur and \( \Phi_{MS} \) has some distinction work to do has to our knowledge not yet been analyzed in the literature, which therefore may give a too pessimistic picture of the performance of the combined procedure.

4 Controversial views of model checking

The necessity of model checking has been stressed by many statisticians for a long time, and this is what students of statistics are often taught. At first sight, model checking seems essential for two reasons. Firstly, statistical methods that a practitioner may want to use are often justified by theoretical results that require model assumptions, and secondly it is easy to construct examples for the breakdown of methods in case that model assumptions are violated in critical ways (e.g., inference based on the arithmetic mean, optimal under the assumption of normality, applied to data generated from a Cauchy distribution will not improve in performance for any number of observations compared with only having a single observation, because the distribution of the mean of \( n > 1 \) observations is still the same Cauchy distribution).
Regarding the foundations of statistics, checking of the model assumptions plays a crucial role in Mayo’s (2018) philosophy of “severe testing”, in which frequentist significance tests are portrayed as major tools for subjecting scientific hypotheses to tests that they could be expected to fail in case they were wrong; and evidence in favor of such hypotheses can only be claimed in case that they survive such severe probing. Mayo acknowledges that significance tests can be misleading in case that the model assumptions are violated, but this does not undermine her philosophy in her view, because the model assumptions themselves can be tested.

A problem with preliminary model checking is that the theory of the model-based methods usually relies on the implicit assumption that there is no data-dependent pre-selection or pre-processing. A check of the model assumptions is a form of pre-selection. This is largely ignored but occasionally mentioned in the literature. Bancroft (1944) was probably the first to show how this can bias a model-based method after model checking. Chatfield (1995) gives a more comprehensive discussion of the issue. Hennig (2007) coined the term “goodness-of-fit paradox” (from now on called “misspecification paradox” here) to emphasize that in case that model assumptions hold, checking them in fact actively invalidates them. Assume that the original distribution of the data fulfills a certain model assumption. Given a probability $\alpha > 0$ that the MS test rejects the model assumption if it holds, the conditional probability for rejection under passing the MS test is obviously $0 < \alpha$, and therefore the conditional distribution must be different from the one originally assumed. It is this conditional distribution that eventually feeds the model-based method that a user wants to apply.

How big a problem is the misspecification paradox? Spanos (2010) argues that it is not a problem at all, because the MS test and the main test “pose very different questions to data”. The MS test tests whether the data “constitute a truly typical realization of the stochastic mechanism described by the model”. He argues that therefore model checking and the model-based testing can be considered separately; model checking is about making sure that the model is “valid for the data” (Spanos, 2018), and if it is, it is appropriate to go on with the model-based analysis.

The point of view taken here, as in Chatfield (1995), Hennig (2007), and elsewhere in the literature reviewed below, is different: In case the model-based (MC) test is only applied if the model is not rejected, the behavior of the MC test should be analyzed conditionally on data not being rejected by the MS test, and this differs from the behavior under the nominal model assumption. We do not think that the misspecification paradox implies that combined procedures are invalid. Rather our perspective is pragmatic. Whether a combined procedure is good or not depends on its statistical characteristics, and how these compare to unconditional use of the MC or AU test. Various such characteristics can be of interest, particularly its behavior both in case that the assumption of the MC test holds, and that it is violated; for the latter case there are many possibilities, and investigation may constrain itself to look at specific alternative models.

In some situations, these different points of view can agree. If the distribution of the test statistic is independent of the outcome of the MS test, formally the misspecification paradox still holds, but it is irrelevant for the resulting data analysis. Conditioning on the result of the MS test will not affect the statistical characteristics of the MC test. An example for this is a MS test based on studentized residuals and a main test based on the minimal sufficient statistic of a Gaussian distribution (Spanos, 2010). Independence here and in most conceivable examples does only hold if the model assumption of the MC test holds, which means that only in this case conditioning on the MS test does not affect the MC test. One may hope that if the model assumption is violated, conditioning on the MS test rather helps the MC test, but as far as the literature reviewed below
has investigated this issue, this is rarely true. More generally it can be expected that if what the MS test does is at most very weakly stochastically connected to the main test (i.e., if in Spanos’s terms they indeed “pose very different questions to the data”), differences between the conditional and the unconditional behavior of the MC test should be small. This can be investigated individually for every combination of MS test and main test, and there is no guarantee that the result will always be that the difference is negligible.

Even in situations in which inference is only very weakly affected by preliminary model checking in case the assumed model holds indeed, the practice of model checking may still be criticized on the grounds that it may not help in case that the model assumption is violated, i.e., if data is generated by a model that deviates from the assumed one, the conditional distribution of the MC test statistic, given that the model assumption is not rejected, may not have characteristics that are any better than if applying the MC test to data with violated model-assumptions in all cases, see Easterling and Anderson (1978).

A view opposite to Spanos’s one, namely that model checking and inference given a parametric model should not be separated, but rather that the problems of finding an appropriate distributional “shape” and parameter values compatible with the data should be treated in a fully integrated fashion, can also be found in the literature (Easterling (1976), Draper (1995), Davies (2014)). Davies (2014) argues that there is no essential difference between fitting a distributional shape, an (in)dependence structure, and estimating a location (which is usually formalized as parameter of a parametric model, but could as well be defined as a nonparametric functional).

Bayesian statistics allows for an integrated treatment by putting prior probabilities on different candidate models, and averaging their contributions. Robust and nonparametric procedures may be seen as alternatives in case that model assumptions of model-based procedures are violated, but they have also been recommended for unconditional use (Hampel et al., 1986), making prior model checking supposedly superfluous. All these approaches still make assumptions; the Bayesian approach assumes that prior distribution and likelihood are correctly specified, robust and nonparametric methods still assume data to be i.i.d., or make other structural assumptions. So the checking of assumptions issue does not easily go away, unless it is claimed (as some subjectivist Bayesians do) that such assumptions are subjective assessments and cannot be checked against data. To our knowledge, however, there is hardly any literature assessing the performance of model checking combined in which the “MC role” is taken by robust, nonparametric or Bayesian inference, but see Bickel (2015) for a combined procedure that involves model checking and robust Bayesian inference.

Another potential objection to model assumption checking is that, in the famous words of George Box, “all models are wrong but some are useful”. It may be argued that model assumption checking is pointless, because we know anyway that model assumptions will be violated in reality in one way or another (e.g., it makes some sense to hold that in the real world no two events can ever be truly independent, and continuous distributions are obviously not “true” as models for data that are discrete because of the limited precision of all human measurement). This has been used as argument against any form of model-based frequentist inference, particularly by subjectivist Bayesians (e.g., de Finetti’s (1974) famous “probability does not exist”). Mayo (2018) however argues that “all models are wrong” on its own is a triviality that does not preclude a successful use of models, and that it is still important and meaningful to test whether models are adequately capturing the aspect of reality of interest in the inquiry, or whether the data are incompatible with the model in ways that will mislead the desired model-based inference (the
latter is our own wording). We broadly agree with this position, although we note that the current practice of model checking is almost exclusively framed in terms of whether model assumptions are fulfilled (or “approximately” fulfilled, which implies that there is a true model that could be approximated) rather than whether data indicate that the specific use made of the model may be corrupted by specific violations of the model assumptions, which would seem more appropriate.

