SYSTEMATIC ERRORS: FACTS AND FICTIONS

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Abstract
The treatment of systematic errors is often mishandled. This is due to lack of understanding and education, based on a fundamental ambiguity as to what is meant by the term. This note addresses the problems and offers guidance to good practice.

1 RIVAL DEFINITIONS: UNCERTAINTY AND MISTAKE

1.1 Random Uncertainties and Mistakes

The word *error* is used in several ways. The everyday dictionary definition is a synonym for *mistake*. In statistics this usage continues (as in 'Type I error' for the rejection of a true hypothesis and 'Type II error' for the acceptance of a false one) but it is also used in the sense of *discrepancy*: the statistician writes the equation of a straight line fit as $y_i = mx_i + c + \epsilon_i$ where ϵ_i is the 'error term', the difference between the measured and the ideal value.

A physicist does not use this language; their interest is concentrated not on the actual discrepancy of a single measurement, but on the overall *uncertainty*. They write a straight line as $y_i = mx_i + c$ where the equals sign signifies agreement to some uncertainty (or resolution) σ, and they will call this the 'error'. (In early texts this was called the 'probable error' but the 'probable' got dropped.) This use of 'error' to mean 'uncertainty' rather than 'mistake' or 'discrepancy' is common in the language, for example 'error bar', 'error analysis', and 'quoted error on the result.'

Suppose a set of measurements have been made of the same quantity, and the values are

$$1.23, 1.25, 1.24, 1.25, 1.21, 1.52, 1.22, 1.27$$

These exhibit some *uncertainty* in the 3rd decimal place, and a *mistake* in that one of the values clearly does not belong with the others.

Statistics provides tools to identify and use the uncertainty. It can be estimated from the rms deviation of the values about the mean, and then used in specifying the accuracy of this mean, or of a single measurement, or the number of measurements that would be needed to achieve some desired accuracy, and so on.

Statistics provides tools to identify a mistake, but not to use it. We can see that the value of 1.52 is wrong - or, more correctly, that the probability of this value being produced by a measurement consistent with the others is so small that we reject it. Statistics does not and cannot tell us what to do next. Perhaps the value is a transcription error for 1.25. Perhaps it was taken while the apparatus was still warming up. Perhaps it is due to an unforeseen and Nobel-prize-winning effect. Perhaps it is right and the others are wrong. What we do next has to be based on experience and common sense, but statistics does not prescribe it.

1.2 Systematic Uncertainties and Mistakes

For consistency physicists must use *systematic error* in the same way as *random error*: to denote a *systematic uncertainty* and not a *systematic mistake*. But consider the following two definitions

'*Systematic effects* is a general category which includes effects such as background, selection bias, scanning efficiency, energy resolution, angle resolution, variation of counter efficiency with beam position and energy, dead time, etc. The uncertainty in the estimation of such a systematic effect is called a *systematic error*.' - Orear[1]

'*Systematic Error:* reproducible inaccuracy introduced by faulty equipment, calibration or technique.' - Bevington[2]

These are taken from widely read and accepted authors, and each on its own would probably get a nod of approval from a practising physicist. However putting them together shows that they are incompatible. The second definition means mistake - the word 'faulty' is a key. The first explicitly defines an uncertainty. It does not contain the sense of fault, blame, or incompetence which is fundamental to the second.

The following examples of 'systematic error' show these two usages.

1 The energy E measured in a calorimeter module is given by

$$E = \alpha D + \beta,$$

where D is some digitisation of the recorded output signal. The error (=uncertainty) on E has a random part due to the random uncertainty on D (from sampling statistics). It has a systematic part due to errors (=uncertainties) on the calibration constants α and β. These are systematic in that from measurement to measurement the value of D will fluctuate about its true value with some standard deviation σ_D, whereas the values of α and β are constant and their discrepancy is applied systematically to all measurements.

2 A branching ratio B is calculated from number of observed decays N out of some total number N_T, where the efficiency of detection is η:

$$B = N/(\eta N_T).$$

There is a statistical Poisson (or perhaps binomial) error on the ratio N/N_T which will fall as more data is gathered. There is an uncertainty on the efficiency η (probably calculated from Monte Carlo simulation) whose contribution will not (unless other steps are taken) fall as more data is taken.

