Contents

Chapter 1

Method Comparison Studies

1.1 What is a method comparison study?

The question of properly assessing "agreement" between two or more methods of measurement is ubiquitous, and is commonly referred to as a 'method comparison study'. Published examples of method comparison studies can be found in disciplines as diverse as Pharmacology (Ludbrook, 1997), Anaesthesia (Myles, 2007), and cardiac imaging methods (Krummenauer et al., 2000).

Historically comparison of two methods of measurement was carried out by use of paired sample $t-$test, correlation coefficients or simple linear regression. Statisticians Martin Bland and Douglas Altman recognized the inadequacies of these approaches for comparing two methods of measurement, and proposed this framework with this application in mind. Although the authors also acknowledge the opportunity to apply other, more complex, methodologies, but argue that simpler approaches is preferable, especially when the results must be 'explained to non-statisticians'.

The approach proposed by Roy deals with the question of agreement, and indeed interchangeability, as developed by Bland and Altman's corpus of work.

1.1.1 Purpose of Method Comparison Studies

Different authors focus on different aspects of comparison problem. Carstensen (2010) provides a review of many descriptions of the purpose of Method Comparison studies, several of which are reproduced here.

> “The question being answered is not always clear, but is usually epxressed as an attempt to quantify the agreement between two methods” (Bland and Altman, 1995).
>
> “Some lack of agreement between different methods of measurement is inevitable. What matters is the amount by which they disagree. We want to know by how much the new method is likely to differ from the old, so that it is not enough to cause problems in the mathematical interpretation we can preplace the old method by the new, or even use the two interchangeably” (Bland and Altman, 1999).
>
> “It often happens that the same physical and chemical property can be measured in different ways. For example, one can determine For example, one can determine sodium in serum by flame atomic emission spectroscopy or by isotope dilution mass spectroscopy. The question arises as to which method is better” (Mandel, 1991).
>
> “In areas of inter-laboratory quality control, method comparisons, assay validations and individual bio-equivalence, etc, the agreement between observations and target (reference) values is of interest” (Lin et al., 2002).
>
> “The purpose of comparing two methods of measurement of a continuous biological variable is to uncover systematic differences, not to point to similarities‘” (Ludbrook, 1997).
>
> “In the pharmaceutical industry, measurement methods that measure the quantity of prdocuts are regulated. The FDA (U.S. Food and Drug Administration) requires that the manufacturer show equivalency prior to ap-

> proving the new or alternatice method in quality control" (Tan & Inglewicz, 1999).

In the view of Dunn (2002), a question relevant to many practitioners is which of the two methods is more precise.

While several major commonalities are present in each definitions, there is a different emphasis for each, which will inevitably give rise to confusion. Carstensen (2010) seems to endorse a simple phrasing of the research question that is proposed by Altman and Bland (1983), i.e. "*do the two methods of measurement agree sufficiently closely?*" with Carstensen (2010) expressing the view that other considerations (for example, the "equivalence" of two methods) to be treated as separate research questions. As such, we will revert to other research questions, such as "equivalence of methods" later, focussing on agreement and repeatability of methods.

In many cases the purpose of the study is to calibrate a new method of measurement against a "Gold Standard" method, a known method that is considered most precise in its measurement. In particular, in medicine, new methods or devices that are cheaper, easier to use, or less invasive, are routinely developed. Agreement between a new method and either a traditional reference or gold standard must be evaluated before the new one is put into practice. Various approaches have been proposed for this purpose in recent years. However it must be noted that absence of measurement error should not be assumed for gold standard methods.

1.1.2 Grubbs' Artillery Round Data

To illustrate the characteristics of a typical method comparison study consider the data in Table 1.1 (Grubbs, 1973). In each of twelve experimental trials, a single round of ammunition was fired from a 155mm gun and its velocity was measured simultaneously (and independently) by three chronographs devices, identified here by the labels 'Fotobalk', 'Counter' and 'Terma'.