A purely logical rebuttal of the view that frequentist methods of inference such as tests can only be valid if the model assumptions are fulfilled is as follows. The basis of that view is that the theoretical characteristics of the methods are derived assuming the model, but this does not imply that their characteristics are so bad as to render inferences invalid if the model does not hold. This would need to be investigated separately, and the role of model checking could then be to distinguish situations in which the characteristics of the MC test are still good, and situations in which this is not the case (although it would require further research to find out in which situation which MS testing does this appropriately).

We here investigate model assumptions that concern data generating mechanisms, and therefore they can be checked against the data. We keep an open mind regarding whether preliminary model checking should be recommended “good practice” and even whether (frequentist) testing is advisable at all; we rather aim at “mapping” the debate than solving it.

5 Are model assumptions checked in practice?

How widespread and established is it actually to check model assumptions before a model-based procedure is applied? This is hard to say, and somewhat contradictory observations can be made. Many statisticians emphasize model assumptions and the requirement to check them when teaching statistics, however hardly any statistics textbook explains how to do this with clear enough algorithmic “recipes” that a non-expert reader could easily follow, and there is hardly any agreement what exactly should be done.

An example for explicit recommendation of model checking in the literature is Rule 8 of the “Ten Simple Rules for Effective Statistical Practice” by Kass et al. (2016) aptly named ‘Check Your Assumptions’. The authors mention that “every statistical inference involves assumption ... even the so-called “model-free” techniques require assumptions, albeit less restrictive assumptions”. Another instance is Osborne and Waters (2002), named aptly “Four assumptions of multiple regression that researchers should always test”.

Choi (2005) mentioned that the most common statistical errors involve “failure to recognize the correct distribution of the data”, leading to incorrect choice of descriptive and inferential statistics. According to Olsen (2003), a frequent error made in data analysis is the application of statistical tests that assumes a normal distribution on data that actually follow a skewed distribution.

Strasak et al. (2007a) conducted a bibliometric analysis of all original research articles published during the first half of the year 2004 in Volume 30, Numbers 1-26 of the New England Journal of Medicine (NJEM) and Volume 10, Numbers 1-6 of Nature Medicine (NMed). They reviewed the use of statistical methods used in these medical journals. At least one kind of inferential statistical method were used in 94.5% out of 91 articles in the NJEM and 82.4% out of 34 papers in NMed. Among the most frequently used methods are the t-test and non-parametric tests at 36.8% and 24.8%, respectively, out of the total number of papers. A subgroup of 53 papers (31 from NJEM and 22 from NMed) were further assessed. It was observed that 20.8% of these
articles contained usage of wrong or suboptimal statistical tests resulting from incompatibility of test with examined data, inappropriate use of parametric methods, or using the wrong statistical test for the hypothesis under investigation. It was observed that 63% of the papers that use the \( t \)-test failed to report whether the test assumptions were checked. Similarly, Strasak et al. (2007b) assessed 15 papers from Wiener Klinische Wochenschrift and 7 papers from Wiener Medizinische Wochenschrift and found that the practice of improper use of statistical methods and failure to validate model assumptions were also found in these Austrian medical journals. It was observed in the papers that reported usage of \( t \)-test, 41.2% failed to report whether the test assumptions were checked. 18.2% of the papers did not include a multiple comparison or \( \alpha \)-level correction.

A Chinese study carried out by Wu et al. (2011) reviewed articles from 10 Chinese biomedical journals regarding the misuse of statistical methods in 1998 and 2008. All the original articles published, 1,335 in 1998 and 1,578 in 2008, were reviewed. Out of these, a total of 1,334 or 45.8% were reported to have incorrectly use either one of the most common statistical methods in these journals, namely the \( t \)-test, contingency tables, analysis of variance (ANOVA) or rank based non-parametric test. The authors are not explicit about whether they count the lack of checking of the model assumptions as “incorrect” and do not give precise numbers about it, but as a result of their study they suspect that researchers did not give enough attention to the distributional characteristics of the variables.

Sridharan and Gowri (2015) studied the statistical errors committed by medical researchers in eight Indian medical and surgical journals over a period of 2 years. They collected 195 articles from 2005 and 220 articles from 2006. They found that 33.7% of these articles did not mention checking normality prior to parametric tests, besides other errors such as using multiple tests without correction. Hassan et al. (2015) compared errors in statistical methods made in articles from ten Indian medical journals in 2003 and 2013 to ascertain whether the statistical methodology used in these journals has improved in one decade by analysis of the number of errors committed. They reviewed 588 articles from 2003 and 774 articles from 2013. The most used statistical methods is the \( t \)-test, contingency tables and ANOVA. They observed that the proportion of erroneous statistical analyses had not decreased significantly, 25% in 2003 compared to 22.6% in 2013. However, they noticed an increased use of rank based non-parametric tests in 2013, which they assume indicates that more attention are being paid to the assumptions of parametric tests. More recently, a study was done in Egypt by Nour-Eldein (2016) that assessed statistical methodology errors in family medicine articles by authors affiliated with the Suez Canal University over 5 years. Out of the 60 papers reviewed, the author found that a quarter (25%) “failed to report that test assumptions were not violated” as well as a few more errors that were made by medical researchers. This obviously does not imply that the assumptions were violated in critical ways when researchers used model-based methods.

These studies are of course limited to assessing the information reported in the publications. There is simply no way of knowing the unpublished details unless the authors were contacted and asked whether assumptions were in fact validated. It was also suggested that some authors merely copied methods from previous work without actually knowing what is needed to be done before running a statistical test. This could result from the fact that model checking was not reported and this practice was copied by subsequent studies. Altman (2002) wrote that “once incorrect procedures become common, it can be hard to stop them from spreading through the medical literature like a genetic mutation”.

Keselman et al. (1998) reviewed articles from 17 journals of educational and behavioral sci-
ence research. The authors claim to provide evidence that the vast majority of educational researchers conduct statistical analyses without taking into account the distributional assumptions of the procedure they are using. Out of the 411 articles reviewed, 61 had a between subjects univariate design. 13 out of the 61 did not report any cell of group standard deviations for any of the dependent variables under investigation. When the authors looked at the remaining articles it was found that the ratio of the largest to smallest standard deviation had a mean of 2.0, a median of 1.5 and a maximum of 23.8. In the articles that carried out factorial studies, the ratios has a mean of 2.8, a median of 1.7 and a maximum of 29.4. This shows that in the majority of the studies, the samples did not show variance homogeneity; often it looked in fact violated, where tests that assume variance homogeneity were used. Only in 12 articles were violations of the distributional assumptions mentioned as a source of concern by the authors.

