Reliability of Vocational Assessment: An evaluation of level 3 electro-technical qualifications

A project report prepared for the Office of Qualifications and Examinations Regulation by City & Guilds in collaboration with Assessment Europe and the Institute for Employment Studies

Sandra Johnson, Assessment Europe

Rod Johnson, Assessment Europe

Linda Miller, Institute for Employment Studies

Andrew Boyle, The City and Guilds of London Institute

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Preface

This report documents the findings of a five-month collaborative research project commissioned by the Office for Qualifications and Examinations Regulation (Ofqual) within its reliability programme. The given replication-based definition of reliability that the project addressed was the following:

> Reliability refers to the consistency of outcomes that would be observed from an assessment process were it to be repeated. High reliability means that broadly the same outcomes would arise. A range of factors that exist in the assessment process can introduce unreliability into assessment results. Given the general parameters and controls that have been established for an assessment process – including test specification, administration conditions, approach to marking, linking design and so on – (un)reliability concerns the impact of the particular details that do happen to vary from one assessment to the next for whatever reason.

City & Guilds provided the response data for the two on-demand computer-delivered multiple-choice tests described in Section 3, and organised the multiple-marker study for the two-section written paper described in Section 4. Rod Johnson and Sandra Johnson analysed the response data for all three tests. Report production was a collaborative effort with the following contributions: Andrew Boyle (Section 1), Rod Johnson (Section 2), Sandra Johnson (Sections 3 and 4), Linda Miller (Section 5). Andrew Boyle was project manager on behalf of City & Guilds.

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Last but definitely not least, the markers who participated in the multiple-marker study deserve special mention for their interest, time and effort – without their cooperation the designed reliability study could not have taken place.

Executive summary

The project reported here is one of a very small number of assessment reliability studies that have been conducted in the vocational education sector in the UK over the past two decades. Previous studies have looked at portfolio assessment or at the sharing of standards between workplace assessors and their internal verifiers in the context of performance assessment. This study was different in that it explored the reliability of written knowledge tests. Of the several hundred vocational qualifications now provided by City & Guilds, the project focused on three high-stakes electro-technical qualifications that were developed for already-qualified electricians to certificate their professional knowledge. Two of the tests were ‘on demand’, delivered online and machine-marked. The third was delivered in traditional paper-based format and marked by electricians with extensive marking experience. In all three cases pass-fail decisions were based on application of single cut-scores. The cut scores were pre-determined in the case of the on-demand tests, but agreed in a post-testing standard setting meeting in the case of the paper-based test.

Generalizability theory was adopted as the framework for reliability estimation in this criterion-referenced context. For the on-demand tests three years’ worth of electronically-stored response data were made available for analysis. In contrast, a designed multiple-marker study was organised for the traditional human-marked test. This was to provide the kind of data required to enable the exploration of effects on assessment reliability of both marker-related and test-related factors, data that the routine operational marking process with its single marking of scripts could not provide.

Reliability coefficients and standard errors of measurement were estimated for candidate assessment in all three cases, and also for marker assessment in the case of the human-marked test. *What if?* analyses were also carried out to estimate the reliability that might be achieved if tests and/or marking procedures were to be modified.

The reliability measures for both the on-demand tests and the human-marked test were satisfactory, if not high. For both types of test the clearest strategy available to improve reliability further would be to increase test length, perhaps in the case of the written test by replacing the existing test with two shorter ones of longer combined length, should that be feasible within financial and logistic constraints.

1 Introduction

1.1 Vocational qualifications

Technical education and training systems in England have histories running back to the industrial revolution (Lang, 1978) and beyond (Evans, 2007). Institutions that will be familiar to modern-day readers had started providing examinations in technical subjects as far back as the 1870s, in particular the Society of Arts, subsequently the Royal Society of Arts (RSA) and now the ‘R’ in the OCR awarding organisation (Watts, 2008), and the City and Guilds of London Institute (Lang, 1978; City & Guilds, 1993).

Vocational education and training (VET) and associated vocational qualifications (VQs) have always appeared to be on a ‘parallel but separate track’ to academic education and examinations. Notwithstanding this, vocational organisations have instigated many high quality educational institutions; for instance, City & Guilds founded Imperial College London in 1907, established the Associated Examinations Board (AEB) in 1953, (AEB is now an integral part of the Assessment and Qualifications Alliance (AQA)), and established the Technician Education Council (TEC) and Business Education Council (BEC) in 1973; BEC and TEC later merged to form BTEC (City & Guilds, 2011a).

Advocates of VQs enumerate their advantages, both in their own right and in contrast to academic qualifications:

> Despite its slow and at times haphazard development, the technical examination system in England has possessed a number of positive features. It offered real opportunities to students for entry and subsequent promotion in their chosen occupations. The examinations offered were more flexible than their school and university counterparts, matching the wide range of crafts, trades, vocations and occupational sectors involved. In addition to written examinations, assessments of practical activity were undertaken in special workshops or science laboratories. Teachers, employers and other key players were more closely involved, with some examinations set by teachers themselves and externally moderated. (Evans, 2008, p.13)

Millions of people achieve VQs every year. Ofqual’s latest ‘qualifications market report’ (Ofqual, 2011a, pp.60-63) shows total qualification achievements by type for the year 2009/10 as follows:

• National Vocational Qualifications (NVQs)	979,000
• Vocationally-related qualifications (VRQs)	2,607,300
• Qualifications & Credit Framework (QCF)	771,300

Very high stakes can attach to some VQs, both for the qualification holders as individuals and for society more broadly. For example, City & Guilds offers a level 2 NVQ in Domestic Natural Gas Installation and Maintenance. Operatives who hold this qualification will be eligible to apply to become a member of one of the UK's Gas Registration Bodies without the need to undertake further independent assessments in the areas covered by the NVQ. In like vein, City & Guilds NPTC Level 2 Award in the Safe Use of Pesticides (QCF) is a legal requirement of the Chemical Regulatory Directorate for anyone applying pesticides on a commercial basis. City & Guilds keeps the National Register of Sprayer Operators, which facilitates Continuing Professional Development to ensure ongoing training.

Both of the above qualifications are examples of 'licences to practise' (LTP) (City & Guilds, 2011b). LTP has been defined as follows:

> The term 'licence to practise' (LTP) refers to any requirements, including professional standards, voluntary or statutory, to which employers and employees in a sector must adhere. (*ibid.*)

LTP is widely used in some countries, including Germany and Demark, and, at state level, the USA, but it is less prevalent in the UK. Whilst some political commentators have bemoaned LTP as bureaucratic and burdensome on employers, others have noted its advantages; these include eliminating unqualified workers from occupations, improving consumer health & safety and confidence, and promoting continuing professional development (CPD) of workers (*ibid.*).

LTP is an umbrella term that can cover several elements, such as criminal records checks, a register of professionals, commitments to ongoing CPD, and so on. VQs, too, can be an important element of LTPs. A VQ that is part of an LTP can form a basis for training and a method of skills recognition amongst workers, and provide a framework for career progression within a licensed industry (*ibid.*). Although LTP is relatively rarely used in the UK, increasing numbers of qualifications are becoming more and more prevalent within their industries.

1.2 Previous VQ reliability research

Assessment in the vocational field is assumed to be quite heavily focused on performance assessment in the workplace. To a great extent this is true. Workplace assessors might be site managers, senior care home assistants, hairdressers, electricians, and so on; they can equally be peripatetic tutors and/or assessors who travel to workplaces from training provider organisations or colleges. Workplace assessors typically assess the growing and final competence of their charges against the appropriate set of national occupational standards (NOS), using detailed criterion-referenced assessment schemes (for an example see Harth & Hemker, 2012). Criterion-referenced assessments of performance lead only to the judgement that the candidate is competent or not yet competent.

Typically, workplace assessors observe candidates as they perform the kinds of task that might be required in the course of carrying out the occupation concerned. The assessment might be of a process or of an end-product, such as a project write-up or a repaired windscreen. This might involve assessment of the activity as it occurs, or a record of that process (e.g. a video), and equally the outputs from that process may be examined at the point of production (e.g. the repaired windscreen) or some time after the activity has been completed (e.g. as a record – perhaps a photograph – contained within a portfolio).

Assessors are required to ensure that candidates produce sufficient evidence "to enable reliable and consistent judgements to be made about the achievement of all the learning outcomes against the stated assessment criteria" (Ofqual, 2008a, p.26). Internal verifiers are responsible for ensuring that assessors carry out proper procedures, and apply assessment criteria appropriately. External verifiers, who are appointed by the awarding bodies, are responsible for "ensuring that assessment decisions are fair, consistent and meet the requirements set out by the national occupational standards" (Harth & Hemker, 2012, p.329).

Assessment reliability was never a central concern in the vocational system, assessment validity being considered paramount (Jessup, 1991). It was not that reliability was considered an unimportant aspect of assessment, but rather that it was considered to be guaranteed by the specificity of the competence-based assessment criteria that were drawn up for each qualification. Not surprisingly in light of this, the issue of reliability has been little researched in this field (Johnson, 2011). A handful of portfolio-based studies have been conducted (Murphy et al. 1995; Greatorex, 2005; Johnson, M., 2008), but their very small scale has meant that no meaningful quantification of reliability achieved could be produced and generalised.

Interest in the reliability of performance assessments is growing, however. For example, the reliability of performance assessment has been explored in the context of postgraduate medical education, using generalizability theory (see Murphy et al., 2009), while Harth and Hemker (2012) explored agreement rates between workplace assessors and internal verifiers working in the same centres. Quite large numbers of candidate portfolios were examined in the study, covering three different qualifications, one of them an electro-technical qualification and two hairdressing qualifications. Agreement rates were very high. However, the researchers were unable to look at agreement rates between workplace assessors in one workplace and internal verifiers working in another, or between internal verifiers working in different companies. Such studies would be particularly revealing, but would require specially designed studies that would be quite complex to organise.

Apart from these exceptions, most of the reliability investigations that have been undertaken previously have focused on academic school leaving examinations, and almost uniquely on the issue of inter-rater reliability (see, for example, Meadows & Billington for a recent review). No prior research has been published for written testing in the vocational sector. Yet knowledge tests are now fairly common here, forming a component, often the only component (as end-of–unit tests), in assessment for many vocational qualifications. The project described in this report is one of two funded by Ofqual to look at the reliability of VQ knowledge tests (for the other project see Boyle & Rahman, 2012).

1.3 Research aims for this project

The specification for this project set out the following research requirements:

- The selection of one or more vocational/occupational subjects for study
- The identification of sources of unreliability to be explored in the selected vocational/occupational assessments
- The compilation of the necessary data for analysis or the design of experiments to collect data for analysis
- The development of a mechanism for quantifying the reliability measures and the standard error of measurement for the selected assessments
- The analyses, interpretation and reporting of the reliability evidence generated.

Subsumed within the above requirements, the project objectives were to:

- identify sources of unreliability that potentially affect the fate of candidates in individual units within particular selected vocational qualifications
- quantify the contributions to measurement error, i.e. to unreliability, of the identified sources
- estimate the reliability, and standard error of measurement, of each unit on the basis of the empirical findings about measurement error contributions
- estimate (where possible) the reliability of whole qualifications, using the information about component unit reliability

- identify how reliability of units and of whole qualifications might be improved, should improvement appear necessary and achievable within logistics, budgets and routine operational procedures
- illustrate through application an appropriate approach to estimating and improving the reliability of units and the qualifications to which they contribute
- identify ways in which ongoing data planning, archiving and retrieval might be improved within City & Guilds (and other vocational qualification providers) to more readily meet what will inevitably be increasing future demands for the kind of assessment research that is essential to assure assessment quality in this field.

The given definition of reliability for the project is the following:

> Reliability refers to the consistency of outcomes that would be observed from an assessment process were it to be repeated. High reliability means that broadly the same outcomes would arise. A range of factors that exist in the assessment process can introduce unreliability into assessment results. Given the general parameters and controls that have been established for an assessment process – including test specification, administration conditions, approach to marking, linking design and so on – (un)reliability concerns the impact of the particular details that do happen to vary from one assessment to the next for whatever reason.

It was intended that the project would adopt generalizability theory (G-theory) as the theoretical basis for its analytical framework (Cronbach, Gleser, Nanda & Rajaratnam, 1972; Brennan, 2001; Cardinet, Johnson & Pini, 2010).

1.4 The qualifications selected for evaluation

Given the experience of previous VQ reliability researchers, we proposed to investigate qualifications and assessments with the following properties:

- The qualifications would be high-stakes for candidates and users.
- Relatively large amounts of data would be readily available electronically.
- The qualifications structure would be relatively simple, with no optionality.

A suite of electro-technical qualifications satisfied these criteria. The qualifications are intended for already-qualified electricians, and essentially offer CPD opportunities leading to confirmation of specific types of specialised professional knowledge. They are also qualifications for which City & Guilds receives high numbers of appeals.