An important aspect of the these data is that all three methods of measurement are

Round	Fotobalk [F]	Counter [C]	Terma [T]
1	793.8	794.6	793.2
2	793.1	793.9	793.3
3	792.4	793.2	792.6
4	794.0	794.0	793.8
5	791.4	792.2	791.6
6	792.4	793.1	791.6
7	791.7	792.4	791.6
8	792.3	792.8	792.4
9	789.6	790.2	788.5
10	794.4	795.0	794.7
11	790.9	791.6	791.3
12	793.5	793.8	793.5

Table 1.1.1: Velocity measurement from the three chronographs (Grubbs 1973).

assumed to have an attended measurement error, and the velocities reported in Table 1.1 can not be assumed to be 'true values' in any absolute sense.

A method of measurement should ideally be both accurate and precise. Barnhart et al. (2007) describes agreement as being a broader term that contains both of those qualities. An accurate measurement method will give results close to the unknown 'true value'. The precision of a method is indicated by how tightly measurements obtained under identical conditions are distributed around their mean measurement value. A precise and accurate method will yield results consistently close to the true value. Of course a method may be accurate, but not precise, if the average of its measurements is close to the true value, but those measurements are highly dispersed. Conversely a method that is not accurate may be quite precise, as it consistently indicates the same level of inaccuracy. The tendency of a method of measurement to consistently give results above or below the true value is a source of systematic bias. The smaller the systematic bias, the greater the accuracy of the method.

In the context of the agreement of two methods, there is also a tendency of one measurement method to consistently give results above or below the other method. Lack of agreement is a consequence of the existence of 'inter-method bias'. For two methods to be considered in good agreement, the inter-method bias should be in the region of zero. A simple estimate of the inter-method bias is given by the differences between pairs of measurements, for example, in Table 1.2 shows possible inter-method bias; the 'Fotobalk' consistently recording smaller velocities than the 'Counter' method.

The absence of inter-method bias is, by itself, not sufficient to establish that two measurement methods agree. The two methods must also have equivalent levels of precision. Should one method yield results considerably more variable than those of the other, they can not be considered to be in agreement. Hene, method comparison studies are required to take account of both inter-method bias and difference in the precision of measurements.

1.1.3 Agreement

Bland and Altman (1986) defined perfect agreement as the case where all of the pairs of measurement data, when plotted on a conventional scatter-plot, lie along the line of equality, where the line of equality is defined as the 45 degree line passing through the origin, (i.e. the line $X = Y$ on the Cartesian plane).

To carry their idea a step further, we define a specific numerical measure of agreement as twice the expected squared perpendicular distance of the pair of random variables (X_1, X_2) to the line of equality or agreement in the (X_1, X_2)-plane, that is, $E(X_1 - X_2)/2$, where X_1 and X_2 denote the continuous measurements of method 1 and method 2, respectively. Obviously, other L_p norms may be considered for the purpose of numerically measuring agreement and warrant future consideration.

Agreement is the extent to which the measure of the variable of interest, under a constant set of experimental conditions, yields the same result on repeated trials

Round	Fotobalk (F)	Counter (C)	Difference (F-C)
1	793.8	794.6	-0.8
2	793.1	793.9	-0.8
3	792.4	793.2	-0.8
4	794.0	794.0	0.0
5	791.4	792.2	-0.8
6	792.4	793.1	-0.7
7	791.7	792.4	-0.7
8	792.3	792.8	-0.5
9	789.6	790.2	-0.6
10	794.4	795.0	-0.6
11	790.9	791.6	-0.7
12	793.5	793.8	-0.3

Table 1.1.2: Difference between Fotobalk and Counter measurements.

(Sanchez and Binkowitz, 1999). The more consistent the results, the more reliable the measuring procedure.

Altman and Bland (1983) define bias (referred to hereafter as inter-method bias) as a *a consistent tendency for one method to exceed the other* and propose estimating its value by determining the mean of the case-wise differences. The variation about this mean shall be estimated by the standard deviation of the case-wise differences. Bland and Altman remark that these estimates are based on the assumption that bias and variability are constant throughout the range of measures.