Hoekstra et al. (2012) proceeded in a different manner. They asked 30 researchers to analyze a number of fictitious datasets and observed and interviewed them regarding the awareness of model assumptions and whether models were checked. They observed that models were checked “correctly” in between 12% and 23% of cases (depending in what assumption was considered). They stated that one possible explanation is the lack of knowledge of “acceptable remedies” in case that an assumption was found to be violated.

The overall impression is that the situation is mixed. Model assumptions are often ignored but by implication of the cited numbers there also seems to be a good number of works in which they are actually checked in some way. It is not reported whether combined procedures are used, i.e., pre-specified choice of the main test conditionally on the outcome of a MS test. Chances are that this mostly happens in a rather informal manner without pre-specification, if at all. There is some scattered literature that uses a combined procedures in a more formal manner, e.g., Gambichler et al. (2002).

For almost all authors of the cited studies of literature containing applied statistic not checking the model assumptions constitutes an error, although this may be seen as somewhat controversial given the existing criticism of preliminary model checking, see above and below. We would not think though that this is a reason for the absence of model checking in the vast majority if not all of the surveyed publications in which this “error” was made. Chances are that even most authors whose work implies negative results for preliminary model checking would agree that simply ignoring model assumptions is not a beneficial approach.

6 Some specific test problems

6.1 The problem of whether to pool variances

Historically the first problem for which preliminary MS testing and combined procedures were investigated was whether to assume equal variances for comparing the means of two samples. Until now this is the problem for which most work investigating combined procedures exists. Let \( X_1, X_2, \ldots, X_n \) be distributed i.i.d. according to \( P_{\mu_1, \sigma_1^2} \) and \( Y_1, Y_2, \ldots, Y_n \) be distributed i.i.d. according to \( P_{\mu_2, \sigma_2^2} \), where \( P_{\mu, \sigma^2} \) denotes the normal distribution with mean \( \mu \) and variance \( \sigma^2 \). If \( \sigma_1^2 = \sigma_2^2 \), the standard two-sample \( t \)-test using a pooled variance estimator from both samples is optimal. If testing is two-sided, this is equivalent to the \( F \)-test using the squared \( t \)-statistic. The \( F \)-test can also be applied to comparing means of more than two samples in an Analysis of Variance setup, and
some early papers use the corresponding terminology despite only comparing two samples.

For $\sigma_1^2 \neq \sigma_2^2$ Welch’s approximate $t$-test with adjusted degrees of freedom depending on the two individual variances is often recommended, see Welch (1938), Satterthwaite (1946), and Welch (1947). See Scheffé (1970) for some alternative solutions to the so-called Behrens-Fisher problem, i.e., comparing means without assuming equal variances.

The normal distribution assumption will be discussed below, but normality has often been seen as not problematic due to the Central Limit Theorem, and therefore the historical starting point is the equal variances assumption. Early authors beginning from Bancroft (1944) did not frame the problem in terms of “making sure that model assumptions are fulfilled”, but rather asked, in a pragmatic manner, under what circumstances pooling variances is advantageous. If the two variances are in fact equal or very similar, it is better to use all observations for estimating a single variance hopefully precisely, whereas if the two variances are very different, the use of a pooled variance will give a biased assessment of the variation of the means and their difference.

It has been demonstrated that the two sample $t$-test is very robust against violations of equality of variances when sample sizes are equal as shown by Hsu (1938), Scheffé (1970), Posten, Yeh and Owen (1982) and Zimmerman (2006). When both variances and sample sizes are unequal, the probability of the Type-I error exceeds the nominal significance level if the larger variance is associated with the smaller sample size and vice versa [Zimmerman (2006); Wiedermann and Alexandrowicz (2007); Moder (2010)], which is amended by Welch’s $t$-test.

Bancroft (1944) was the first to investigate preliminary testing of the equality of variances. By using an $F$ test as MS test to test the homogeneity of estimates of variance, he decided to use either a pooled estimate of $\sigma_2^2$ or just the sample variance of the first sample. He then looked at bias and variance of the resulting variance estimator, concluding that the lowest bias can be had by never pooling the estimate and not use the MS test to check the model assumption. On the other hand, the variance is lowest for the pooled estimate. Bancroft gives a set of numerical results but refrains from a general recommendation whether one should always pool, never pool, or use the combined procedure.

Starting from Bancroft’s work, from the end of the 1940s, a good amount of research was done on the problem of pooling variances, much of which concerned the estimation of means and the corresponding mean squared errors, but some work also dealt with combined testing procedures. Bancroft and Han (1977) published a comprehensive bibliography, also including other problems of preliminary assumption testing. One reason for the popularity of the variance pooling problem in early work is that, as long as normality is assumed, only the ratio of the variances needs to be varied to cover the case of violated model assumptions, which makes it easier to achieve theoretical results without computer-intensive simulations.

For the purpose of the current presentation, Gurland and McCullough (1962) stand out among the early work. They defined four combined procedures for testing equality of means. The decision about equal variances was made based on the ratio of the sample variances. Two different procedures were compared for the case that equality of variances is rejected, and procedures were further distinguished by whether it can be assumed as known that in case of inequality of the variance a specific one of the variances is larger than the other. For these procedures the authors were able to compute sizes (type I error probabilities) and power analytically without simulations. They presented the results in comprehensive tables depending on the actual ratio between variances and the sample sizes. The combined procedures could achieve better power (with acceptable type I error probabilities) than the test without equal variances assumption occasionally but not often;
results for unconditional use of pooling were not given.

Bancroft (1964) gave recommendations for when to pool different mean squares occurring in Analysis of Variance tables used for standard testing problems based on significance tests of their equality. Recommendations depend on to what extent the involved degrees of freedom are imbalanced (less balance requires a higher level of the preliminary test, because pooling is more dangerous in that case). Another recommendation is to choose a much higher significance level of 0.25 or even 0.5 for the MS test than one would normally use in significance testing. This recommendation turned up again in later work combined procedures for other problems. Given that the AU test is based on a more general model assumption, it turns out to be advantageous in many situations to use it not only if there is strong evidence against the model assumption of the test based on the more constrained model, but already if a certain amount of violation of the constrained model seems just a realistic possibility.

Arnold (1970) considered a different problem, namely whether to pool observations of two groups if the mean of the first group is the main target for testing. Pooling assumes that the two means are equal, so a test for equality of means here is the MS test. Arnold observes that in vast regions of the parameter space a better power can be achieved without pooling. The recommendation to generally use the test that requires less restrictive model assumptions because the combined procedure is better only in small regions of the parameter or distribution space of interest is another recurring theme in the literature about combined procedures. Moser, Stevens and Watts (1989) came to the same conclusion comparing a combined procedure for the problem of pooling variances with a standard two-sample t-test and Satterwaite’s Approximate F-test (Satterthwaite (1946)), an alternative to Welch’s t-test, based on size and power evaluations. They recommended to always use Satterwaite’s Approximate F-test without testing the equal variances assumption. Moser and Stevens (1992) recommended to never test the equal variances assumption. The standard t-test was recommended under certain sample sizes, whereas mostly the AU test was recommended. Results for Satterwaite’s Approximate F-test and Welch’s t-test were approximately equal.