The qualifications are the following:

1) Level 3 Certificates for the Code of Practice for In-Service Inspection and Testing of Electrical Equipment (2377):

- Level 3 Certificate in Management of Electrical Equipment Maintenance 100/4338/X
- Level 3 Certificate for the Inspection and Testing of Electrical Equipment 100/4339/1

These one-unit qualifications:

> … are aimed at those with administrative responsibilities for the maintenance of electrical equipment and for those undertaking practical inspection and testing of electrical equipment. It also allows those with an administrative responsibility for the testing and inspection of electrical equipment to gain a qualification suitable to their job role. (City & Guilds, 2008a, p.6)

The single unit in each of these qualifications is assessed using an online on-demand multiple-choice knowledge test.

2) Level 3 Certificate in the Certification of Electrical Installations (inspection, testing and certification of electrical installations) (2391-10)

This two-unit qualification was developed:

> … to satisfy the requirements for Proposed Qualified Supervisors (PQSs) for various scheme operatives to ensure they are conversant with the requirements of BS7671 for inspection, testing and certification of electrical installations (City & Guilds, 2008b, p.6)

Unit 301 was assessed using a written examination while unit 302 was assessed through a practical task. For logistic reasons to do with the constraints of operational delivery, unit 302 was not included in this project – the research therefore examined unit 301 only.

There is no absolute statutory requirement for electricians to hold these qualifications, but nonetheless they are widely used within the electrical sector to indicate competence in areas beyond that certified by the NVQ and effectively constitute a standard that electricians need in order to work in certain areas. Membership bodies encourage their affiliates to hold these qualifications – they see it as a form of professional development for the highly skilled electricians that they seek to support. Employers also use these qualifications; for example, the 2391 number is often quoted on job adverts and used as a benchmark indicator – for instance, as a sifting tool by HR departments.

The 2377 suite is continually reviewed and updated in accordance with the Institution of Engineering and Technology (IET) Code of Practice for In-service Inspection and Testing of Electrical Equipment and other industry related regulations. The average number of candidates registering for assessment per annum for the 2377-100 and 2377-200 qualifications are around 2,000 and 13,000, respectively.

The 2391 suite had approximately 11,000 candidate entries per annum across the two units. The suite has recently been reviewed and updated in accordance with the

IET Wiring Regulations: 17th Edition, Amendment 1 and the QCF, and has as a result been split into two qualifications from 2012:

- 2394 Level 3 Award in the Initial Verification and Certification of Electrical Installations
- 2395 Level 3 Principles, practices and legislation for the periodic inspection, testing and condition reporting of electrical installations

For each of these qualifications assessment is now based on the use of computer-generated multiple-choice tests and a practical task.

Before considering the details of the research carried out for the selected qualifications we offer in Section 2 a general overview of the concept of 'reliability', and in doing so identify the particular interpretation of reliability that has been applied in this project.

In Section 3 we consider the machine-marked on-demand tests of the 2377 qualifications before moving on in Section 4 to look at the traditional human-marked unit test of qualification 2391. In Section 5 we summarise findings, and offer a relatively broad interpretation. Finally we draw out some possible implications for future practice within City & Guilds, for the assessment of these and related qualifications.

2 Reliability

Practically all modern treatments of the reliability of educational assessment are built on the conceptual foundations of classical measurement theory, which are long established and extremely well documented. We limit discussion here to a few issues which we have found to be most relevant to questions of vocational assessment, but which do not do justice to a century of thoughtful scholarship. For more extensive treatment, the reader is referred to the classic formulation of Lord and Novick (1968) and to more recent overviews, among which we suggest Haertel (2006), Johnson and Johnson (2012a), Meyer (2010) and Raykov and Marcoulides (2011).

In the basic formulation of the theory a test is submitted to a group of individuals. The 'test' can in fact be any assessment instrument designed to measure some aspect of an individual's performative or cognitive capacity, often evaluated by an observer: for example playing a designated piece on a musical instrument, baking a cake, changing a dressing on a wound, or responding to a series of multiple-choice questions. In deference to tradition, we adopt the conventional term 'test' for any such assessment instrument and 'marker' for the observer and, reflecting the emphasis here on vocational assessment, 'candidate' for the individual being tested.

Tests of the kind we are concerned with are typically imperfect, in the sense that if they could plausibly be repeated we would not necessarily expect exactly the same outcome, or *score*, for any individual on each repetition. Technically, the theory of measurement attributes the differences between (hypothetical) replications of the same test (or between applications of two or more equivalent test *forms*) to *measurement error*. The difference on any given replication between the observed score and the measurement error is what measurement professionals call the *true score*.

The reasoning in the previous paragraph leads to the well-known fundamental equation of measurement theory:

{observed score} = {true score} + [measurement error},

or, in the more familiar symbolic form:

[1] $X = \tau + E$

It would actually be more accurate to write [1] as

[1a] $X_f = \tau + E_f$

using the subscript *f* to distinguish between particular test replications or, more plausibly, equivalent test forms. Note that the true score, $\boldsymbol{\tau}$, does not carry a subscript, because the theory depends on an individual's true score remaining constant across test replications.

To complete the picture, we need to extend [1a] to reflect the fact that tests are usually applied simultaneously to a number of candidates, giving us:

[1b] $X_{cf} = T_c + E_{cf}$

In words, [1b] says that the observed score, *X*, of candidate *c* on test form *f* is equal to *c*'s true score plus some measurement error associated with *c* taking test form *f*. We have used the upper case T_c for the true score, reflecting the fact that we are now dealing with a variable, defined over a population of candidates.

To develop the theory we need a few simple and reasonable assumptions (see for example Johnson and Johnson, 2010), notably that the expected value (i.e. the hypothetical long-run average) of an individual's observed score is the same as the true score (so that the expected value of the measurement error is zero), and that observed scores are not correlated with measurement error. Note that, contrary to what is sometimes claimed, we do not need to assume that measurement errors are normally distributed.

Using these assumptions, it can be easily shown that the relation of equation [1] between observed scores, true scores and measurement error extends also to the variances of the three quantities, namely

[2] Var(X) = Var(T) + Var(E)

Equation [2] is more usually written, using the standard notation σ_Y^2 for the variance of some variable *Y*, in the form

[2a] $\sigma_X^2 = \sigma_T^2 + \sigma_E^2$

Equation [2] says that the observed variation in candidates' scores on a test can be partitioned into variation among individuals' true scores, and variation due to measurement error.

What, though, has all this to do with reliability? To see what an answer might be, we need to revisit a few more of the standard results of measurement theory. The results are simply restated here without derivation or proof; we refer the interested reader to any introductory text on measurement theory, including those cited above.

In effect, given [1] and [2] and the associated assumptions, all of the quantities listed below in Table 2.1 are defined as indicators of reliability, and each is equal to any of the others:

ρ_{XT}^2	the squared correlation between true score and observed score
$\rho_{XX'}$	the correlation between the scores on a test *X* and on a second equivalent test *X'*
ρ_I	the *intraclass correlation*, the correlation between the scores of two candidates with the same true score;
$\sigma_T^2/(\sigma_T^2+\sigma_E^2)$	the ratio of true score variance to true score variance plus error variance
σ_T^2/σ_X^2	the ratio of true score variance to observed score variance
$1-\sigma_E^2/\sigma_X^2$	the complement to unity of the ratio of error variance to observed score variance

Table 2.1: Equivalent forms of reliability index

Table 2.1 is not exhaustive: a few simple algebraic manipulations involving these quantities and equation [2] can yield any number of equivalent expressions: $(\rho_{XT}^2.\sigma_X^2)/(\sigma_T^2+\sigma_X^2)$ could be one, for example.

The indices in Table 1 only refer to whole tests, whereas in real situations we more often than not have to deal with composite tests made up of several parts, including the special case of a test consisting of a number of items. Specifically, suppose that a test score *X* is made up of *k* item scores *Y*$_1$, *Y*$_2$, …, *Yk*, so that

$$X = Y_1 + Y_2 + \cdots + Y_k$$

Suppose further that all the items Y_j have the same variance and the same reliability ρ_Y. Then the reliability of the composite test X can be shown to be:

[3] $\rho_X = (k\rho_Y)/(1+(k-1)\rho_Y)$

Equation [3] is a generalisation of the Spearman-Brown prophecy formula (Spearman, 1910; Brown, 1910), which underlies many of the most influential results in reliability theory, including the derivation of coefficient alpha (Cronbach, 1951), undoubtedly the most frequently used reliability index. It is also the precursor to the 'what-if' analyses of G-theory, as illustrated in Chapters 3 and 4 of this report.

Now that we see how a reliability index is constructed, we can begin to understand what it might be telling us. Take, for instance, the variance ratio $\sigma_T^2/(\sigma_T^2+\sigma_E^2)$. What we see from this is that there are potentially two sources of variation which can contribute to reliability: true score variance and error variance. Thus, a reliability of, say, 0.7 suggests that 30% of total variation in observed scores of a test is due to measurement error

It also follows from the variance ratio formulation of reliability that a low level of variation among the true scores of candidates for a test will tend to lower the reliability of the test, (this is a somewhat counterintuitive result, which we shall return to several times in this report). Alternatively, a low level of error variance compared to true score variance will tend to increase reliability as measured by any of these indicators. In fact, this second behaviour is closer to what we naturally anticipate, because intuitively we expect that measurement error should be what reliability is all about: the smaller the measurement error associated with a test the closer a candidate's score is likely to be, on average, to the 'true' score; and hence the greater the confidence we can have in the test as an accurate reflection of the attribute being measured. An alternative approach to reliability, then, is to seek ways to find the amount of variability in the errors, by using [2] to estimate the error variance, or its square root, called the *standard error of measurement*, or *SEM*. In this perspective, the aims of measurement theory might more appropriately be characterised as

a) to design assessment strategies which minimise the amount of measurement error;
b) where measurement error cannot be eliminated, to find ways of quantifying the amount of error in a test.

Note that, in the logic of (a), investigations into the reliability of a test may be more effective when carried out before the test is used operationally than *a posteriori*, when the test has already been used in a practical setting. We revisit this question later.

In practice, though, more effort has been expended on finding ways of reporting the reliability of a test, typically of one that is already in use. Indeed, a brief glance at the literature on reliability can soon reveal a bewildering variety of reliability coefficients, most of which reduce essentially to one of the equivalent forms listed above, depending essentially on the estimation strategy used. So, correlational definitions, like $\rho_{XX'}$, suggest procedures like test-retest or split halves, which involve comparing the observed results of tests regarded as 'equivalent'; while definitions in terms of ratios of true score and error variances lead naturally to 'internal consistency' estimates of reliability based on a single test administration.

Unfortunately, all the reliability coefficients of Table 1 are fairly blunt instruments, based as they are on the simple relationship expressed in Equations [1] and [2]. They may serve as a starting point for reasoning about reliability, but they give us little help in figuring out how we can disentangle from all of the circumstances surrounding the construction, administration and scoring of a test those which contribute to the candidates' true scores and those which are part of measurement error.

In a realistic testing situation, a variety of factors can potentially contribute to variation in observed scores: candidates, test items, the form of the test (short answer, essay, completing a task), markers, occasion of testing (morning, afternoon or evening, beginning or end of training), instruction styles and strategies are among the most important. Orthodox treatments of assessment reliability single out the contribution of just one of these – typically candidates, occasionally markers, rarely any other – as 'true score' and treat everything else indiscriminately as measurement error. As a consequence, however ingenious and sophisticated the mathematics involved in defining and calculating a reliability coefficient, its precision and descriptive power will necessarily be constrained to the effect of a single factor.

There is, however, no reason why we may not extend equation [2] to include additional sources of variation on the right hand side, some of which we may want to treat as being associated with the true score and some with measurement error. For example, consider a testing situation involving observations, Y_{cqmo}, representing marker m rating candidate c on question q at occasion o. We might partition the score variation associated with this situation as

[4] $$\sigma_Y^2 = \sigma_c^2 + \sigma_q^2 + \sigma_m^2 + \sigma_o^2 + \sigma_R^2 \ .$$

Note that, as well as terms representing the variance of candidates, questions, markers and occasions, we have added a fifth term, σ_R^2, on the right hand side of [4] which we call the *residual variance*, taking the place of the error variance in [2]. The residual variance, like the error variance in [2], is a catch-all for everything else which does not explicitly appear on the right hand side of the equation. We deliberately avoid using the term 'error' to describe the residual, since figuring out the contribution to measurement error of each of the other terms on the right hand side for a particular testing situation will be the main point of the exercise.

Equation [4], as it stands, suggests that the different sources of variation are not related to each other: that certain types of markers never favour particular candidates, for example, or that there is no consistent relationship between particular types of candidate and specific times of the day (morning, afternoon or evening, for example). A more realistic model would include *interaction* terms, representing effects which vary systematically in the same direction. Interactions between two effects are denoted by two subscripts, like σ^2_{cq}, the interaction variance between candidates and questions, whose magnitude reflects the extent to which the different candidates tend to do well (or badly) on the same questions. Three-way interactions are notated by three subscripts, like σ^2_{cqm}, and so on.