1.2 Improper MCS Techniques

The issue of whether two measurement methods are comparable to the extent that they can be used interchangeably with sufficient accuracy is encountered frequently in scientific research. Historically, comparison of two methods of measurement was carried out by use of paired sample $t-$test, simple linear regression, or correlation coefficients.

1.2.1 Paired sample *t*-test

Bartko (1994) discusses the use of the well known paired sample t test to test for inter-method bias; $H : \mu_d = 0$. The test statistic is distributed a t random variable with $n - 1$ degrees of freedom and is calculated as follows,

$$t^* = \frac{\bar{d}}{\frac{s_d}{\sqrt{n}}} \tag{1.1}$$

where $\bar{d}$ and s_d is the average of the differences of the n observations. Only if the two methods show comparable precision then the paired sample student t-test is appropriate for assessing the magnitude of the bias.

This method can be potentially misused for method comparison studies. Paired $t-$tests test only whether the mean responses are the same, and so provides a useful test for inter-method bias. However, no insight can be obtained about the variability of the case-wise differences by the paired $t-$test, critically undermining it as a stand-alone procedure.

1.2.2 Regression Methods

On account of the fact that one set of measurements are linearly related to another, one could surmise that simple linear Regression is the most suitable approach to analyzing comparisons. However simple linear regression is considered by many Altman and Bland (1983); Cornbleet and Cochrane (1979); Ludbrook (1997) to be wholly unsuitable for method comparison studies. Simple linear regression is defined as such with the name ‘Model I regression’ by Cornbleet and Cochrane (1979), in contrast to ’Model II regression’ models, which shall be discussed later on.

A key assumptions of simple linear regression is that the independent variable values are without random error. For method comparison studies, both sets of measurement must be assumed to be measured with imprecision and neither case can be taken to be a reference method. Arbitrarily selecting either method as reference (i.e. the independent variable) will yield conflicting outcomes: a regression of X on Y would yield an entirely different result from fitting Y on X.

Further criticisms of linear regression exist. Firstly regression methods are uninformative about the variability of the differences. Secondly Regression models are unduly influenced by outliers. Lastly, Regression Models can not be used to effectively analyze repeated measurements.

The Identity Plot

Altman and Bland (1983) states that regression analysis can offer useful insights, and recommending an ‘Identity Plot’, a simple graphical approach that yields a cursory examination of how well the measurement methods agree. In the case of good agreement, the co-variates of the Identity plot accord closely with the $X = Y$ line. This plot is not useful for a thorough examination of the data. O’Brien et al. (1990) notes that data points will tend to cluster around the line of equality, obscuring interpretation. An identity plot shall complement demonstrations of commonly used approaches in the next chapter.

Decomposition of Inter-Method Bias

Regression approaches are useful for a making a detailed examination of the biases across the range of measurements, allowing inter-method bias to be decomposed into constant bias and proportional bias. Regression methods can determine the presence of inter-method bias, and the levels of constant bias and proportional bias thereof Ludbrook (1997, 2002).

Constant bias describes the case where one method gives values that are consistently different to the other across the whole range. Using a naive estimation of bias, such as the mean of differences, it may incorrectly indicate absence of bias, by yielding a mean difference close to zero. This would be caused by positive differences in the measurements at one end of the range of measurements being canceled out by negative differences at the other end of the scale. Proportional Bias exists when two methods agree on average, but exhibit differences over a range of measurements, i.e. the differences are proportional to the scale of the measurement. A measurement method may be subject to any combination of fixed bias or proportional bias, or both (Ludbrook, 2002).

Constant or proportional bias using linear regression can be detected by an individual test on the intercept or the slope of the line regressed from the results of the two methods to be compared. If there is no constant bias, the intercept is equal to zero and, similarly, if there is no proportional bias, the slope is equal to one. Thus, carrying out hypothesis tests on these coefficients (where the null hypotheses are $\beta_0 = 0$ and $\beta_1 = 1$) allow us to test for the presence of both types of bias.