Gans (1981) simulated the combined procedure for pooling variances on data from normal, uniform, and exponential distributions and concluded that the combined procedure does not fully remove the bias of the standard t-test.

Markowski and Markowski (1990) evaluated the setup of having an MS test of homogeneity of variances, the $F$-test, before doing a $t$-test for various combinations of sample size and significance level, looking also at non-normal distributions. The samples were drawn from normal distributions, a contaminated normal distribution with a higher frequency of outliers, the exponential distribution and the chi-squared distribution. For data with non-normal distributions, the results support those of Box (1953) and strongly discourage use of the $F$-test as an MS test. For situation with data generated from the normal distribution, the MS test was either unnecessary or ineffective as an MS test to alert the researcher that a $t$-test may be inappropriate. For equal sample sizes, no MS test is needed as the $t$-test is robust enough. However, for unequal sample sizes, the $t$-test is not so robust and the authors note that a more effective MS test would be desirable.

These results are backed up by Albers, Boon and Kallenberg (2000a), who presented a second order asymptotic analysis of the combined procedure for pooling variances with the $F$-test as MS-test. They argue that this procedure can only achieve a better power than unconditional testing under the unconstrained model if the test size is also increased. This means that there are only two possibilities for the combined procedure to improve upon the MC test. Either the combined procedure is anticonservative, i.e., violates the desired test level, which would be deemed unac-
ceptable in most applications, or the size of the MC test is smaller than the nominal level, which if its assumptions are not fulfilled is sometimes the case. Albers, Boon and Kallenberg (2000b) extend these results to the analysis of a more general problem for distributions $P_{\theta, \tau}$ from a parametric family with two parameters $\theta$ and $\tau$, where $\theta = 0$ is the main null hypothesis of interest and the decision between an MC test assuming $\tau = 0$ and an AU test without that assumption is made based on an MS test testing $\tau = 0$ (in the two-sample variance pooling problem, $\tau$ could be the logarithm of the ratio between the variances; a simpler example would be the choice between Gauss- and $t$-test in the one-sample problem, where the MS test tests whether the variance is equal to a given fixed value). The tests are all assumed to allow a certain mathematical expansion that is fulfilled by standard tests such as likelihood ratio tests. Once more, the combined procedure can only achieve better power at the price of a larger size, potentially being anticonservative. Another interesting aspect is that the authors introduce a correlation parameter $\rho$ formalizing the dependence between the MS-test and the main tests. In line with the discussion in Section 2, they state that for strong dependence preliminary testing is not sensible, and their results consider the case $\rho \rightarrow 0$.

Zimmerman (2004) investigated by simulation the rejection rates of a combined procedure using the Levene test as MS-test on samples of different sizes with equal and unequal variances followed by either a pooled-variance Student $t$-test or Welch’s $t$-test. Only the type I error probability was considered. The final recommendation was to use the Welch $t$-test unconditionally, especially when the sample sizes are unequal, where both the pooled-variance test and the combined procedure were found to be prone to exceed the nominal level. Zimmerman (2014) looked at the behavior of the pooled-variance $t$-test for samples that were selected so that the variance ratios did not exceed a certain cut-off value, and found that the resulting conditional sizes and powers were substantially affected.

6.2 Tests of normality in the one-sample problem

The simplest problem in which preliminary misspecification testing has been investigated is the problem of estimating the location of a sample. The standard model-based procedure for this is the one-sample Student’s $t$-test. It assumes the observations $X_1, X_2, \ldots, X_n$ to be i.i.d. normal. For non-normal distributions with existing variance the $t$-test is asymptotically equivalent to the Gauss-test, which is asymptotically correct due to the Central Limit Theorem. The $t$-test is therefore often branded robust against non-normality if the sample is not too low, see, e.g., Bartlett (1935), Lehmann and Romano (2005). An issue is that the quality of the asymptotic approximation does not only depend on $n$, but also on the underlying distributional shape, as the speed of normal approximation is not uniform. Very skew distributions or extreme outliers can affect the power of the $t$-test for fairly large $n$, see Cressie (1980) for a detailed discussion. Cressie mentions that the biggest problems occur for violations of independence, however we are not aware of any literature examining of independence testing combined with the $t$-test. Instead, a number of publications examine preliminary normality testing for the $t$-test.

Some work focuses just on the quality of the MS tests. There is a good amount of general studies investigating and comparing normality tests without specific reference to its effect on subsequent inference and combined procedures. Razali and Wah (2011), comparing the Shapiro-Wilk, Kolmogorov-Smirnov, Lilliefors and Anderson-Darling tests, concluded that Shapiro-Wilk has the best power for a given level. The Anderson-Darling test was a close second. This result concurr
with Mendes and Pala (2003), Farrell and Rogers-Stewart (2006) and Keskin (2006).

Some authors investigated normality tests regarding its use for subsequent inference without considering the later main test. Schoder et al. (2006) concluded that the Kolmogorov-Smirnov test performs badly, and they advised against preliminary testing of normality. Keselman et al. (2013) discussed different types of testing for normality. They used the Kolmogorov-Smirnov, Cramer-von Mises and Anderson-Darling tests, 26 different shapes of distributions (14 distributions with different skewness and kurtosis values, 8 contaminated normal mixture models and 4 multinomial models), 3 sample sizes and 4 different level of significance. They concluded that the Anderson-Darling test is the most effective one at detecting non-normality relevant to subsequent $t$-testing, and they suggested that for deciding whether the MC test should be used, the MS test be carried out at a significance level larger than 0.05, for example 0.15 or 0.20, in order to increase the power.

The next group of work examines running a $t$-test conditionally on passing normality by a preliminary normality test, without considering what happens if normality is rejected. Easterling and Anderson (1978) state that they started their investigation from the belief that the practice of this two-stage procedure is not only conventional, but also good. To test this they considered various distributions such as normal, uniform, exponential, two central and two non-central $t$-distributions. They only considered sample sizes 10 and 20. They collected 1000 samples for both situations, normality passed and rejected, respectively, at 10% significance level, using both the Anderson-Darling and the Shapiro-Wilk normality tests. After obtaining those samples, the empirical distribution of the 1000 $t$-values was compared to the expected frequencies from the Student’s $t$-distribution. This worked reasonably well when the samples were drawn from the normal distribution. For symmetric non-normal distributions, the results were mixed, and for situations where the distributions were asymmetric, the distribution of the $t$-values did not resemble a Student’s $t$-distribution, which they take as an argument against the practice of preliminary normality testing, because in case that the underlying distribution is not normal, normality testing does not help. They discuss this as follows:

> There are various reasons why the distributions of the $t$ ratio in the cases considered might not follow a Student’s $t$ distribution — the non normality of the numerator, the non zero expectation of the numerator, the non chi-squareness of the square of the denominator, and lack of independence of numerator and denominator. For the asymmetric sampling distributions, the empirical distributions of $t$ (not shown in this paper) suggest that the preliminary goodness of fit test causes a shift in mean. In order to obtain a sample from such a distribution which would pass a test for normality (which includes symmetry as a property) that sample would have to have fewer observations in the elongated tail than are expected.