A model whose right hand side includes all possible interactions (except with the residual) is called a *fully crossed* model. Here is the fully crossed version of [4]:

[5] $$\sigma^2_Y = \sigma^2_c + \sigma^2_q + \sigma^2_m + \sigma^2_o + \sigma^2_R + \sigma^2_{cq} + \sigma^2_{cm} + \sigma^2_{co} + \sigma^2_{qm} + \sigma^2_{qo} + \sigma^2_{mo} + \sigma^2_{cqm} + \sigma^2_{cqo} + \sigma^2_{cmo} + \sigma^2_{qmo} + \sigma^2_{cqmo} + \sigma^2_R$$

We often refer to an equation like [5] as a *design*, in deference to an extensive literature on experimental design, in which the partition of variance plays a major role.

Note that with the kind of data with which we typically have to deal it is generally not possible to disentangle the residual variance from the highest-order interaction variance (i.e. σ^2_{cqmo} in [5]). We normally expect the effects of these two sources of variation to be conflated, or *confounded*, and consequently we use either notation to refer to both simultaneously. We return to this question in a little more detail below.

Recall that our goal is to distinguish between sources of variation generated by differences in true scores and those which are part of measurement error. To illustrate the point, we need to use an example which is less complicated than [5]. Our example, [6], distinguishes just two sources of variation, candidates and questions:

[6] $$\sigma^2_Y = \sigma^2_c + \sigma^2_q + \sigma^2_R \ ,$$

where the residual variance, σ^2_R, is, as above, confounded with the higher-order interaction variance σ^2_{cq}.

Suppose now that [6] corresponds to a situation where we prepare a set of test questions designed to rank candidates, perhaps with a view to sending the best few on a management training course. In this situation, we take the variance between candidates, as usual, as the indicator of true score variation. As regards error variance, we consider only the candidate-question interaction (confounded, of course, with the residual); since we are only concerned with ranking candidates, and all candidates take the same questions we simply discard the question variance altogether as it contributes neither to true score nor to measurement error. The resulting value for the error variance will be called *relative* error variance (because it arises from a design which is intended to rank candidates relative to one another). It corresponds to the notion of *norm-referencing* in test design.

Given values for true score variance and measurement error variance, we can define a corresponding reliability coefficient, which we call the relative reliability coefficient, for a single question:

[7] $\rho_{YRel} = \sigma_c^2 / \left(\sigma_c^2 + \sigma_{cq}^2\right)$.

If there are n_q questions on the test form, we can ramp up [7] from the reliability of a single question to produce a relative reliability coefficient for the full test of n_q questions.

[7a] $\rho_{XRel} = {}^{\sigma_c^2}\!\Big/\!_{\left(\sigma_c^2 + \sigma_{cq}^2/n_q\right)}$

It can be shown (*cf* Cronbach *et al*, 1972) that [7a], originally called $E\hat{\rho}^2$, is algebraically equivalent to Cronbach's coefficient alpha. The relative reliability coefficient, however, in contrast to alpha, can be defined for any design, not just one involving candidates and questions.

Designs like [6] are not just used to rank candidates. Very frequently their purpose may be to evaluate candidates relative to an external criterion (indeed assessment intended for this purpose is called *criterion-referenced*, as opposed to norm-referenced, assessment). Clearly, in terms of the design [6], the only specific provision in the available conditions of testing for linking the assessment to an external criterion is through the questions; the selection of the questions themselves, then, as well as the interaction between questions and candidates, is a potential contributor to measurement error. In this kind of situation, we call the error variance *absolute* error variance, and the associated reliability coefficient an absolute reliability coefficient.

The absolute reliability coefficient for a single question under design [6] is

[8] $\rho_{YAbs} = \sigma_c^2 / \left(\sigma_c^2 + \sigma_q^2 + \sigma_{cq}^2\right)$

As is the case for the relative coefficient, we can ramp up [8] using [3] to produce the absolute reliability coefficient for the whole test of n_q questions:

[8a] $\rho_{XAbs} = {}^{\sigma_c^2}\!\Big/\!_{\left(\sigma_c^2 + \sigma_q^2/n_q + \sigma_{cq}^2/n_q\right)}$

The coefficient [8a] for the design [6] is equivalent to the Φ (phi) coefficient (Brennan and Kane, 1977).

The coefficients defined in [7], [7a], [8] and [8a] are abstract quantities, expressed in terms of generally unknown population variances. In practical applications they have to be estimated by replacing the variance components σ_ω^2, for each sequence of subscripts ω, by their corresponding estimates $\hat{\sigma}_\omega^2$. It is not always evident that appropriate estimation strategies exist, nor do observed data always permit estimation of the chosen index. The issues surrounding estimation of variance components in all but the simplest cases are difficult and complex, and beyond the scope of this report. Brennan (2001a) gives what is probably the most thorough treatment, though interspersed with discussion of many aspects of the dependability of assessments. Searle, Casella and McCulloch (2006) remains the defining treatment of the topic of variance component estimation in general.

A special, but important and frequent, case of criterion-referenced assessment arises in constructing tests which are intended to place candidates on either side of one or more ordered threshold values (pass-fail, allocation of examination grades, accept-reject, and so on). A common use of such tests occurs in *mastery testing*, designed to determine whether candidates have achieved a particular skill or competence. Where the underlying test yields a numerical score, it is usual to define a fixed *cut score* as the threshold above which mastery can be deemed to have been achieved.

Writing λ for the cut score, it can be of interest to construct a reliability-like index for the difference score $T_c - \lambda$, rather than for a candidate's true score, T_c, as would be the case for a general reliability coefficient. We can achieve this by substituting the candidate (true-score) variance in [8a] with the variance of $(T_c - \lambda)$,

[9] $$Var(T_c - \lambda) = \sigma_c^2 + (\tau - \lambda)^2.$$

.

The result is the index known today as $\Phi(\lambda)$ (phi(lambda))

[10] $$\Phi(\lambda) = {(\sigma_c^2 + (\tau - \lambda)^2)}\Big/{\left(\sigma_c^2 + (\tau - \lambda)^2 + \sigma_q^2/n_q + \sigma_{cq}^2/n_q\right)}$$

We say 'known today' since, when $\Phi(\lambda)$ was introduced by Brennan and Kane (1977), it was originally called $M(C)$. Somewhere along the line it was renamed $\Phi(\lambda)$.

Some caution is required in estimating [10] from observed data, because while the variance components can reasonably be replaced by their sample counterparts, simple substitution of the sample mean, $\bar{X}$, as the ‘natural’ estimator of τ actually yields a biased estimate of $(\tau - \lambda)^2$. The unbiased estimate of $(\tau - \lambda)^2$ is $(\bar{X} - \lambda)^2 - \sigma_{\bar{X}}^2$. If in doubt, it is probably safer to use a reputable software package to do the estimation. Another potential pitfall is that the standard derivation of $\Phi(\lambda)$ requires that τ, and hence λ, be on the item score, rather than the test score, metric. When using standard software, we should expect to have to scale λ accordingly. Yet another issue is that $\Phi(\lambda)$ increases with the size of the distance between the cut score λ and the population mean score τ. Consequently, we can always make our test look more ‘reliable’ by further distancing the cut score from the mean, up to a limit of 100% (or, theoretically, zero).

In fact, we should be cautious about reporting any reliability or reliability-like coefficients for criterion-referenced tests. It can frequently happen that these tests, whose purpose, we recall, is *not* to spread candidates on a scale, are applied to a self-selecting group of candidates whose true scores might be expected a priori to cluster about a value or values corresponding to the criterion around which the test is designed. As a result, candidates’ scores could be predicted to be concentrated in a narrow band, with correspondingly low true score variance. We have already remarked that a reliability coefficient is influenced by two factors: the size of the true score variance and the size of the error variance. In the case we are discussing, then, it could be quite possible that an entirely predictable low true score variance might result in a counter-intuitive low reliability irrespective of the amount of measurement error associated with the test. For this reason, in a context of criterion-referenced assessment, we advise reporting a second quantity, the *standard error of measurement*, or *SEM*, in preference to – or at least alongside – the reliability coefficient.

The standard error of measurement is just the square root of the error variance for the test, and is on the same scale as the mean item score for the test. Just as there is a relative and an absolute reliability coefficient, so there are absolute and relative standard errors of measurement. The two standard errors of measurement corresponding to design [6] are:

$$[11a] \quad SEM_{\bar{X}Rel} = \sqrt{\sigma_{cq}^2 / n_q}$$

$$[11b] \quad SEM_{\bar{X}Abs} = \sqrt{\sigma_q^2 / n_q + \sigma_{cq}^2 / n_q}$$

We have already mentioned that in designs like [5] and [6] above we typically cannot separate out the residual from other components. Informally, the reason is this. Think of a data set of observations associated with [6]. The data can be arranged in a matrix, as they might be displayed in Excel or SPSS, with n_c rows corresponding to the candidates, n_q columns corresponding to the questions, and cells containing the $n_c \times n_q$ scores. The general picture would be something like Figure 2.1

	Question 1	Question 2	$\cdots$	Question n_q
Candidate 1	Y_{11}	Y_{12}	$\cdots$	Y_{1n_q}
Candidate 2	Y_{21}	Y_{22}	$\cdots$	Y_{2n_q}
$\vdots$	$\vdots$	$\vdots$	$\ddots$	$\vdots$
Candidate n_c	Y_{n_c1}	Y_{n_c2}	$\cdots$	$Y_{n_cn_q}$

Figure 2.1: candidate × question matrix

Looking at Figure 2.1, it is clear that there are n_q data points in row 1 that can potentially be used to estimate the score of candidate 1, n_q points in row 2 to estimate the score of candidate 2, and so on. Similarly, there are n_c points in column 1 available for estimating the score for question 1, n_c points in column 2 for computing an estimate for question 2, and so forth. On the other hand, for estimating the effect of interaction between each candidate *c* and each question *q*, we have just one value Y_{cq}. Consequently, there is no information left in the data which we can use to estimate any residual effects independently of the effect of any candidate-question interaction. Technically, we say that the residual is *confounded* with the candidate-question interaction. It would be possible to find independent estimates of the residual effect if there were more than one observation per cell, but such situations are rare in educational assessment. In general, in situations we are likely to encounter, the residual will typically be confounded with the highest order-interaction. This is why we remarked earlier, when discussing variance decompositions like [5] and [6], that we would normally be unable to distinguish between the highest-order interaction variance and the residual variance.

Figure 2.1 shows schematically the data organisation corresponding to a fully crossed design. But we are not always in a position to work with fully crossed data. Take, for example, on-demand tests, typically computer generated, where each candidate is allocated a set of questions generated randomly from an item pool. We can assume that items are extracted from the pool without replacement, so that all candidates within the same session receive different questions. Technically, we say that items are nested within candidates, and we use the notation i:c, as opposed to i × c (which we read as “i crossed with c”). This nesting scheme is a simplified variant of the on-demand tests which we analyse in Chapter 3.

In this simple nested arrangement, we cannot separate an independent item effect from an item-candidate interaction effect. The corresponding design is therefore more simple than the crossed design [6], namely:

[12] $\sigma_Y^2 = \sigma_c^2 + \sigma_{i:c}^2 + \sigma_R^2$

Moreover, because we still only have one observation per cell, just as in the crossed case, we are unable to distinguish the item-candidate interaction from the residual. In this case therefore only one reliability coefficient is available, because effectively on the right hand side we only have one useable term, $\sigma_{i:c}^2$, for our error variance. The reliability for a single item is therefore

[13] $\rho_{YNested} = \sigma_c^2 / \left(\sigma_c^2 + \sigma_{i:c}^2\right)$

The scaled-up coefficient for a test with items nested in candidates is then

[13a] $\rho_{XNested} = \sigma_c^2 / \left(\sigma_c^2 + \sigma_{i:c}^2 / n_i\right)$

And the associated standard error of measurement is

[13b] $SEM_{\bar{X}Nested} = \sqrt{\sigma_{i:c}^2 / n_i}$

Before going on to discuss the application of the principles discussed in this chapter, we conclude with some general remarks on how and why we should be concerned with the reliability of our assessments.

It is normal practice in the testing community to construct tests, to apply them in practice and then, perhaps, to report the reliability of their results. We say “perhaps” because considerations of reliability often come second, for understandable reasons, to the day-to-day business of making sure that the right tests are developed, delivered, marked and the results published, all to very tight schedules. But the need to ensure that our tests are consistent is also important, for consistency is an essential component of fairness and strong moral, ethical and legal pressures should constrain us to ensure that our tests are as fair to candidates as we can possibly make them. In this perspective it makes sense to publish reliability information along with test results whenever possible, so that it can be seen that the test producer is striving to be as fair as possible to consumers of what can often be high-stakes assessments.

While there are laudable reasons for producing reliability statistics along with test results, there are even better reasons for evaluating the reliability of tests, wherever possible, *before* the test is actually administered to candidates, or, where this is logistically impossible, for building into the test administration procedure methodologies for extracting the maximum amount of reliability information from live testing situations.

We claimed at the beginning of this chapter that there are two goals of measurement theory:

a) to design assessment strategies which minimise the amount of measurement error;
b) where measurement error cannot be eliminated, to find ways of quantifying the amount of error in a test.