If the basic assumptions underlying linear regression are not met, the regression equation, and consequently the estimations of bias are undermined. Outliers are a source of error in regression estimates.

1.2.3 The Correlation Coefficient

It is well known that Pearson's correlation coefficient is a measure of the linear association between two variables, not the agreement between two variables. Arguments against its usage have been made repeatedly in the relevant literature, with Altman and Bland (1983), Bland and Altman (1986), Bland and Altman (2003) and Giavarina (2015) as examples.

Correlation is wholly inadequate to assess agreement because it only evaluates only the linear association of two sets of observations. Nonetheless this is not necessarily the same as agreement. It is possible for two methods to be highly correlated, yet have poor agreement due to any combination of constant and proportional bias.

The correlation coefficient can be close to 1 even when there is considerable bias between the two methods. For example, if one method gives measurements that are always 10 units higher than the other method, the correlation will be 1 exactly, but the measurements will always be 10 units apart.

The magnitude of the correlation coefficient is affected by the range of subjects/units studied.

The correlation coefficient can be made smaller by measuring samples that are similar to each other and larger by measuring samples that are very different from each other.

1.3 Replicate Measurements and Repeatability

Repeated measurements on several subjects can be used to quantify measurement error, the variation between measurements of the same quantity on the same individual.

Thus far, the formulation for comparison of two measurement methods is one where one measurement by each method is taken on each subject. If the paired measurements are taken in a short period of time so that no real systemic changes can take place on each item, they can be considered true replicates.

Further to Bland and Altman (1999), a formal definition is required of what exactly replicate measurements are

By replicates we mean two or more measurements on the same individual taken in identical conditions. In general this requirement means that the measurements are taken in quick succession.

Roy accords with Bland and Altman's definition of a replicate, as being two or more measurements on the same individual under identical conditions. Roy allows the assumption that replicated measurements are equi-correlated.

Replicate measurements are linked over time. However the method can be easily extended to cover situations where they are not linked over time.

Should enough time elapse for systemic changes, linked repeated measurements can not be treated as true replicates. Should there be two or more measurements by each methods, these measurement are known as ‘replicate measurements’. Carstensen et al. (2008) recommends the use of replicate measurements, but acknowledges the additional computational complexity.

Bland and Altman (1986) address this problem by offering two different approaches. The premise of the first approach is that replicate measurements can be treated as independent measurements. The second approach is based upon using the mean of the each group of replicates as a representative value of that group. Using either of these approaches will allow an analyst to estimate the inter method bias.

However, because of the removal of the effects of the replicate measurements error, this would cause the estimation of the standard deviation of the differences to be unduly small. Bland and Altman (1986) propose a correction for this.

Carstensen et al. (2008) takes issue with the limits of agreement based on mean values of replicate measurements, in that they can only be interpreted as prediction limits for difference between means of repeated measurements by both methods, as opposed to the difference of all measurements. Incorrect conclusions would be caused by such a misinterpretation.

Carstensen et al. (2008) demonstrates how the limits of agreement calculated using the mean of replicates are ‘much too narrow as prediction limits for differences between future single measurements’. This paper also comments that, while treating the replicate measurements as independent will cause a downward bias on the limits of agreement calculation, this method is preferable to the ‘mean of replicates’ approach.

Bland and Altman attend to the issue of repeated measures in 1996.

Bland and Altman discuss two metrics for measurement error; the within-subject standard deviation, and the correlation coefficient.

Roy (2009) accords with Bland and Altman’s definition of a replicate, as being two or more measurements on the same individual under identical conditions. Roy allows the assumption that replicated measurements are equi-correlated. Roy allows unequal numbers of replicates.

Replicate measurements are linked over time. However the method can be easily extended to cover situations where they are not linked over time.

Repeated measurements are said to be linked if a direct correspondence exists between successive measurements across measurements, i.e. pairing. Such measurements are commonly made with a time interval between them, but simultaneously for both methods. Paired measurements are exchangeable, but individual measurements are not.

In this model, the variances of the random effects must depend on m, since the different methods do not necessarily measure on the same scale, and different methods naturally must be assumed to have different variances. Carstensen (2004) attends to the issue of comparative variances.