3 Measurements are taken with a steel rule. The rule was calibrated at a temperature of 15 C and the measurements are taken in a warmer laboratory, and the experimenter does not allow for thermal expansion.

4 During the processing of data, numbers are rounded down by omitting all digits after the decimal point.

The first two examples are systematic errors in Orear's sense. There is a systematic effect, encapsulated in α, β, and η, and an uncertainty in that effect, encapsulated in σ_α, σ_β, and σ_η. These errors can be handled by standard techniques, as will be described later.

The third and fourth are examples of the second definition, indeed Example 3 is taken from Bevington; they arise from mistakes. In order to consider how to handle them one has to specify the situation more precisely (as will be done in what follows.)

For consistency we should use Orear's definition rather than Bevington's. In an ideal world the term 'systematic error' might be avoided, and replaced by 'systematic uncertainty' but that is unrealistic. It is vital to distinguish *systematic effects* from the *systematic errors* which are the uncertainties in those effects and from the *systematic mistakes* resulting from the neglect of such effects. Confusion between these three concepts is widespread, and responsible for poor practice.

Of course systematic mistakes still exist, and still need to be identified. But calling them *mistakes* makes clear that although statistics can help to find them, it does not provide tools to tell us what to do with them.

1.3 Systematic Errors and Bias

The terms 'bias' and 'systematic error' are treated as synonymous by some authors [3,4,5]. This is not a full enough definition to be helpful. In discussing a bias one has to consider its status.

Once a bias is known, it can be corrected for: an estimator with known bias can be trivially replaced by an unbiassed estimator. If the bias is unknown and unsuspected then one can by definition do nothing about it. The match between 'bias' and 'systematic error' under our definition is the case where a bias is known to exist, but its exact size (systematic effect) is unknown (systematic uncertainty).

We apply this to the example of measurements with an expanding steel rule.

- If the expansion coefficient is known, as are the two temperatures of calibration and actual measurement, then the measurements can be corrected and the bias is removed; the systematic effect is known exactly and there is no systematic error.
- If the effect is ignored then this is a mistake. Hopefully consistency checks will be done and will (through statistical techniques) reveal a discrepancy for which the physicist will (through common sense, experience and intuition) realise the cause.
- If the effect is known to exist but the temperature at which the actual measurements was taken was not recorded, and one can only give the laboratory temperature to within a few degrees, that is a systematic uncertainty on a systematic effect, and a systematic error in the accepted sense.

2 SYSTEMATIC ERRORS CAN BE BAYESIAN

A *random* uncertainty fits neatly into the frequentist definition of probability. In considering a large ensemble of measurements, different results are obtained. One can speak of the probability of a particular result as the limit of a fraction of measurements giving that result. But if a measurement with a *systematic* uncertainty is repeated then, by definition, the same result is obtained every time, giving an ensemble of identical results which cannot be used to say anything about probability.

In some cases there is a clear way out. The calibration of a calorimeter, for example, may be determined through a separate experiment; the ensemble to be considered is then the ensemble of calibration experiments, rather than the ensemble of actual measurements. A resistor with value $100 \pm 10\ \Omega$ used in voltage and current measurements will always have the same value (of, perhaps, $106\ \Omega$) but it came from a drawer full of nominal $100\ \Omega$ resistors with a spread of values.

In some cases there is no escape. This occurs particularly for so-called 'theory errors'. For example, consider the determination of luminosity in e^+e^- collisions through measuring small angle Bhabha scatters. Perhaps the cross section has been calculated to third order in the fine structure constant α. It is inaccurate in that it deviates from the exact expression. Yet a different calculation will always gives same result. One can guess at this inaccuracy: setting it to a few times α^4 would be sensible. But there is no (obvious) ensemble to use. To quote a figure for an uncertainty in such a situation requires one to use a subjective (Bayesian) definition of probability.

Even for a practitioner who generally uses and advocates a frequentist definition of probability, there are times when the Bayesian definition has to be invoked. This can be excused when a particular systematic error is (as it usually is) a small part of the total error. In doing so it is important that one is aware of what one is doing, and the possible pitfalls.