They tried to adjust for the shift in mean, but this did not improve the results. As a result they favored a non-parametric approach. If a probability model is to be used as a reporting device to discover and describe patterns of variability, then normality testing could be sensible.

Schucany and Ng (2006) investigated the Type I error rate of the one sample $t$-test given that the sample has passed the Shapiro-Wilk test for normality, i.e., the conditional Type I error rate. Data were sampled from normal, uniform, exponential and Cauchy populations. The simulation study showed that, for the uniform distribution, screening of samples by an MS test for normality leads to a more conservative conditional Type I error rate than application of the one-sample $t$-test without MS testing. In contrast, for the exponential distribution, the conditional Type I error rate is
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even more elevated than the Type I error rate of the \( t \)-test without MS testing (i.e. the unconditional Type I error rate) which is already above the nominal level. Furthermore, larger sample sizes and more liberal significance levels of the MS test shift the conditional Type I error rate even further away from the unconditional Type I error rate of the \( t \)-test and also from the nominal level, leading to either more conservative or more liberal test decisions. This common feature of the uniform and exponential distributions is especially interesting to note as, in both cases, the \( t \)-test without MS testing shows an acceptable Type I error rate at least for moderate sample sizes.

Rochon and Keiser (2011) investigated the reasons behind the characteristics of the one sample \( t \)-test after MS testing for normality. Samples were drawn from the exponential, log-normal, uniform, Student’s \( t \) with 2 degrees of freedom and standard normal distributions that had passed the pretest. The Shapiro-Wilk test and the Lilliefors modification of the Kolmogorov Smirnov test were used. It was found that the results from the two MS tests were similar, therefore only results from the Shapiro-Wilk test were presented. For the exponential and log-normal distributions, the Type I error rate is elevated for unselected samples and it is further increased by the MS test for normality. The inspection of the densities of the samples that pass the MS test shows that the closer the underlying population distribution is to the normal, the less impact the normality test has. Where the population distribution is farther from the normal, the MS screening in fact selects samples that look like normal and thus can no longer be considered representative of the true underlying population. They concluded that formal MS testing for normality cannot be recommended.

The unconditional \( t \)-test relying on the normal approximation of reasonably large sample sizes by way of the Central Limit Theorem taking into account that the one sample \( t \)-test is more sensitive to skewness than to heaviness or lightness of the tails (Rao, 1998) is an alternative. If it is at least known that the underlying population distribution is symmetric, a non-parametric application such as the Wilcoxon-Mann-Whitney signed-rank test could be considered. It is recommended that the model assumptions should be checked from external data sources if possible, and not from the data set at hand.

To our knowledge, in the one-sample problem there are no investigations of full combined procedures.

6.3 Tests of normality in the two-sample problem

For the two-sample problem, the Wilcoxon-Mann-Whitney (WMW) rank test is a popular alternative to the two-sample \( t \)-test with (in the context of preliminary normality testing) mostly assumed equal variances. In principle most arguments and results from the one-sample problem apply here as well, with the additional complication that normality is assumed for both samples, and can be tested either by testing both samples separately, or by pooling residuals from the mean. When its assumptions are met, the two-sample Student’s \( t \)-test was shown to perform superior to nonparametric tests in Hodges and Lehmann (1956) and Randles and Wolfe (1979). As for the one-sample problem, there are also claims and results that the two-sample \( t \)-test is rather robust to violations of the normality assumption (Hsu and Feldt, 1969; Rasch and Guiard, 2004) and some evidence that this is sometimes not the case and the WMW rank test is superior and does not lose much power even if normality is fulfilled (Neave and Granger, 1968). Fay and Proschan (2010) produced a survey on comparing the two-sample \( t \)-test with the WMW test, citing mainly theoretical arguments. They also discussed some decision rules between these two tests, recommending against normality testing.
Rochon et al. (2012) investigated by simulation combined procedures based on both strategies for preliminary normality testing (both samples separately, and pooled residuals) using a Shapiro-Wilk test of normality. Data were simulated from normal, exponential, and uniform distributions. The MC test was the two sample $t$-test, the AU test was the WMW test. Although preliminary testing showed a substantial effect on conditional sizes and powers, the overall sizes and powers of the combined procedure were acceptable. Looking at the results in detail, for all the simulated distributions, the MC test achieved a higher power than the AU test (with the combined procedure somewhere in between), whereas all delivered more or less acceptable type I error rates. Overall therefore in these situations the use of the WMW test as AU test is not favorable, be it with or without preliminary MS testing. The authors advise that it would be better to use information from extra-data sources to decide between the MC and the AU test (as had been already advised by Easterling and Anderson (1978)). The pooled residuals normal testing strategy looked a little bit better than the separate groups one, but this may be due to the fact that in all experiments both samples were simulated from the same distributional family, which makes sure that pooling the residuals will not be misleading.

In the light of the conceptual problems and mixed results with preliminary normality testing, Zimmerman (2011) achieves good simulation results with an alternative approach, namely running both the two-sample $t$-test and the WMW test, choosing the two-sample $t$-test in case the suitably standardized values of the test statistics are similar and the WMW test in case the p-values are very different, although the tuning of this approach is somewhat less intuitive, and the MS paradox still applies, i.e., in case that the assumptions for the two-sample $t$-test are originally fulfilled, they are violated conditionally on the decision rule choosing that test.

### 6.4 Regression

In standard linear regression,

$$y_i = \beta_0 + \beta_1 x_{1i} + \ldots + \beta_p x_{pi} + e_i, \ i = 1, \ldots, n,$$

with response $Y = (y_1, \ldots, y_n)$ and explanatory variables $X_j = (x_{j1}, \ldots, x_{jn}), \ j = 1, \ldots, p$. $e_1, \ldots, e_n$ are in the simplest case assumed i.i.d. normally distributed with mean 0 and equal variances.

The regression model selection problem is the problem to select a subset of a given set of explanatory variables $\{X_1, \ldots, X_p\}$. This can be framed as a model misspecification test problem, because a standard regression assumes that all variables that systematically influence the response variable are in the model. If it is of interest, as main test problem, to test $\beta_j = 0$ for a specific $j$, the MS test would be a test of null hypotheses $\beta_k = 0$ for one or more of the explanatory variables with $k \neq j$. The MC test would test $\beta_j = 0$ in a model with $X_k$ removed, and the AU test would test $\beta_j = 0$ in a model including $X_k$. This problem was mentioned as second example in Bancroft’s (1944) seminal paper on preliminary assumption testing.