We suggest that the first goal is equally as important as, and logically prior to the second.

In Chapters 3 and 4 we give the outline of a methodology which can indeed be used in practical situations to improve the amount of reliability in a test.

3 The computer-generated on-demand tests (2377)

3.1 The 2377 certificates and their assessment

In this section we look at a form of assessment that has been introduced relatively recently into examining practice in awarding bodies in the UK, including in City & Guilds, and which is growing in popularity. This is flexible on-demand assessment using computer-generated and computer-marked multiple-choice tests. The two specific tests that we evaluate here are among several hundred end-of-unit multiple-choice tests of knowledge that centres now deliver to candidates online on behalf of City & Guilds (see Boyle & Rahman, 2012, for further examples). As noted in Section 1, the tests belong to single-unit level 3 electro-technical certificates 'for the Code of Practice for in-Service Inspection and Testing of Electrical Equipment' designed to confirm specialised knowledge in two particular areas:

- *Management of Electrical Equipment Maintenance (Certificate 2377-100)*
- *Inspection and Testing of Electrical Equipment (Certificate 2377-200)*

Before accepting candidates onto the 2377-200 course centres are obliged to give them a practical skills test in which they must demonstrate an ability to inspect and test an item of Class 1 equipment (such as an iron, kettle or toaster), and to complete specific record sheets correctly. This practical assessment is the responsibility of centres, and is not investigated here.

Achievement of each of the 2377 qualifications was uniquely dependent on passing an on-demand online end-of-unit multiple-choice test; at the time this project was carried out the 2377-100 test form was composed of 45 items to be answered in 90 minutes, while that for 2377-200 comprised 30 items to be answered in 60 minutes. Tests were created by drawing items from a pre-existing item pool using stratified random sampling, stratification ensuring that given test specifications were met.

In the case of 2377-100 the specification required that the test include a different pre-defined number of items representing each of six learning outcomes (see Table 3.1; note that the number of items representing each learning outcome was itself made up of 1-4 items from each of a series of sub-outcomes). The items were drawn from an item pool that was by 2011 around four times the size of a generated test.

In the case of 2377-200 the specification demanded for each test an equal number of items representing each of five learning outcomes (Table 3.2). In this case the item pool contained around five times the number of items needed in any one test.

Outcome	*Title*	*No. items*
1.1	Law and scope of legislation relevant to the management of electrical equipment maintenance	7
1.2	Types, use and testing of electrical equipment used for in-service inspection and testing	10
1.3	Categories, frequency and practicalities of in-service inspection and testing	12
1.4	Procedures, documentation and user responsibilities that are required for in-service inspection and testing	10
1.5	Training that is required for in-service inspection and testing	2
1.6	Appropriate test instruments and how they are used within in-service inspection and testing	4
	Total items	45

Table 3.1: Test specification for 2377-100 Management of Electrical Equipment Maintenance (*Source*: City & Guilds, 2008a, p.24)

Outcome	*Title*	*No. items*
2.1	Equipment construction	6
2.2	Inspection	6
2.3	Combined inspection and testing	6
2.4	Use of instruments and recording of data	6
2.5	Equipment	6
	Total items	30

Table 3.2: Test specification for 2377-200 Inspection and Testing of Electrical Equipment (*Source:* City & Guilds, 2008a, p.24)

The content covered by the tests was based on the IEE Code of Practice for In-Service Inspection and Testing of Electrical Equipment, and an ’80 per cent correct’ cut-score was applied to distinguish passes from fails for each qualification (i.e. 36 of the available 45 marks for 2377-100 and 24 of the available 30 marks for 2377-200). Several thousand candidates entered for these tests each year.

Until quite recently, these and similar tests were delivered to candidates in paper-based form, with all the candidates in any test session taking the same test. Now test forms are newly generated for individual candidates on demand. Candidates are allowed to access a test at any time from four hours before the scheduled test time to four hours after it, and they are free to retake tests (as different test forms) as many times as they choose within a testing window. When a candidate elects to be assessed a complete set of items is selected for delivery, following the relevant test specification (there is no targeted testing element here). While the majority of candidates work systematically through their allocated items, there are occasionally instances where a candidate gives up before the end of the test or where a technical problem arises that invalidates that particular assessment.

The response datasets supplied for analysis spanned the three-year period from October 2008 to September 2011: for candidate numbers see Table 3.3. The pattern of performance barely differed from one testing window to another, so that in this section we consider each test dataset over the entire period. A total of 5,346 candidates attempted a 2377-100 test over the three-year period; as Table 3.3 shows, after excluding all but the first attempt of resit candidates (i.e. candidates submitting to assessment more than once over the period) and incomplete records (i.e. candidates with fewer than 45 recorded item responses) the number of useable records was 4,646. Over the period, 34,904 candidates were entered for 2377-200 assessment; after excluding second and subsequent resits and incomplete records 28,671 records were available for analysis.

	2377-100	*2377-200*
Registrations	5,346	34,904
Resits*	509	4,737
Too few responses	218	1,799
Useable total	4,646	28,671

** First attempts are included in the analysis total*

Table 3.3: Candidate statistics for 2377-100 and 2377-200 for the period October 2008 to September 2011

The test score distributions in both cases were severely left-skewed, almost all candidates having total scores in the top half of the mark scale, or even in the top third in the case of 2377-100 (see Figures 3.1 and 3.2).

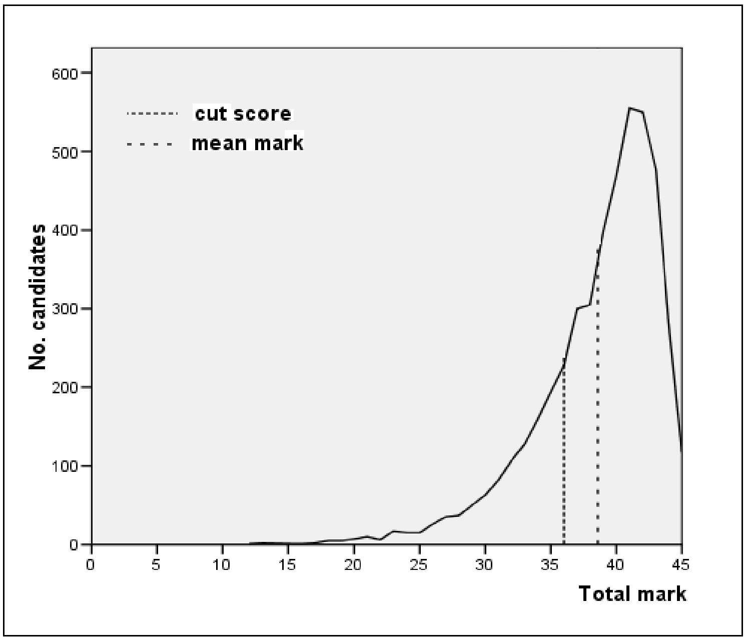


Figure 3.1: The mark distribution for the 2377-100 tests (4,646 candidates over a 3-year period)

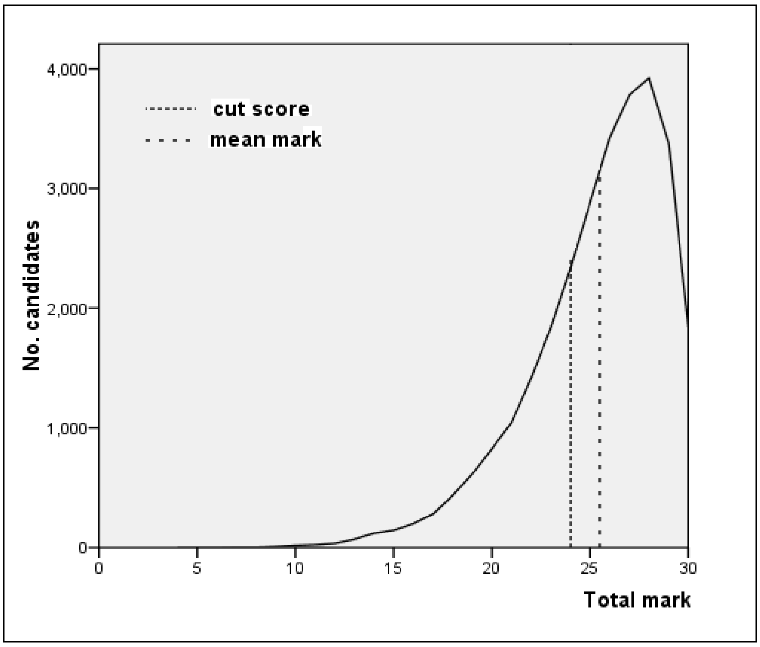


Figure 3.2: The mark distribution for the 2377-200 tests (28,671 candidates over a 3-year period)

The mean score achieved for 2377-100 was 38.6. After application of the cut score (36 marks), 3,680 candidates passed the test and achieved the qualification (a 79.2% pass rate). The mean test score for 2377-200 was 25.4 and the cut score 24. Of the 28,671 candidates in the analysis dataset, 21,575 achieved or exceeded the cut score and so gained the 2377-200 qualification (a 75.3% pass rate).

So, how reliable were these high-stakes tests? This is a central question for this research project.

3.2 An approach to reliability estimation

The 2377 tests are in some sense ‘mastery’ tests, even if the criterion for mastery is not as clear-cut as it might be for narrow skills assessment. The examiners responsible for each of these qualifications made the decision that the success rate required for a candidate to achieve a pass, and therefore to gain the qualification, was to be high, at 80 per cent. In principle, for an individual candidate it is irrelevant how well or how badly other candidates might do on the test. The goal for each candidate is to study the content domain sufficiently well to have the highest possible chance of meeting the ’80 per cent correct’ criterion. Maximally spreading candidates on the score scale to facilitate a separation of ‘successful’ candidates from others is not the aim. Indeed, it would be perfectly acceptable for all candidates at any one time to be clustered above the cut score, and for all therefore to pass the test and achieve the qualification.

The usual aim of knowledge assessment in VQs, and certainly the aim of knowledge testing in the cases of 2377-100 and 2377-200, is not to maximally differentiate among candidates in terms of their test scores, but rather to provide ‘absolute’ information about each candidate in order to serve a pass-fail decision that is referred to the predetermined ‘mastery’ cut score. It follows that Cronbach’s alpha coefficient, which is relevant in norm-referencing application, is not the appropriate measure of reliability to use here, and indeed it cannot even be calculated when not all candidates take the same test items, In principle the appropriate reliability coefficients to use are the phi coefficient and phi(lambda), both of which are fully described in Section 2.

In fact, this model of assessment does not lend itself well at all to the concept of a reliability coefficient, since such coefficients typically estimate the proportion of ‘true score’ variance, in this case between-candidate variance, in total observed score variance, as explained in Section 2. Where candidates might legitimately cluster around a single cut score the between-candidate variance will be low, as in consequence will be any calculated ratio-based reliability coefficient. A better indicator of reliability would be the standard error of measurement (SEM) associated with candidates’ true scores, from which can be calculated confidence intervals around observed test scores. The smaller the standard error of measurement associated with a generic test score the lower will be the risk of candidate misclassification on application of the cut score.

But how to estimate the measurement error? When paper-based testing was the norm and all candidates attempted the same test at the same time, then there would have been a very simple design available for analysis. This is c x i, where c and i represent, respectively, candidates and items, with x indicating that candidates and items are technically ‘crossed’ (as explained in Section 2, this simply means that all the candidates attempt the same test and therefore the same set of items). From a G-study analysis three variance components could be estimated: between-candidate variance (c), between-item variance (i) and candidate-item interaction variance confounded with residual variance (ci, e). This design is illustrated in Figure 3.3.

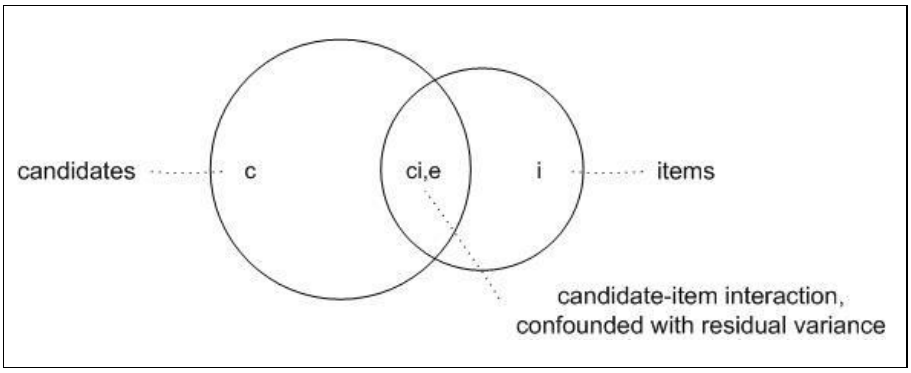

candidates
c
ci,e
i
items
candidate-item interaction,
confounded with residual variance

Figure 3.3: The crossed design c x i, candidates by items

But candidates are not crossed with items here. Different candidates take different sets of items as their 2377 test (their particular test form). If the sets of items delivered to different candidates were non-overlapping then we would have a pure ‘nested’ design in place of the previous crossed design. The nested design is shown in Figure 3.4. In symbols we now have i:c, the colon indicating that items are ‘nested’ within candidates – different candidates are given different items to attempt.