Bland and Altman (1999) also remark that an important feature of replicate observations is that they should be independent of each other. This issue is addressed by Carstensen (2010), in terms of exchangeability and linkage. Carstenen advises that repeated measurements come in two *substantially different* forms, depending on the circumstances of their measurement: exchangable and linked.

Measurements taken in quick succession by the same observer using the same instrument on the same subject can be considered true replicates. Roy (2009) notes that some measurements may not be 'true' replicates.

Roy's approach assumes the use of 'true replicates'. However data may not be collected in this way. In such cases, the correlation matrix on the replicates may require a different structure, such as the autoregressive order one $AR(1)$ structure. However determining MLEs with such a structure would be computational intense, if possible at all.

One important feature of replicate observations is that they should be independent of each other. In essence, this is achieved by ensuring that the observer makes each measurement independent of knowledge of the previous value(s). This may be difficult *to achieve in practice* (Bland and Altman, 1999).

1.3.1 Exchangeable and Linked measurements

Repeated measurements are said to be exchangeable if no relationship exists between successive measurements across measurements. If the condition of exchangeability exists, a group of measurement of the same item determined by the same method can be re-arranged in any permutation without prejudice to proper analysis. There is no reason to believe that the true value of the underlying variable has changed over the course of the measurements.

Exchangeable repeated measurements can be treated as true replicates. For the purposes of method comparison studies the following remarks can be made. The $r-$th measurement made by method 1 has no special correspondence to the $r-$th measurement made by method 2, and consequently any pairing of repeated measurements are as good as each other.

Replicate measurements are linked over time. However the method can be easily extended to cover situations where they are not linked over time.

1.3.2 Repeatability

Repeatability describes the variation in measurements taken by a single method of measurement on the same item and under the same conditions. A measurement method can be said to have a good level of repeatability if there is consistency in repeated measurements on the same subject using that method. Conversely, a method has poor repeatability if there is considerable variation in repeated measurements.

Repeatability is defined by the IUPAC (2009) as '*the closeness of agreement between independent results obtained with the same method on identical test material, under the same conditions (same operator, same apparatus, same laboratory and after short intervals of time)*' and is determined by taking multiple measurements on a series of subjects. A similar set of criteria is described in the *Guidelines for Evaluating and*

Expressing the Uncertainty of NIST Measurement Results.

Barnhart et al. (2007) emphasizes the importance of repeatability as part of an overall method comparison study, a view endorsed by Carstensen et al. (2008). The repeatability of two methods influence the amount of agreement which is possible between those methods. Before there can be good agreement between two methods, a method must have good agreement with itself. If one method has poor repeatability in the sense of considerable variability, then agreement between two methods is bound to be poor (?Roy, 2009). Bland and Altman (1999) strongly recommends the simultaneous estimation of repeatability and agreement be collecting replicated data. However Roy (2009) notes the lack of convenience in such calculations.

Barnhart et al. (2007) remarks that it is important to report repeatability when assessing measurement, because it measures the purest form of random error not influenced by other factors, while further remarking '*curiously replicate measurements are rarely made in method comparison studies, so that an important aspect of comparability is often overlooked.*

Statistical procedures on within-item variances of two methods are equivalent to tests on their respective repeatability coefficients. A formal test is introduced by Roy (2009), which will discussed in due course.

If replicate measurements by a method are available, it is simple to estimate the measurement error for a method, using a model with fixed effects for item, then taking the residual standard deviation as measurement error standard deviation. However, if replicates are linked, this may produce an estimate that biased upwards.

1.3.3 Types of Comparison Studies

Lewis et al. (1991) categorize method comparison studies into three different types, namely: calibration, comparison and conversion. The key difference between the first two is whether or not a 'gold standard' method is used. In situations where one instrument or method is known to be 'accurate and precise', it is considered as the 'gold standard' (Lewis et al., 1991). A method that is not considered to be a gold standard is referred to as an 'approximate method'. In calibration studies they are referred to as criterion methods and test methods respectively.