Traditional model selection approaches such as forward selection and backward elimination are often based on such tests and have been analyzed (and criticized) a lot in the literature. We will not review this literature here. There is sophisticated and innovative literature on post-selection inference in this problem. Berk et al. (2013) propose a procedure in which main inference is adjusted for simultaneous testing taking into account all possible submodels that could have been selected. Efron (2014) uses bootstrap methods to do inference that takes the model selection process into account. Both approaches could also involve other MS testing such as of normality,
homoscedasticity, or linearity assumptions, as long as combined procedures are fully specified. For specific model selection methods there now exists work allowing for exact post-selection inference, see Lee et al. (2016). For a critical perspective on these issues see Leeb and Pötscher (2005, 2015). In econometrics, David Hendry and co-workers developed an automatic modeling system that involves MS testing and conditional subsequent testing with adjustments for decisions in the modeling process, see, e.g., Hendry and Doornik (2014). Earlier, some authors such as Saleh and Sen (1983) analyzed the effect of preliminary testing on later conditional main testing.

Godfrey (1988) listed a plethora of MS tests to test the various assumptions of linear regression. However, no systemic way to apply these tests was discussed. In fact, Godfrey noted that the literature left more questions open rather than answered. Some of these questions are: (i) the choice among different MS tests, (ii) whether to use nonparametric or parametric tests, (iii) what to do when any of the model assumptions are invalid as well as (iv) some potential problems with MS testing such as repeated use of data, multiple testing and pre-test bias. Godfrey (1996) discussed destructive and constructive value of MS tests. He concluded that efforts should be made to develop ‘attractive’, useful and simple combined procedures, keeping in mind that the combination of tests must be well-behaved. One suggestion was to use the Bonferroni correction for each test as “the asymptotic dependence of test statistics is likely to be the rule, rather than the exception, and this will reduce the constructive value of individual checks for misspecification”.

Giles and Giles (1993) reviewed the substantial amount of work done in econometrics regarding preliminary testing in regression up to that time, a limited amount of which is about MC and/or AU tests conditionally on MS tests. This involves pre-testing of a known fixed variance value, homoscedasticity, and independence against an autocorrelation alternatives. The cited results are mixed. King and Giles (1984) comment positively on a combined procedure in which absence of autocorrelation is tested first by a Durbin-Watson or t-test. Conditionally on the result of that MS test, either a standard t-test of a regression parameter is run (MC test) or a test based on an empirically generalized least squares estimator taking autocorrelation into account (AU test). In simulations the combined procedure performs similar to the MC test and better than the AU test in absence of autocorrelation, and similar to the AU test and better than the MC test in presence of autocorrelation. Also here it is recommended to run the MS test at a level higher than the usual 5%. Most related post-1993 work in econometrics seems to be on estimation after pre-testing, and regression model selection. Ohtani and Toyoda (1985) propose a combined procedure for testing linear hypotheses in regression conditionally on testing for known variance. Toyoda and Ohtani (1986) test the equality or different regressions conditionally on testing for equal variances. In both papers power gains for the combined procedure are reported, which are sometimes but not always accompanied with an increased type I error probability.

### 6.5 More than one misspecification test

Rasch et al. (2011) assessed the statistical properties of a three-stage procedure including testing for normality and for homogeneity of the variances. They considered 5 distributions with different location, spread, skewness and kurtosis parameters. Various sample sizes, equal and unequal, and different ratios of the standard deviation were considered. They considered three main statistical tests, the Student’s t-test, the Welch’s t-test and the WMW test. For the MS testing, they used the Kolmogorov-Smirnov test for testing normality and Levene’s test for testing the homogeneity of the variances of the two samples that were generated. If normality was rejected by the
Kolmogorov-Smirnov test, the WMW test was used. If normality was not rejected, the Levene’s test was run and if homogeneity was rejected, the Welch’s t-test was used and if homogeneity was not rejected, the standard t-test was used. The authors presented the rejection rates and the power of the procedure and compared it with the tests when the model assumption were not checked. The authors concluded that assumptions underlying the two sample t-test should not be pre-tested because “pre-testing leads to unknown final Type I and Type II risks if the respective statistical tests are performed using the same set of observations”. They prefer Welch’s t-test overall to both Student’s t-test and the WMW test. This preference is in line with an earlier recommendation of Rasch and Guiard (2004), who advise against the WMW test in case of unequal variances.

To our knowledge this is the only investigation of a combined procedure involving more than one MS test apart from the work on regression model selection cited in Section 6.6. Spanos proposed a “probabilistic reduction”-approach (e.g., Spanos, 2018) in order to systematize the process of model building involving MS testing of various assumptions, but he did not define a fully automatized procedure that could be investigated by means of theory or simulation.

6.6 Discussion

Although many authors have, in one way or another, investigated the effects of preliminary MS testing or later application of model-based procedures, there are some limitations in the existing literature. Only very few papers have compared the performance of a fully specified combined procedure with unconditional uses of both the MC and the AU test. Some of these have only looked at type I error probabilities but not power, some have only looked at the situation in which the model assumption is in fact fulfilled, and some have studied setups in which either the unconditional MC or the AU test works well across the board, making a combined procedure superfluous, although it is widely acknowledged that situations in which either unconditional test can perform badly depending on the unknown data generating process do exist.

Recurring themes in the work investigating combined procedures are a mostly critical position that authors take on preliminary MS testing; in case that it is done, a recommendation to use a higher level for the MS test than the conventional 5%; the requirement that the MS test should be independent or approximately independent of the later MC and AU test. In some setups, despite being often critical of the combined procedure, authors acknowledge that there is a requirement for distinguishing between situations in which the MC test should be applied and situations in which the AU test is favorable. Apart from MS tests, such distinctions could come from prior information and sources outside the data. Occasionally recommendations are conditional on sample sizes.

Comparing a full combined procedure with unconditional use of the MC test or the AU test, a typical pattern should be that under the model assumption for the MC test, the MC test is best regarding power, and the combined procedure performs between the unconditional MC test and AU test, and if that model assumption is violated, the AU test is best, and the combined procedure is once more between the MC test and the AU test. King and Giles (1984), Toyada and Ohtani (1986) are examples for this. Results on test size are consistent with this (i.e., in cases where the combined procedure violates the nominal test level, at least one of the unconditional procedures does that as well). Such results can be interpreted charitably for the combined procedure, which allows for some kind of maximin performance. It seems to us that part of the criticism of the combined procedure is motivated by the fact that it does not do what some seem to expect or hope it to do, namely to help making sure that model assumptions are fulfilled, and to otherwise leave
performance characteristics untouched, which is destroyed by the misspecification paradox.

However, for pooling variances in the two-sample problem Welch’s t-test seems to perform well more or less across the board, and in the case of normal testing for the two-sample problem, non-normal distributions have been chosen for simulation in the literature for which WMW does not seem to be of much use, although such distributions exist.

A more sober look at the results reveals that the combined procedures are almost always competitive with at least one of the unconditional tests, and often with them both. It is clear, though, that recommendations need to depend on the specific problem, the specific tests involved, and often also on in what way exactly model assumptions of the MC test are violated.