In this situation just two components of variance can be separately estimated: the between-candidate variance, as before, and the now even more confounded residual variance: the between-item variance has joined the candidate-item interaction variance as components in the residual variance. This means that we can no longer isolate the between-item variance for separate quantification. If we *had* been interested in how well either of the tests spread candidates on the test score scale we can no longer do so (we cannot calculate alpha, or the relative G coefficient, which is equivalent to alpha for two-factor designs). But spreading candidates is not the aim, so that nothing is lost in a technical sense. However, one small issue that remains is that we do not actually have pure nesting going on here, as there is almost always some degree of overlap between test forms in terms of common items.

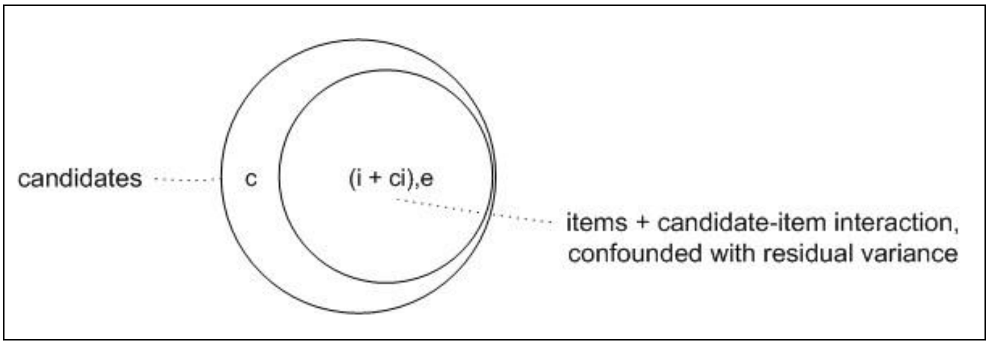

candidates
c
(i + ci),e
items + candidate-item interaction,
confounded with residual variance

Figure 3.4: The nested design i:c, meaning items are nested within candidates

Before presenting the results of the analyses we could usefully consider ‘learning outcomes’ as a potential factor in the analysis design. The test specifications for both the 2377-100 and the 2377-200 tests were designed with a given structure in terms of representation of the different learning outcomes (see Tables 3.1 and 3.2 in Section 3.1). It would be possible in principle to add ‘learning outcomes’ to the nested i:c design, as another nesting factor for items, giving the design i:(co). But the numbers of items included in each test to represent the different learning outcomes are small, and in the case of 2377-100 they also vary across outcomes. Any analysis results would therefore not be well-based. In any case, the factor ‘learning outcomes’ can play no role in terms of measurement error. This is because the six learning outcomes of 2377-100 and the five learning outcomes of 2377-200 account for the totality of the content domain being assessed in the respective qualifications. Outcomes are not sampled in the assessment. ‘Learning outcomes’ is therefore by definition a fixed factor, and as such it cannot contribute to measurement error, either alone or through interaction with candidates. For these reasons the reliability analyses focused on candidates and test items only.

3.3 Analysis results

Before presenting the analysis results, one small issue deserves mention. This has to do with the way that item deliveries and candidate responses were recorded. Candidates’ responses to the items in their test forms were recorded as the answer options that they chose (A to D). But no code was recorded when a candidate did not respond to an item. No ‘test form identifier’ was recorded either. The consequence was that when a candidate had fewer than 45 responses in the dataset for 2377-100, or fewer than 30 for 2377-200, it was impossible to know whether that candidate had reneged on the test at some point, or there had been a technical problem, or certain items had simply not been attempted. Given this, for the purpose of the analyses carried out here, to maximise the validity of results interpretation, all candidates with fewer than the requisite number of item responses were excluded from the dataset.

Data analysis was carried out using both EduG (SSRE, 2006) and urGENOVA (Brennan 2001b). The results for each of the three years separately were virtually identical for both qualifications. Table 3.4 therefore presents the results for the entire 3-year period for both qualifications. As the table confirms, the number of candidates who took a test for one or other of these qualifications in the period was extremely high, particularly for 2377-200.

variance source	*sum of squares*	*degrees of freedom*	*mean square*	*variance component estimate*	*%**
<u>2377-100</u>					
candidates (4,646)	2262.115	4645	0.487	0.008	7
confounded residual	23202.124	204424	0.114	0.114	93
<u>2377-200</u>					
candidates (28,671)	11571.212	28670	0.404	0.009	7
confounded residual	100379.466	831459	0.121	0.121	93

* *Variance component estimates as percentages of total item score variance*

Table 3.4: ANOVA tables for the 2377-100 and 2377-200 tests

We also see from Table 3.4 that in both qualification tests the between-candidate variance accounted for just 7% of the total variance, leading to predictably lowish reliability coefficients – recall that these are variance ratios in which the numerator is the between-candidate variance. For interest we nevertheless offer the values of phi and of phi(lambda) – see Section 2 for the relevant formulae. In computing phi(lambda), the cut score, lambda, conventionally expressed on the mean item score metric, is 0.8 for both of the 2377 tests. This is because while the actual cut scores for the two tests were different – a total test score of 36 in the case of the 45-item 2377-100 test and 24 in the case of the 30-item 2377-200 test – the cut score as a percentage test score in both cases was 80 per cent, giving an average item mark of 0.8 for the binary-scored items.

Using the component information from Table 3.4, we calculate that the phi estimate for the 2377-100 test is 0.77, with phi(0.8) higher at 0.82. The phi estimate for 2377-200 is 0.70 with phi(0.8) at 0.74. These values are consistent with those for the majority of other similar City & Guilds tests analysed in the same way (Boyle & Rahman, 2012). These are modest values, entirely to be expected from peaked test distributions that have arisen by design, since the testing aim is not candidate spread. The 95% confidence intervals around a candidate's total test score are, respectively, ± 4.4 marks and ± 3.7 marks for 2377-100 and 2377-200; corresponding intervals around percentage mean scores are ± 9.8 percentage points and ± 12.4 percentage points, respectively.

While the value of phi is not particularly important in this context, given the expected clustering of candidates around the cut score, we can nevertheless predict its values, and those of phi(lambda), for test forms of different lengths (should longer tests be feasible to develop and to operate). For these particular tests, the *what if?* analyses simply require the substitution of different numbers of test items into the expressions for the reliability coefficient and the SEM given in Section 2 (expressions 13a and 13b, respectively).

Table 3.5 provides the results of this prediction, and also shows how the 95% confidence intervals can be expected to change as item numbers increase. From the results we can deduce that for both qualifications, should an increase in test length be an option, increasing tests to 50 items would produce phi coefficient values of around 0.8 and phi(lambda) coefficients above 0.8.

Simultaneously, the precision of candidate mean scores would improve, with 95% confidence intervals around candidates' percentage mean test scores reducing from the previous value of ± 9.8 percentage points to roughly ± 9.3 percentage points for 2377-100, and from the previous ± 12.4 percentage points to ± 9.6 percentage points for 2377-200. For 2377-100, confidence intervals around test total scores would decrease from 9.8 per cent of the previous nominal 45-mark scale (i.e. ± 4.4 marks) to 9.4 per cent of the new 50-mark scale (± 4.7 marks); for 2377-200 intervals would decrease from 12.3 per cent of the previous 30-mark scale (i.e. ± 3.7 marks) to 9.6 per cent of the new 50-mark scale (i.e. ± 4.8 marks). Increases to 60 items would barely affect the phi coefficient, but would continue to have a positive impact on the 95% confidence intervals around test total scores.

	2377-100				*2377-200*			
			*95% CI**				*95% CI**	
	phi	*phi(0.8)*	*P*	*R*	*phi*	*phi(0.8)*	*P*	*R*
50 items	0.78	0.84	± 9.3	± 4.7	0.80	0.83	± 9.6	± 4.8
60 items	0.81	0.86	± 8.5	± 5.1	0.82	0.85	± 8.8	± 5.3

** Figures in 'P' columns are margins of error associated with percentage mean test scores, and are expressed in percentage points; figures in 'R' columns are margins of error around total test scores, and are expressed in raw marks.*

Table 3.5: Predicted values of phi, phi(0.8) and 95% confidence intervals around percentage mean test scores and test total scores for longer tests

4 The traditional-format unit test (2391)

4.1 Unit 301 and its assessment

Qualification 2391 is a two-unit level 3 certificate entitled *Certification of Electrical Installations (inspection, testing and certification of electrical installations) (2391-10)*. It was developed to serve the needs of already qualified electricians aiming to become qualified supervisors, in confirming their knowledge and understanding of the requirements of BS7671.

Unlike the 2377 units described in Section 3, unit 2391-301 was assessed using a traditional human-marked written examination. The 2½-hour knowledge test comprised a total of 26 questions jointly spanning three knowledge domains: ‘preparation for inspection and testing’, ‘inspection’ and ‘testing’. Section A comprised 20 multi-part short answer questions, each worth three marks, while Section B comprised six structured questions meriting 15 marks each, giving a possible test total mark of 150. Unit assessment was offered on specific dates throughout the year, with all the candidates in any particular series taking the same paper-based test.

Markers attended an initial standardisation meeting before starting their work, and were then periodically monitored over the marking period using a small number of seeded scripts pre-marked by the team leader, as is standard practice in the major awarding organisations at this time. In the operational process centres posted all of their completed scripts directly to their allocated markers, who then marked and forwarded the marked scripts and accompanying mark record sheets to City & Guilds.

Candidates were graded pass-fail on the basis of a cut score determined during a post-assessment standard setting meeting. Like the test itself, the meeting followed a traditional format (see, for example, Robinson 2007 for details). Participants, in this case the chief examiner, a team leader and an examiner, reviewed:

- the current and past examination papers
- statistical evidence about candidate performances on these papers (frequency distributions and summary statistics)
- pass rates for past papers, and pass rates on the current paper for alternative cut score choices (within a few points either way of the previous year’s cut score)
- a handful of candidate scripts in the range of potential cut scores.

A total of 2,915 candidates registered for the December 2011 test session, and 2,401 candidates actually completed the test. The total marks achieved varied from 0 to 136, the mean mark was 83.7 and the standard deviation 22.5. Application of the cut score of 94, arrived at during the post-assessment standard setting meeting, resulted in a pass rate for the test of just over 37 per cent (896 candidates).

The examiners responsible for this unit paper reported that a principal reason for the low pass rate is that year after year many candidates were being entered for the test by their centres when they did not yet have the necessary technical knowledge or appropriate practical experience; improvements in pay and job prospects that typically follow from acquisition of this qualification explain its attractiveness, as does the availability to centres of funding based on enrolments and not outcomes. Other explanations offered by the examiners included inadequate candidate study preparation (intensive cramming rather than systematic study over a period of time), candidate unfamiliarity with classroom-based learning and timed tests, and inadequately trained teachers/tutors within some centres.

Figure 4.1 illustrates the slightly left-skewed mark distribution, and indicates the locations of the mean score and the cut score.

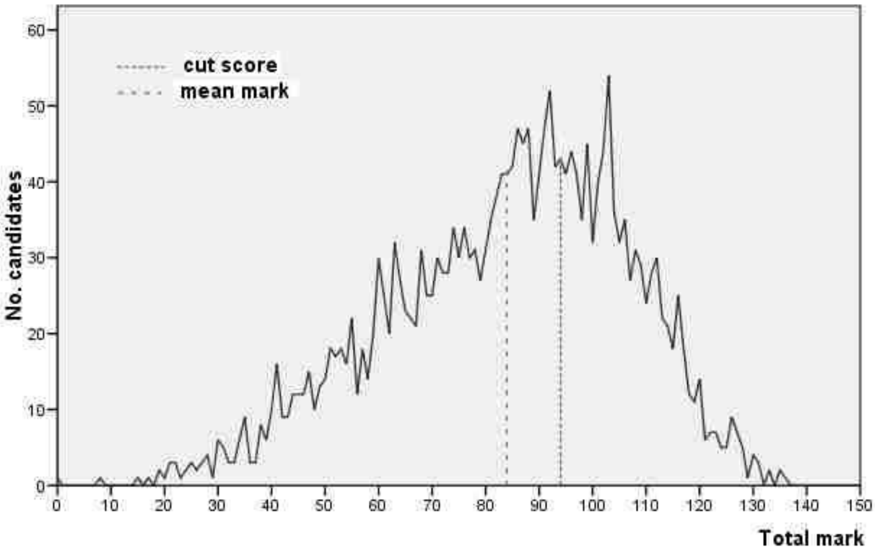


Figure 4.1: The mark distribution for the December 2011 test

The principal question of interest here is how technically reliable was this assessment exercise?

4.2 The multiple-marker study

In the operational situation each candidate is marked by just one marker, and not all markers are required to mark the few seeded scripts that are typically used for the ongoing monitoring of marking standards. The regular operational data do not, therefore, lend themselves to reliability investigation, because potentially important factors, in particular marker effects, are ‘hidden’.