1. Calibration problems. The purpose is to establish a relationship between methods, one of which is an approximate method, the other a gold standard. The results of the approximate method can be mapped to a known probability distribution of the results of the gold standard (Lewis et al., 1991). (In such studies, the gold standard method and corresponding approximate method are generally referred to a criterion method and test method respectively.) Altman and Bland (1983) make clear that their framework is not intended for calibration problems.

2. Comparison problems. When two approximate methods, that use the same units of measurement, are to be compared. This is the case w for which Bland and Altman's Methodology is intended, and therefore it is the most relevant of the three for this thesis.

3. Conversion problems. When two approximate methods, that use different units of measurement, are to be compared. This situation would arise when the measurement methods use 'different proxies', i.e different mechanisms of measurement. Lewis et al. (1991) deals specifically with this issue. In the context of this thesis, it is the least relevant of the three cases.

? discusses the relevance of gold Standards in the context of MCS. Currently the phrase 'gold standard' describes the most accurate method of measurement available. No other criteria are set out. Further to **?**, various gold standards have a varying levels of repeatability. Dunn cites the example of the sphygmomanometer, which is prone to measurement error. Consequently it can be said that a measurement method can be the 'gold standard', yet have poor repeatability.

? recognizes this problem. Hence, if the most accurate method is considered to have poor repeatability, it is referred to as a "bronze standard". Again, no formal definition of a 'bronze standard' exists.

Dunn (2002, p.47) cautions that 'gold standards' should not be assumed to be error free and that 'it is of necessity a subjective decision when we come to decide that a particular method or instrument can be treated as if it was a gold standard'. The clinician gold standard, the sphygmomanometer, is used as an example thereof. The sphygmomanometer 'leaves considerable room for improvement'. Pizzi (1999) similarly addresses the issue of glod standards, 'well-established gold standard may itself be imprecise or even unreliable'.

The NIST F1 Caesium fountain atomic clock is considered to be the gold standard when measuring time, and is the primary time and frequency standard for the United States. The NIST F1 is accurate to within one second per 60 million years (NIST, 2009).

Measurements of the interior of the human body are, by definition, invasive medical procedures. The design of method must balance the need for accuracy of measurement with the well-being of the patient. This will inevitably lead to the measurement error as described by Dunn (2002). The magnetic resonance angiogram, used to measure internal anatomy, is considered to the gold standard for measuring aortic dissection. Medical tests based upon the angiogram are reported to have a false positive reporting rate of 5% and a false negative reporting rate of 8% (ACR, 2008).

In literature gold standards are, perhaps more accurately, can be referred to as 'fuzzy gold standards' (Phelps and Hutson, 1995). Consequently, when one of the methods is essentially a fuzzy gold standard, as opposed to a 'true' gold standard, the comparison of the criterion and test methods should be consider both in the context of a comparison study and a calibration study.

According to Bland and Altman, one should use the methodology previous outlined, even when one of the raters is a Gold Standard.

1.4 Outline of Thesis

Thus, the basic concepts of method comparison are introduced. The intention of this thesis is to develop the theory of method comparison studies using Linear Mixed Effects models. Chapter two will provide a review of the prevalent methods, highlighting particular flaws where relevant. Chapter three shall describe Linear Mixed effects models, and how the use of the linear mixed effects models can be extended to method comparison studies. Implementations of important existing work is presented using the `R` programming language.

Chapter three shall describes linear mixed effects models, and how the use of the linear mixed effects models have so far extended to method comparison studies. Implementations of important existing work shall be presented, using the `R` programming language.

Model diagnostics are an integral component of a complete statistical analysis. In chapter four model diagnostics are described in depth, with particular emphasis on linear mixed effects models.

In the fifth chapter, important linear mixed effects model diagnostic methods shall be extended to method comparison studies, and proposed methods are demonstrated on data sets that have become well known in literature on method comparison. The purpose is to both calibrate these methods and to demonstrate applications for them. The last chapter deals with robust measures of important parameters such as agreement.

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