7 A positive result for combined procedures

The overall message from the literature does not seem very satisfactory. On the one hand, model assumptions are important and their violation can severely damage results. On the other hand, most comments on testing the model assumptions before using a method that is based on them, and only using the model-based method if the model assumption is passed are rather critical. Bayesians may think that all this only confirms that frequentist statistics does not work and should not be used, but the Bayesian approach does not free statistics from model assumptions, and it has been argued that Bayesians should do more to check them (Gelman and Shalizi, 2012).

In this section we present a point of view and a result that makes us think somewhat more positively about combined procedures and the impact of preliminary model testing. A characteristic of the literature analyzing combined procedures is that they compare the combined procedure with unconditional MC or AU tests both in situations where the model assumption of the MC test is fulfilled, or not fulfilled. However, they do not investigate a situation in which the MS test can do what it is supposed to do, namely to distinguish between these situations. This can be modeled in the simplest case as follows, using the notation from Section 3. Let \( P_\theta \) be a distribution that fulfills the model assumptions of the MC test, and \( Q \in M \setminus M_\Theta \) a distribution that violates these assumptions. For considerations of power, let the null hypothesis of the main test be violated, i.e., \( \theta \notin \Theta_0 \) and \( Q \notin M^* \) (an analogous setup is possible for considerations of size). We may observe data from \( P_\theta \) or from \( Q \). Assume that a dataset is with probability \( \lambda \in [0, 1] \) generated from \( P_\theta \) and with probability \( 1 - \lambda \) from \( Q \) (we stress that as opposed to standard mixture models, \( \lambda \) governs the distribution of the whole dataset, not every single observation independently). The cases \( \lambda = 0 \) and \( \lambda = 1 \) are those that have been treated in the literature, but only if \( \lambda \in (0, 1) \) the ability of the MS test to inform the researcher whether the data are more likely from \( P_\theta \) or from \( Q \) is actually required.

We ran several simulations of such a setup (looking for example at normality in the two-sample problem), which will in detail be published elsewhere. Figure 1 shows a typical pattern of results. In this situation, for \( \lambda = 0 \) (model assumption violated), the AU test is best and the MC test is worst. For \( \lambda = 1 \), the MC test is best and the AU test is worst. The combined procedure is in between, which was mostly the case in our simulations. Here, the combined procedure is for both of these situations close to the better one of the unconditional tests (to what extent this holds depends on details of the setup). The powers of all three tests are linear functions of \( \lambda \) (linearity in the plot is distorted by random variation only), and the consequence is that the combined procedure performs clearly better than both unconditional tests over the best part of the range of \( \lambda \). Unless
an unconditional test achieved perfect power (for too easily detectable violations of the $H_0$), in our simulations it was always the case that for a good range of $\lambda$-values the combined procedure was the best. To brand the combined procedure “winner” would require the nominal level to be respected under $H_0$ (i.e., for both $P_0$, $\theta \in \Theta_0$ and $Q \in M'$), which was very often though not always the case.

Before stating a general result, here are some words on the relevance of such a setup. Obviously it is not realistic that only two distributions are possible, one of which fulfills the model assumptions of the MC test. We wanted to keep the setup simple, but of course one could look at mixtures of a wider range of distributions, even a continuous range (for example for ratios between group-wise variances). In any case, the setup is more flexible than looking at $\lambda = 0$ and $\lambda = 1$ only, which is what has been done in the literature up to now. In real research, is there something like a probability $\lambda$ that model assumptions will be fulfilled? Of course model assumptions will never hold precisely, but the idea seems appealing to us that a researcher in a certain field who very often applies certain tests comes across a certain percentage different from 0 or 1 of cases which are well-behaved in the sense that a certain model assumption is a good if not perfect description of what is going on (the setup has a certain Bayesian flavor, but the researcher may not be interested in priors or posteriors for $\lambda$ because the proportion $\lambda$ under such an interpretation is pieced together from situations concerning different research topics).

We use the notation from Section 3 with the following additions. $P_\lambda$ stands for distribution of the overall two step experiment, i.e., first selecting either $P = P_0$ or $P = Q$ with probabilities $\lambda$, $1 - \lambda$ respectively, and then generating a dataset $z$ from $\tilde{P}$. The events of rejection of the respective $H_0$ are denoted $R_{MS} = \{\Phi_{MS}(z) = 1\}$, $R_{MC} = \{\Phi_{MC}(z) = 1\}$, $R_{AU} = \{\Phi_{AU}(z) = 1\}$, $R_C = \{\Phi_C(z) = 1\}$. Here are some assumptions:

(I) $\Delta_\theta = P_0(R_{MC}) - P_0(R_{AU}) > 0$,

(II) $\Delta_Q = Q(R_{AU}) - Q(R_{MC}) > 0$,

(III) $\alpha_{MS} = Q(R_{MS}) > \alpha_{MS} = P_\theta(R_{MS})$,

(IV) Both $R_{MC}$ and $R_{AU}$ are independent of $R_{MS}$ under both $P_\theta$ and $Q$.

Keep in mind that this is about power, i.e., the $H_0$ of the main test is violated for both $P_\theta$ and $Q$. Assumption (I) means that the MC test has the better power under $P_\theta$, (II) means that the AU test has the better power under $Q$. Assumption (III) means that the MS test has some use, i.e., it has a certain (possibly weak) ability to distinguish between $P_\theta$ and $Q$. All these are essential requirements for preliminary model assumption testing to make sense. Assumption (IV) though is very restrictive. It asks that rejection of the main null hypothesis by both main tests is independent of the decision made by the MS test. This is unrealistic in most situations. However, it can be relaxed (at the price of a more tedious proof that we do not present here) to demanding that there is a small enough $\delta > 0$ (dependent on the involved probabilities) so that $|P_\theta(R_{MC}|R_{MS}) - P_\theta(R_{MC}|R^c_{MS})|$, $|P_\theta(R_{AU}|R_{MS}) - P_\theta(R_{AU}|R^c_{MS})|$, $|Q(R_{MC}|R_{MS}) - Q(R_{MC}|R^c_{MS})|$, and $|Q(R_{AU}|R_{MS}) - Q(R_{AU}|R^c_{MS})|$ are all smaller than $\delta$, which can be fulfilled in many cases of interest. As emphasized earlier, approximate independence of the MS test and the main tests is an important desirable feature of a combined test, and it should not surprise that a condition of this kind is required.

The following Lemma states that the combined procedure has a better power than both the MC test and the AU test for at least some $\lambda$. Although this in itself is not a particularly strong
Figure 1: Power of combined procedure, MC, and AU test across different \( \lambda \)s from an exemplary simulation. The MC test here is Welch’s two-sample \( t \)-test, the AU test the WMW-test, the MS test Shapiro-Wilks, for \( \lambda = 1 \) corresponds to normal distributions with mean difference 1, \( \lambda = 0 \) corresponds to \( t_3 \)-distributions with mean difference 1.

result, in many situations, according to our simulations, the range of \( \lambda \) for which this holds is quite large. Also the Lemma serves to give an idea of the required ingredients, i.e., what is important for the combined procedure to be superior to both the MC and the AU test, which is mainly the approximate independence between MS test and the main tests. (I)-(III) only require that the involved tests roughly do what they are supposed to do (and not even necessarily very well).