The potential contributions of markers to measurement error include between-marker differences in severity, i.e. differences in general standards of marking. Familiarly known as ‘inter-marker reliability’, this contribution to measurement error is generally small after marker standardisation, particularly when compared with the contributions associated with the implicit question sampling that test development involves. Less well-researched interaction effects involving markers are also pertinent. Among these are marker-question interaction, marker-candidate interaction and marker-time interaction (‘marker drift’):

- Marker-question interaction is where different markers have different opinions of the worth of different questions. At its most simplistic, one marker might give generally higher marks to candidates’ performances on question 1 than on question 2, while another marker might do the reverse.
- Marker-candidate interaction arises when different markers rate different scripts differently, such as one marker marking candidate A higher than candidate B and another doing the opposite (or rating candidate A much higher than candidate B compared with the first marker).
- Marker- occasion interaction is where markers change their relative standards of marking from one occasion to another, for example as they mark from start to finish of an operational marking period.

These are all examples of interactions involving markers that are commonly subsumed under the term ‘intra-marker’ variability.

To provide a dataset that would serve the kind of reliability investigation needed for this type of test, a multiple-marker study was organised, albeit of small scale. The study used candidate scripts and markers from the December 2011 test session. The principal aim of the exercise was to furnish appropriate data for a G-study analysis, from which interpretable and generalisable estimates of assessment reliability might be produced. We needed to be able to quantify the contributions to score variance that could be attributed to candidates themselves, to the questions they attempted, to the markers who marked their responses, and to interactions among these main factors. We were also interested in exploring the issue of possible marker ‘drift’.

A total of 15 markers participated in the operational marking of the 2391 scripts. Of these, 12 markers, including the team leader, agreed to take part in the designed multiple-marker study. The markers participated in the usual marker standardisation meeting organised by City & Guilds for the unit paper (early December 2011), and marked a normal allocation of candidate scripts during the regular operational marking exercise (mid-December 2011 to mid-January 2012). In addition, these volunteers also marked scripts whose marking results would contribute to the multiple-marker study.

The maximum number of additional scripts that it was considered feasible to add to markers’ normal operational allocation was 30. Thus, 30 candidates were randomly selected from the unit 301 entry list, and this selection was in turn divided at random into two groups of 15. Once the relevant scripts became available they were photocopied and batched for delivery to the markers. One batch of 15 scripts was distributed to the study markers at the end of the operational standardisation meeting for at-home marking in the week following the meeting. The second batch of 15 scripts was sent to them by post after the Christmas/New Year break for marking toward the end of the relatively short operational marking period. Mark record sheets were completed by the markers and sent by email to City & Guilds immediately after each of the assigned study marking sessions.

The two sections of questions that comprised the test differed in terms of:

- the type of question they contained (multi-part short-answer questions in Section A, longer structured questions in Section B);
- the number of questions they contained (20 in Section A, six in Section B);
- the mark tariffs attached to the questions (three marks each in Section A, 15 marks each in Section B).

By its nature, Section B would in principle be the most vulnerable to marker-related sources of score variation. For all these reasons it made sense to analyse the two sections separately in the first instance. The analyses were based on question scores and not on part-question scores, even though markers recorded their marking decisions at the level of part questions. The reason is that the mark tariffs associated with question parts differed both within and across questions, thus complicating not only any data analysis but also results interpretation.

For each section the analysis design followed a four-factor mixed model. All the candidates attempted the same test paper and in consequence attempted, or had the opportunity to attempt, all the questions in that paper. So, candidates were 'crossed' with questions. Moreover, the markers who participated in the study marked all the candidate scripts. Markers were, therefore, crossed both with questions and with candidates. This three-factor crossing is reflected in the expression c x q x m, with c, q and m representing, respectively, the factors candidates, questions and markers.

While markers and questions are also crossed with occasions, all the markers marking scripts at the beginning and towards the end of the operational marking period, candidates cannot be. 'Occasions' must be considered as a nesting factor for candidates, because it would not have been meaningful for markers to mark the same candidate scripts on two different occasions so close together in time. There might – certainly would – have been recall effects at play, so that it could never be assumed that the marks given to the question responses in a candidate's script in mid-December 2011 would be quite independently given to those same question responses in mid-January 2012. Thus, inevitably, the scripts marked on the different occasions had to be from different candidates, meaning that candidates were 'nested' within occasions of marking. This nested relationship is denoted as c:o, where c and o represent candidate and occasions, respectively.

The design that therefore underpinned the G-study analysis of each section is:

c:o x q x m

with c, o, q and m representing, respectively, candidates (i.e. scripts), occasions, questions and markers. This design, and the sources of variance that can be separately quantified in the analysis, is shown schematically in Figure 4.2.

The nesting relationship involving candidates and occasions is shown in Figure 4.2 as concentric circles, while crossed relationships are indicated by intersecting circles. The resulting sectors in the diagram represent the potential sources of score variation that can be separately quantified in the analysis – though note that sector sizes bear no intended relationship with the importance of the different variance sources. The variance components open to isolation and estimation here are those for questions, occasions, candidates (within occasions) and markers, along with components associated with the following interaction effects: questions-occasions, markers-occasions, markers-questions, candidates- questions, candidates-markers. The highest order interaction, candidates-questions-markers, is confounded in the residual variance with all unidentified effects and random error.

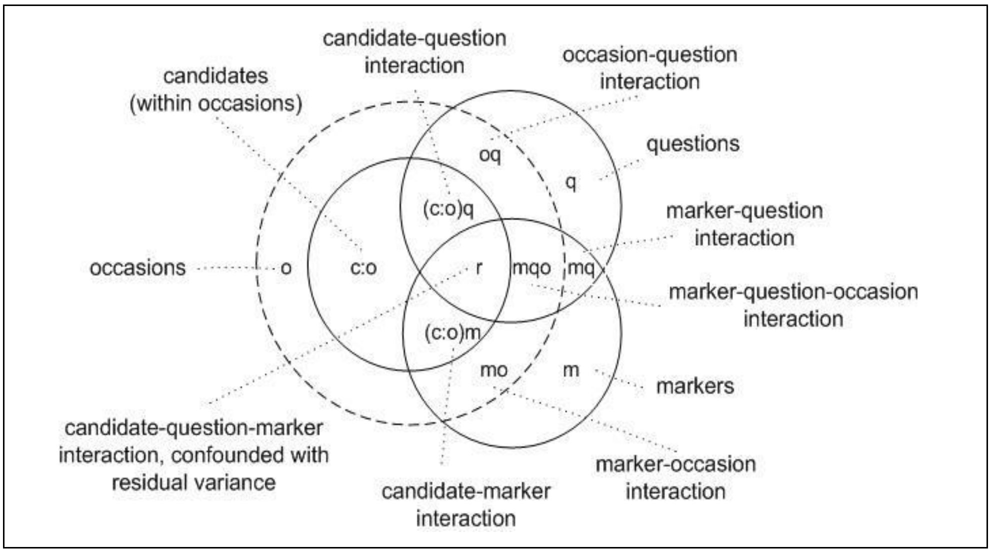

candidate-question interaction
occasion-question interaction
candidates (within occasions)
questions
marker-question interaction
occasions
marker-question-occasion interaction
markers
candidate-question-marker interaction, confounded with residual variance
candidate-marker interaction
marker-occasion interaction
oq
q
(c:o)q
o
c:o
r
mqo
mq
(c:o)m
mo
m

Figure 4.2: The underpinning analysis design for each paper section

4.3 Analysis results

4.3.1 Section results

Tables 4.1 and 4.2 present the analysis results for Section A and Section B, respectively; these were produced using the software package urGENOVA (Brennan 2001b). The first feature to note in both tables must be the small negative variance component estimates. In theory variances cannot be negative. But these are ANOVA-based variance estimates, and if the variances themselves are close to zero then their estimates might emerge with values close to zero in either direction. So that while the variance estimate for ‘occasions’ in Table 4.1 is small and negative, the variance estimate for ‘markers’ is equally small but positive. When negative estimates are very small conventional practice is to set them to zero in follow-on calculations. The same practice is not usually followed in the case of small positive estimates, but it could legitimately be.

The second important point to note in both tables is the absence of any evidence of important marker or occasion effects. Inter-marker variation, though it exists, is extremely small, as are interaction effects involving markers, including marker drift. Note, though, that marker contributions to question score variance are simply being dwarfed here by contributions from other sources, in particular candidates and questions. The identifiable sources of variation that almost all the mark variation in the two datasets can be attributed to are between-candidate variance (higher in Section B than in Section A), between-question variance (higher in Section A than in Section B) and, above all, the candidate-question interaction variance, that accounts for over half the total variance in each case. The residual variance, which contains the marker-candidate-question interaction variance, accounts for 12% of the total variance for each section.

source of variance	*sum of squares*	*degrees of freedom*	*mean square*	*variance component estimate*	*%**
occasions	33.6200	1	33.6200	-0.0018	-
questions	2151.5683	19	113.2399	0.2954	23
markers	17.0083	11	1.5462	0.0016	-
occasion-question interaction	125.7300	19	6.6174	-0.0082	-
marker-occasion interaction	3.3467	11	0.3042	0.0002	-
marker-question interaction	116.7083	209	0.5584	0.0098	1
candidates (within occasions)	1162.1880	28	41.5067	0.1398	11
marker-question-occasion	55.4367	209	0.2653	0.0075	<1
candidate-question interaction	4242.2783	532	7.9742	0.6518	52
candidate-marker interaction	43.6117	308	0.1416	-0.0005	-
confounded residual	891.3883	5852	0.1523	0.1523	12

* *Variance component estimates as percentages of total question score variance*

Table 4.1: ANOVA table for Section A

source of variance	*sum of squares*	*degrees of freedom*	*mean square*	*variance component estimate*	*%**
occasions	94.5852	1	94.5852	-0.3423	-
questions	2767.9444	5	553.5889	1.1761	6
markers	169.1833	11	15.3803	0.0563	<1
occasion-question interaction	638.0315	5	127.6063	0.0101	<1
marker-occasion interaction	29.1926	11	2.6539	-0.0039	-
marker-question interaction	312.7889	55	5.6871	0.1173	<1
candidates (within occasions)	12931.3593	28	461.8343	4.6811	25
marker-question-occasion	169.7241	55	3.0859	0.0612	<1
candidate-question interaction	17482.4963	140	124.8750	10.2256	55
candidate-marker interaction	642.2630	308	2.0853	-0.0137	-
confounded residual	3338.0148	15400	2.1675	2.1675	12

* *Variance component estimates as percentages of total question score variance*

Table 4.2: ANOVA table for Section B

Figure 4.3 shows the compositions of the total question score variance for each section in terms of the main contributing sources.

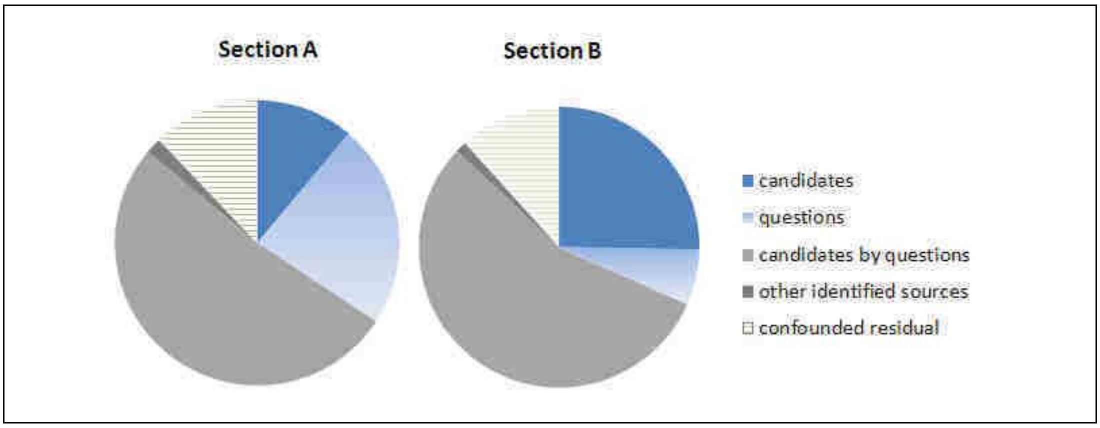


Figure 4.3: The composition of the total question score variance for Sections A and B

The high interaction variance reflects the fact that different candidates found the different questions in each section harder or easier than others to differing degrees. It was not the case that one candidate did better on all the questions than some other candidate, and to the same extent. The candidates' performance profiles were 'jagged'. Again, the examiners responsible for this paper threw useful light on this phenomenon. The 2391 qualification certifies electricians to work in various different environments – domestic, industrial, commercial. For this reason questions related to different environments are included in the examination. Perhaps not surprisingly, candidates perform differently on different questions based on their experience. For some questions the answer is the same regardless of the context in which it is set, but due to lack of experience in the environment under question some candidates struggle and give a wrong answer. Other related factors already mentioned are candidates being enrolled too early, with varying levels of knowledge and experience, and teachers/tutors being themselves insufficiently trained.

Using the component estimates in Tables 4.1 and 4.2 we find that *for the data from 12 independent multiple markings* the value of phi is 0.74 for Section A and 0.70 for Section B. The SEM for a candidate section total score, averaged over the 12 markers, is 4.4 marks in the first case and 8.4 marks in the second. It follows that 95% confidence intervals around candidate section scores are ± 8.4 marks for Section A (an interval spanning almost 30 per cent of the 0-60 mark scale) and ±16.4 marks for Section B (an interval spanning 37 per cent of the 0-90 mark scale).

Although there was rather little inter-marker or intra-marker variation evident in the analysis results (for analyses based on mean score and not total score metrics) there was nevertheless some. When question marks are totalled to produce section totals, then any degree of marker variability will affect the reliability of candidate assessment should just one marker mark each script, as in the regular operational processing. Table 4.3 offers *what if?* estimates of section reliability and score precision for single marking (the current operational situation) and for blind double marking. These estimates are produced simply by substituting the new numbers of markers into the relevant formulae for phi and the SEM.

	Section A (20 3-mark questions)		*Section B (6 15-mark questions)*	
	phi	*95% CI**	*Phi*	*95% CI**
Single marking	0.71	± 9.4	0.66	± 18.0
Double marking	0.73	± 9.0	0.68	± 17.1

** These are given as marks around candidates' section total scores*

Table 4.3: Estimated reliabilities for single and double marking

In both cases, while it would increase overall test length, score precision could be improved by adding more questions, should this be a feasible option (for example, by splitting the two sections into two separate tests, between them including more questions). In fact, as Section 1 notes, the 2391 qualification was replaced with two new narrower qualifications in 2012, with different assessment arrangements, so that these findings no longer have relevance for this particular unit. Notwithstanding, we move on to consider the reliability of candidate total test scores across the two sections.

4.3.2 Whole-test results: composite score reliability

We have seen in the previous section the reliability evidence for candidate measurement for Section A and Section B separately. But what is the result of adding the two sections together? To answer this question we need to carry out a composite score analysis. In the case of unit 301 the composite test score is the simple sum of the two section scores. Since Section A has a maximum total mark of 60 while Section B has a maximum total mark of 90, this means that Section A carries a lower weight than Section B in the total test score (40 per cent versus 60 per cent).

Since there was no evidence for either section of any marker drift over time or of any other effects involving occasions, the factor 'occasions' can be dropped, so that a new design underpinning analysis could be:

c x q:s x m

where c, q and m represent, as before, candidates, questions and markers, and s represents sections. Candidates, questions and markers are crossed, while questions are nested within sections.

However, a univariate analysis would not be appropriate here, because the questions in the two sections are different in nature and in mark tariff. We need at this point to appeal to a multivariate generalizability analysis (see Brennan, 2001, Section 10, for full details; He, 2012, for a summary and simulated example; Johnson & Johnson, 2012b, for real-data example applications). Each section is separately analysed following the fully crossed random effects design c x q x m (see Figure 4.4), the estimated variance components for the sections then being weighted appropriately to produce reliability information for composite scores. The analysis was carried out using mGenova (Brennan 2001c); the results are shown in Table 4.4.

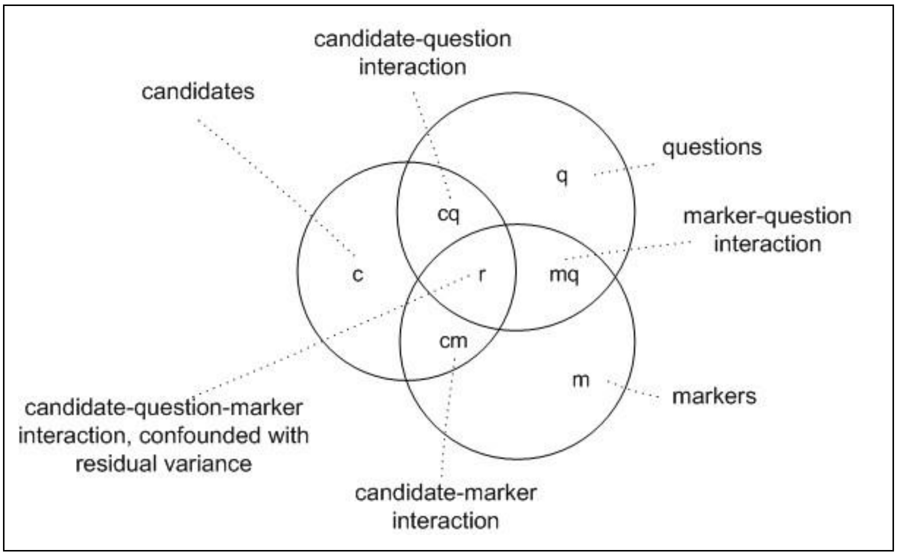

candidate-question interaction
candidates
questions
q
cq
marker-question interaction
c
r
mq
cm
m
markers
candidate-question-marker interaction, confounded with residual variance
candidate-marker interaction

Figure 4.4: The random effects design c x q x m

As Table 4.4 shows, the dependability coefficient for the 2391-301 composite test is estimated to be a modest 0.71, while the cut score reliability estimate (for a cut score of 94) is higher at 0.86.

Of greater importance than the reliability coefficient, however, is the SEM, and the associated 95% confidence interval around a generic candidate total test score. In

Table 4.3 we saw that the 95% confidence intervals around total scores for the two sections of the test, i.e. Section A and Section B, were ± 9.4 marks and ± 18 marks, respectively, spanning around 30% and 40% of the respective mark scales of 0-60 and 0-90. Table 4.4 shows that for the test as a whole the SEM is 10.4 marks, giving a 95% confidence interval around a composite test score of ± 20.3 marks, or around a quarter of the 0-150 mark scale. The SEM, and hence the confidence interval, can be expected to differ at different points in the mark distribution; a larger-scale multiple marking study could have allowed a more targeted investigation, such as an analysis of the situation for candidates near the cut score.

variance component estimates and % contributions to question score variation	*Section A*	*Section B*	*whole test**
candidates (0.1388, 4.5041)	11	25	
questions (0.2914, 1.1809)	23	6	
markers (0.0017, 0.0544)	-	<1	
candidate-question interaction (0.6476, 10.2308)	52	56	
candidate-marker interaction (-0.0005, -0.0157)	-	-	
marker-question interaction (0.0134, 0.1163)	1	<1	
confounded residual (0.1562, 2.1992)	13	12	
Reliability estimates for the operational case of single marking:			
phi	0.71	0.66	0.71
phi(3.615) **			0.86
SEM for a candidate total score	4.8	9.2	10.4
95% confidence interval around total scores	± 9.4	± 18.0	± 20.3

** There were 20 3-mark questions in Section A and six 15-mark questions in Section B, giving section maximum marks of 60 and 90, respectively, and a maximum possible test score of 150; 2,401 candidates.*

*** The cut score for the test was 94 marks out of the total of 150 marks; lambda is conventionally expressed in the mean score metric, and in this case a test score of 94 over 26 questions gives a lambda value for computational use of 3.615*

Table 4.4: G-study results for the whole 2391-301 unit test

Within the constraints of budget, logistics and operational timescales, one or other of the following options might have been considered for feasibility had there been a need to further improve reliability: encouraging centres to reduce the heterogeneity in the candidature (in terms of relevant experience and test preparedness, thus minimising the contribution to measurement error of the candidate-question interaction), increasing the number of questions in the test (perhaps splitting the sections into tests in their own right, as noted earlier, to avoid an increased risk of test fatigue should the current test be replaced by another of even longer length), and rechecking the Section B marking of the small percentage of candidates within two or three marks of the cut score (to minimise the chances of any candidate misclassification - see section 4.4). In the event, qualification 2391 had already been identified for replacement by two alternative qualifications when this project was in the planning stage, so that reliability improvement is in effect an irrelevant issue for this particular qualification.

4.4 A focus on marker performance

The analysis results presented earlier in this section confirm that the estimated contribution of markers to score variation at question level appeared to be negligible. This is a welcome finding. But it is perhaps not a surprising one. The domain against which candidates' electro-technical knowledge was being assessed is by definition highly technical and well-defined. Section A of the test comprised short-answer factual recall questions that had very clearly identified correct answers. Even in Section B the marking scheme for the structured questions was very detailed and in general awarded single digit marks for specific atomistic elements in candidates' responses. Marker quality assurance was also rigorous (see below, and City & Guilds 2011c), with markers showing any weaknesses in their own electro-technical knowledge being identified for team leader support and perhaps dismissal. The consequence both of a tightly specified knowledge domain combined with a very detailed relatively atomistic mark scheme and strong marker quality assurance could be expected to be high levels of agreement in mark awarding. This will be the case for many VQ written tests sharing similar characteristics.

In addition, it must be noted that the markers who took part in the reliability study were fully aware that the batches of scripts they were marking at the beginning and near the end of the operational marking period were 'special'. This could potentially have had some effect on their marking behaviour, but the impact for this particular test was probably not important – given the constraints on marking noted above. Nevertheless, should similar studies be organised in the future it would be better if the identities of the study scripts could be kept from the markers. In other words, markers should be sent the 'additional' papers mixed with their normal papers so that they could not readily be distinguished. Finally, it must be remembered that marker contributions to mark variability at the question level are dwarfed by variance contributions from candidates, questions and their interaction.

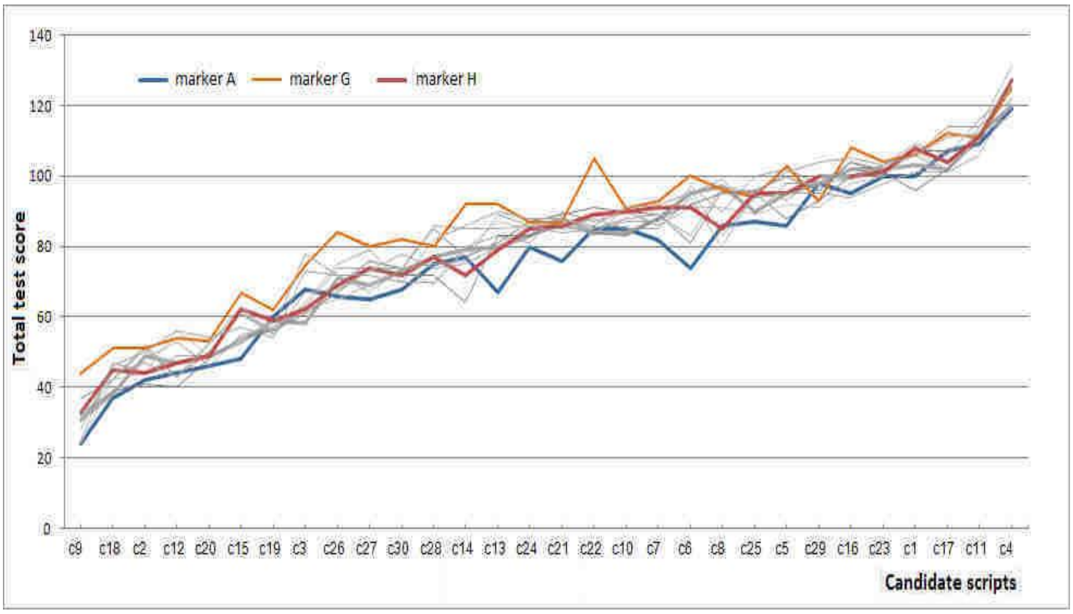
What can we now say about how well markers could have been measured in this same exercise? If we think about a marker's total mark on each section, averaged over candidates, then we know that the precision of that mark will be affected by question-related and candidate-related factors, just as the precision of candidates' section scores was influenced by marker-related and question-related factors. For the given section tests, the larger the candidate sample that markers are asked to mark, i.e. the larger the number of scripts they are asked to rate, the more precisely we can expect to measure their 'true scores', i.e. their absolute standards of marking, as evidenced in their section total marks.

Using the variance component information in Tables 4.1 and 4.2 again, we can shift attention from candidate measurement to focus on marker measurement, and calculate 95% confidence intervals around markers' section total scores. For the situation in the multiple-marker study, the 95% confidence intervals around a marker's section total mark (averaged over 30 scripts) is ± 5.7 marks for Section A (which had a 0-60 mark scale) and ± 7.8 marks for Section B (which had a 0-90 mark scale); in both cases the confidence interval is around 10 per cent of the length of the section mark scale. If these confidence intervals were considered too wide for individual markers to be fairly judged in terms of their 'absolute' overall marking standards, they could be reduced by increasing the number of candidate scripts their section marks are averaged over.

Even for situations like the 2391-301 testing, where differences in marker standards might confidently be assumed to be relatively low, City & Guilds nevertheless engages in ongoing quality assurance, through the use of seeded scripts. These are a small number of scripts that are pre-marked by an expert marker, usually the team leader. The scripts are delivered to the regular markers at points throughout the marking period and their marks then compared with those of the expert (the regular markers are unaware of the marks given by the expert marker). In this way 'aberrant' markers can be identified. These are typically markers whose allocated total mark for one or more scripts falls outside some given threshold when compared with the mark allocated by the expert. But on these occasions rather few scripts are used – five or 10 on each check (City & Guilds, 2011c) – and the criterion for team leader action is quite strict. In the December 2011 marking process two of the 12 markers that participated in the multiple-marking study were identified as aberrant. In both cases all the scripts marked by those markers were re-marked. But could the 'threshold test' give misleading results at times?