**Lemma 1.** Assuming (I)-(IV), \( \exists \lambda \in (0, 1) \) such that both \( P_\lambda(R_C) > P_\lambda(R_{MC}) \) and \( P_\lambda(R_C) > P_\lambda(R_{AU}) \).

**Proof.** Obviously,

\[
P_\lambda(R_{MC}) = \lambda P_\theta(R_{MC}) + (1 - \lambda)Q(R_{MC}),
\]
\[
P_\lambda(R_{AU}) = \lambda P_\theta(R_{AU}) + (1 - \lambda)Q(R_{AU}).
\]

By (I), for \( \lambda = 1 \): \( P_\lambda(R_{MC}) > P_\lambda(R_{AU}) \) and, by (II), for \( \lambda = 0 \): \( P_\lambda(R_{AU}) > P_\lambda(R_{MC}) \). As \( P_\lambda(R_{MC}) \) and \( P_\lambda(R_{AU}) \) are linear functions of \( \lambda \), there must be \( \lambda^* \in (0, 1) \) so that \( P_{\lambda^*}(R_{AU}) = P_{\lambda^*}(R_{MC}) \).

Obtain

\[
P_{\lambda^*}(R_{MC}) = P_{\lambda^*}(R_{AU}) \iff \\
\lambda^* P_\theta(R_{MC}) + (1 - \lambda^*)Q(R_{MC}) = \lambda^* P_\theta(R_{AU}) + (1 - \lambda^*)Q(R_{AU}) \iff \\
\lambda^* (\Delta_\theta + \Delta_Q) = \Delta_Q \iff \\
\lambda^* = \frac{\Delta_Q}{\Delta_\theta + \Delta_Q}.
\]
This yields, by help of (IV),

\[ P_{\lambda^*}(R_C) = \lambda^*P_{\theta}(R_C) + (1 - \lambda^*)Q(R_C) \]

\[ = \lambda^* [\alpha_{MS}P_{\theta}(R_{AU}|R_{MS}) + (1 - \alpha_{MS})P_{\theta}(R_{MC}|R_{MS}^c)] \]

\[ + (1 - \lambda^*) [\alpha_{MS}^*Q(R_{AU}|R_{MS}) + (1 - \alpha_{MS}^*)Q(R_{MC}|R_{MS}^c)] \]

\[ = \lambda^* [\alpha_{MS}P_{\theta}(R_{AU}) + (1 - \alpha_{MS})P_{\theta}(R_{MC})] \]

\[ + (1 - \lambda^*) [\alpha_{MS}^*Q(R_{AU}) + (1 - \alpha_{MS}^*)Q(R_{MC})] \]

\[ = \frac{\Delta_Q}{\Delta_\theta + \Delta_Q} [\alpha_{MS} \Delta_\theta - \alpha_{MS}^* \Delta_Q] + \alpha_{MS}^* \Delta_Q + P_{\lambda^*}(R_{MC}) \]

\[ = \Delta_Q \left[ -\alpha_{MS} \Delta_\theta + \alpha_{MS}^* \Delta_Q + \alpha_{MS}^* \Delta_\theta + \alpha_{MS}^* \Delta_Q \right] + P_{\lambda^*}(R_{MC}) \]

\[ = \frac{\Delta_\theta \Delta_Q}{\Delta_\theta + \Delta_Q} \left[ \alpha_{MS}^* - \alpha_{MS} \right] + P_{\lambda^*}(R_{MC}) \]

\[ = \frac{\Delta_\theta \Delta_Q}{\Delta_\theta + \Delta_Q} \left[ \alpha_{MS}^* - \alpha_{MS} \right] + P_{\lambda^*}(R_{AU}). \]

\[ \frac{\Delta_\theta \Delta_Q}{\Delta_\theta + \Delta_Q} \left[ \alpha_{MS}^* - \alpha_{MS} \right] \text{ is larger than zero by (I)-(III), so } P_{\lambda^*}(R_C) \text{ is larger than both } P_{\lambda^*}(R_{MC}) \text{ and } P_{\lambda^*}(R_{AU}). \]

\[ \square \]

8 Conclusion

Given that statisticians often emphasize that statistical inference relies on model assumptions, and that these need to be checked, the literature investigating this practice is surprisingly critical. Preliminary tests of model assumptions have in many situations been found to strongly affect the characteristics of subsequent inference and to invalidate the theory based on the very model assumptions the approach was meant to secure. In some setups either running a less constrained test or running the model-based test without preliminary testing have been found superior to the combined procedure involving preliminary MS testing. This is in contrast to a fairly general view among statisticians that model assumptions should be checked; that view is explicitly or implicitly taken in most of the work empirically investigating “correct” or “incorrect” use of statistics in practice, see Section 5. The existence of situations in which performance characteristics rely strongly on whether model assumptions are fulfilled or not has been acknowledged also by authors that were more critical of preliminary testing, and therefore there is a role for model checking. There is however little elaboration of its benefits in the literature. One contribution of the present work is the investigation of combined procedures in a setup in which both distributions fulfilling and violating model assumptions can occur. This is more favorable for combined procedures than just looking at either fulfilled or violated model assumptions in isolation, and we believe that it is appropriate, because MS tests are used for distinguishing situations in which the model assumptions are appropriate from those where they are not, and this is only exploited in a setup where both can happen.

Mainly for this reason we believe that overall the literature gives a somewhat too pessimistic assessment of combined procedures involving MS testing, and that model checking (and drawing consequences from the result) is more useful than the literature suggests. The fact that preliminary assumption checking technically violates the assumptions it is meant to secure is probably assessed
more negatively from the position that models can and should be “true”, whereas it may be a rather mild problem if it is acknowledged that model assumptions, while providing ideal and potentially optimal conditions for the application of model-based procedures, are not necessary conditions for their use.

In any case, this depends on the specific combined procedure and the considered data generating processes. We believe that the focus of model checking is too much on the formal assumptions and not enough on deriving tests that can find the particular violations of model assumptions that are most problematic in terms of level and power. Here is an example from the literature review (Rochon et al., 2012). In terms of power, the two-sample t-test is better than the nonparametric WMW test if the underlying distributions are uniform. This clearly violates the normality assumption of the t-test (despite being asymptotically still correct), and will be picked up by many normality tests. Still it would be a bad decision to use the WMW test instead, even though its assumptions are fulfilled. An optimal combined procedure therefore should involve an MS-test that picks up only those deviations from normality for which the WMW test (or whatever test is chosen as AU test) is actually helpful. The development of MS tests that are better suited for this task and the investigation of the resulting combined procedures is a promising research area.

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