Figure 4.5 shows the degree of agreement/disagreement in the whole-script mark allocations of the 12 study markers for the 30 scripts that they evaluated in the multiple-marker study, with scripts ordered from lowest performing candidate (with an average over markers of around 38 marks) to best performing candidate (with an average over markers of 124 marks). The first feature of interest to note is the general degree of agreement in mark allocations, the majority of the markers following the same pattern of marking (for reference, marker H was the team leader).

Note also, though, the greater divergence of outcome for middle-performing scripts compared with high and low performing scripts. This would be expected, since high performing and low performing candidates will generally offer consistent performances across the questions. Other features to note are the sometimes large differences in the marks awarded by markers to several individual candidates within this generally tidy pattern. This is a common phenomenon that is not confined to City & Guilds or to this particular test paper.



Grey lines relate to the nine markers other than markers A, G and H that were involved in the study

Figure 4.5: Marker agreement across the range of candidate performance

High inter-marker correlations (the average here was 0.97) and low contributions of marker variability to total score variation at question level (as shown in Tables 4.1

and 4.2) can lead to false confidence that all is well and nothing needs improvement. High correlations simply mean that markers rank candidates similarly; the longer the mark scale the greater the chance that high inter-marker correlations will emerge. Low contributions of marker-related variability to score variation at question level, for its part, might simply reflect the fact that candidate-related and question-related variability is much higher, as in this case.

When we look at the results of cumulating the marks that the different markers awarded to the 26 question attempts of individual candidates to produce total test scores, we see quite large differences in some cases. Even for the best and worst performing candidates shown in Figure 4.5 we see differences of 10 to 20 marks in the total test scores awarded by the different markers. For the handful of candidates in this small sample who emerged from the exercise with total marks within two or three marks of the cut score (94, recall), there was an almost even chance of passing or failing the test, and therefore of gaining or not gaining the qualification. This is because around half the markers produced total marks at or above 94 and half below. Outside this interval there was unanimous agreement in pass-fail outcome, if not in total marks awarded.

Among the12 markers in Figure 4.5, two are clearly distinguishable from the rest. One of these is marker G, who tended to be more lenient than the other markers, especially in the middle of the script range; this marker was in fact identified as aberrant during the routine City & Guilds quality assurance checks. The other is marker A, who tended to be more severe than the others, though for only a handful of scripts. This marker was not identified as aberrant in the operational quality assurance checks, and on further investigation the reason for the unusually low marks awarded to particular scripts in the marker study became clear: they were mark transcription errors rather than inappropriate mark allocations. In contrast, a marker whose performance in the multiple-marker study was entirely in line with the general pattern *was* identified as aberrant in a seeded script check.

It should be a relatively straightforward matter to identify individual markers as being more or less severe in their marking than all other markers – although, as we have established earlier in this section, markers' marks would need to be averaged over more than a handful of scripts if their absolute marking standards are to be estimated with confidence. If a discrepancy were found to be consistent across scripts then that marker's marks could simply be adjusted up or down by some appropriate amount (the fact that markers typically mark all the scripts from particular centres risks confusing the issue). If discrepancies were inconsistent then an across-the-board adjustment would not be an option, and in that case the only sensible strategy available would indeed be to re-mark all of that individual's assigned scripts, as was organised for this test in the case of marker G.

4.5 Study limitations

For unavoidable reasons the multiple-marker study was on this occasion very small-scale in terms of the number of scripts that it was anticipated could be independently marked by the 12 markers on top of their normal operational workload. The 30 scripts reviewed were, however, randomly selected from the 2391 test entry, and the performance of the 30 candidates do appear to be a fair reflection of that of the entire candidate group (see Figure 4.1). In that sense the results of the study can be generalised to the larger group of candidates. The 12 markers who elected to participate in the study were not unusual either, and, as a sample, their performances, too, can be generalised to all markers of similar type (similar personal and professional characteristics). In other words, despite the small size of the candidate and marker samples the results of the study can reasonably be generalised to the whole marking operation.

What is lost as a result of the small scale of the study is the ability to look closely at the reliability situation for particular candidate subgroups, in particular for those candidates within a few marks of the cut score. In any repeat venture it would be good if the number of scripts evaluated could be at least tripled, and if the scripts could all be selected from around that critical cut score. This latter feature would not necessarily require that the script sample be selected and the marking study carried out after the entire operational assessment had taken place, since cut score possibilities within a fairly narrow range are typically identified quite early in the process on the basis of previous years' results.

5 Discussion and implications

5.1 *Study findings and implications*

Ofqual’s reliability programme was prompted by concerns regarding the lack of a coherent body of knowledge on the issue of assessment reliability. As a result there has been a flurry of research in this area, leading to the publication of a compendium of knowledge on this issue (Opposs & He, 2012). Despite this, work looking at reliability in vocational assessments remains relatively scarce, as noted in Section 1. The current study, together with a partner study that looked at the picture across multiple-choice knowledge tests in many different units (Boyle & Rahman, 2012), constitutes a significant step in building and broadening the body of knowledge relating to the reliability of vocational qualifications.

The aims of this particular project were to investigate the reliability of unit knowledge tests undertaken as part of electro-technical qualifications, and, where reliability improvement was indicated, to identify strategies for achieving that improvement. The findings indicate that for the most part there are grounds for confidence in these qualifications, with consistent levels of marker reliability being maintained over time for the marked unit paper, and satisfactory if not high levels of reliability being obtained for candidate assessment overall in all three qualifications.

Increases to the numbers of items included in the machine-delivered on-demand multiple-choice tests explored in the study would improve reliability. In fact, this has already happened, as during the progress of this research the introduction of new wiring regulations necessitated the review and revision of both tests. While undertaking this activity City & Guilds also took the decision to increase the test lengths in terms of numbers of items. During the review some items were amended or removed and the subject expert undertook a further quality check of all the remaining items. The shorter test format for qualification 2377-100 has now been extended from 45 to 50 items, while that for qualification 2377-200 has been extended from 30 to 45 items. Therefore the recommendations arising from this work have in part already been implemented.

Turning next to the traditional marked paper-based unit test of qualification 2391, the designed multiple-marker study allowed the post-standardisation judgements of 12 City & Guilds assessors to be compared at two time points: immediately after the standardisation meeting and four weeks later towards the end of the operational marking period. Analysis revealed very similar patterns of marking behaviour on the two occasions, with no evidence of 'marker drift'. Moreover, in general inter-assessor agreement was good. Agreement following the standardisation meeting is to be expected: achieving agreement in ratings across all assessors is after all the main purpose of such exercises. What is particularly encouraging about these findings, though, is that there was no fall from that initial post-standardisation level of reliability. This is an extremely important finding, because it speaks to the maintenance of assessor standards and the consistency of likely assessment outcomes over time.

However, despite these positive headline findings there is evidence of some room for improvement. The main conclusions from this part of the work are that while markers are largely marking consistently, the confidence intervals around candidates' total scores are nevertheless larger than they perhaps could be. The research suggests that assessment reliability for candidates could be improved by increasing the number of questions in the test, either by developing a single longer test or by replacing the two-section test with two tests, with respective assessment objectives matching those of the two original sections. The feasibility of either option would need to be evaluated against implications for cost and logistics. It should be noted that, as mentioned in Section 1, City & Guilds had been in the process of reviewing qualification 2391 before this project began, and had decided to replace it with two narrower qualifications, each assessed using the on-demand multiple-choice test format plus a practical assignment.

5.2 Conclusions and recommendations

As we noted at the outset of this work, reliability is central to the esteem in which vocational qualifications are held, for without reliability in assessment there can be little by way of quality assurance or comparability – in other words, the qualifications would fail in their duty to provide a benchmark of performance. Given current concerns over (lack of) parity of esteem for vocational qualifications across Europe (see Brockmann, Clarke & Winch, 2011), and in particular within the UK the continued questioning by some commentators of the value of vocational qualifications, there is particular reason for examining the reliability of vocational assessments. There is however another reason for concern regarding reliability, and this is at the level of the individual. Issues of reliability are fundamental to notions of equitable treatment during the process of assessment.

Some writers (see, for example, Parkes, 2007) have noted that the absence of evidence can be construed as the absence of reliability itself: in other words, the lack of evidence relating to reliability is taken as proof that tests are themselves not reliable. It is true that there has been little research in this area, but this is now being addressed through the commissioning of research such as that reported here. This research is important for two main reasons: to reassure candidates (and their teachers and families) that they are likely to be treated as fairly in assessments for these qualifications as they would be were they to opt for an ‘academic’ qualification; and secondly, to inform future item reviews and the actions of scheme managers, chairs, and lead assessors. Given the real momentum behind apprenticeships in the UK at present, both these points are likely to assume ever-greater prominence.

Given the relatively little exploration to date of the reliability of vocational assessments, this work constitutes an important contribution to the current limited body of knowledge regarding reliability in these qualifications. While there has been some investigation of assessor behaviours in judging portfolios of evidence (i.e. a mix of records of observation, questioning and documentation; see Greatorex and Shannon, 2003) there has been little research focused specifically on the actions of markers in assessing the knowledge tests used within vocational qualifications. In part the dearth of research on the testing of knowledge may be attributable to the history of the role of knowledge within vocational qualifications (and in particular within NVQs, as the primary vocational route in the UK since 1986). It will be recalled that when NVQs were first introduced there were no explicit tests of knowledge, because the desire was for all knowledge to be assessed through performance. The range statement (see also range variables and range indicators) established the contexts within which the performance should be exhibited and assessed and in so doing, also implied the knowledge needed to perform in that range of contexts (Miller, 1992). It was only where assessment would become unnecessarily laborious and drawn-out that knowledge would be assessed directly, and in the early stages this was envisaged as being through questioning by the assessor.

This in turn may be one of the reasons for one of the long-standing myths surrounding vocational assessments: that no knowledge tests are used in vocational qualifications. In fact, within a few years of the introduction of NVQs several issues had emerged regarding the testing of underpinning knowledge: if testing was to be left to the questioning strategies of individual assessors, how could verifiers assess extent and sufficiency of coverage? Recording of questions and answers could add significantly to the assessment burden. Some high risk occupations raised particular concerns. But it was perhaps the case of apprenticeships that lent the greatest weight of argument to the debate. If young people were to gain their major preparation for the workplace through this type of qualification and assessment, would this provide a sufficient and coherent body of knowledge to underpin their occupational competence and to form a foundation for any future learning?

Subsequently, technical certificates were introduced, which in some cases were designed from scratch and elsewhere were re-designed from the certificates and diplomas that had existed prior to, and sometimes had continued in parallel with, NVQs. Awarding organisations such as City & Guilds had retained long-standing item banks which they had continued to use (with updating as appropriate) within their bespoke qualifications provided to companies. These were once again pressed into service when explicit knowledge testing returned to the vocational arena.

As we noted above, it was particularly in high risk areas of work that written (or, more recently, online) tests were viewed as essential to guarantee competent performance. The tests examined in the current study related to the knowledge needed to assure competent performance with electrical equipment and installations. Whilst these certificates assess knowledge within a relatively high-risk sector of the economy, the domains assessed are well-defined and delimited. The tests appear to show reasonable levels of reliability and assessors' marks are also generally dependable.

However, these tests provide only limited room for variance in the answers sought, one being multiple-choice, the other consisting of a short-answer section and a second section requiring slightly longer answers. As answers become longer, factors such as phraseology come more to the fore, and markers have a greater role to play in interpreting responses. It is perhaps in these types of assessment that less reliability might be expected. However, the types of assessment chosen for study in this project were circumscribed by the need to ensure the work could be successfully undertaken within both the tight timescale and the available budget. Examination of less well-constrained tests and other modes of assessment (of observed performance, for example) would be valuable, but would require greater resources than those available for the research described in this report. Nonetheless, as interest grows in vocational options, there is likely to be a need for evidence that these assessment options are at least as reliable as their academic equivalents.

It should be noted that at the time of writing, City & Guilds was planning research into the validity of observational workplace-based assessment. It is also the organisation's intention to follow up this sponsored research with an internal project to look at how the insights gained from reliability studies can be implemented within day-to-day operations. This process had already begun immediately after the end of the study, when the research findings were presented to the relevant chief examiners, team leaders and markers during the routine post-award debriefing meeting. Further wider follow-up will take into account matters such as how to present statistical findings, how to generate indices operationally for hundreds of tests, and so on.

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Office of Qualifications and Examinations Regulation

Spring Place
Coventry Business Park
Herald Avenue
Coventry CV5 6UB

2nd Floor
Glendinning House
6 Murray Street
Belfast BT1 6DN

Telephone 0300 303 3344
Textphone 0300 303 3345
Helpline 0300 303 3346