2.1 Prior pitfalls: an illustration

These dangers appear in a recent example [6]. Consider an experiment where limits are obtained on some quantity R (perhaps a branching ratio or cross section) from some observed number of events n. This was considered by Cousins and Highland [7] who wrote

$$n = SR$$

where S is the 'sensitivity' factor, containing factors such as the detection efficiency, and therefore has some associated uncertainty σ_S which is probably Bayesian. The limits on R are compounded from the statistical (Poisson) variation on n and the variation in S. Consider a particular value of R as an upper limit: the confidence level can be computed by repeatedly taking that value, multiplying it by a value drawn from $Gauss(S, \sigma_S)$, and using that as the mean for generation of a Poisson random number. The fraction of times that this value is less than or equal to the observed n gives the confidence level for this value of R. R can then be adjusted and the process repeated till the desired confidence level is attained. This can be done using approximate analytical formulae [7] or by a toy Monte Carlo [6,7]

However it would be equally valid to write [8]

$$R = An$$

where the appropriate factor A is merely the inverse of S. A trivial change. And yet if one applies the same proportional uncertainty to S and to A one gets different results. For example, suppose 3 events are observed, and you have an uncertainty of 10% on S or A, which are both taken as 1, and consider $R = 5$. The probability of 3 events or less is 27.2% from the first method but 26.6% from the second. The results are different because the priors are different; a Gaussian in S is not the same as a Gaussian in $A \equiv 1/S$.

A third possibility would be to use a Jeffreys' prior. The prescription for this is to effectively transform to a variable for which the Fisher information is flat, and take a flat prior in that. To call this 'objective' is an overstatement, but it does offer a unique prescription. Here it means working in $\ln A$ or equivalently $\ln S$, and generating a Gaussian in that. This gives a value intermediate between the two others.

The moral is that, as is well known to statisticians, with Bayesian statistics one must (unless one has some *a priori* reason for a particular form) investigate the stability of a result under changes in the prior. This example shows that variation does occur at the sort of level to which results are generally quoted.

3 EVALUATING EFFECTS OF SYSTEMATIC UNCERTAINTY

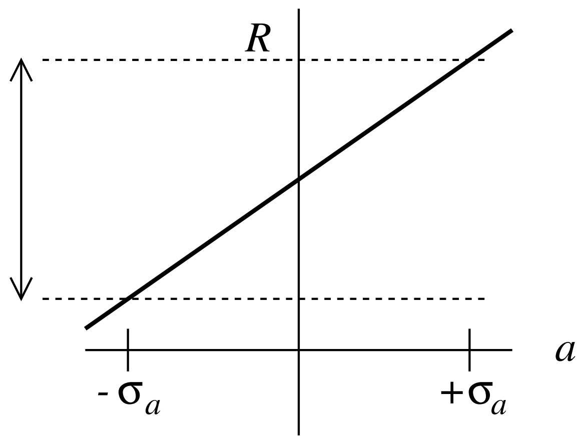

Figure 1: Evaluating the effect of an uncertainty

There is a widespread myth that when errors are systematic, the standard techniques for errors do not apply, and practitioners follow prescriptions handed down from supervisor to student. This is not so. The standard undergraduate combination of errors formula still applies, though one has to be careful to include the correlation terms.

In some cases this is all that is required. If the energy measurement has a systematic error then the error on, say, an invariant mass made from these quantities can be found in the standard way. In other cases they cannot. Suppose the experimental result R depends on some parameter a which is not known exactly, but with some uncertainty σ_a. This parameter a could be one that affects the Monte Carlo generation of simulated events used to extract other quantities in the analysis, which means that the effects of this uncertainty cannot be followed through combination of errors algebra. Instead one generates samples at the best value a_0, and usually at two other values, $a_0 + \sigma_a$ and $a_0 - \sigma_a$ to obtain $R' = \frac{dR}{da}$, as shown in Figure 1. The quoted result is $R(a_0)$, and the error due to the uncertainty in a is $\sigma_a R'$ which is the difference in R. (In some cases more points may be appropriate to investigate possible non-linearity, or different a values to avoid numerical errors. The choice to evaluate at $\pm\sigma$ is for convenience.) This can be done for the final result or for some intermediate result which can then be used in a combination of errors formula. Indeed with today's processing power this method is generally used rather than using algebra, as it gets straight to the answer without assumptions about effects being small.

In some cases this procedure can be simplified: for example if the invariant mass is used to select pairs of photons in a window near the π^0 mass, and the number of these pairs used to give a further value, then given an uncertain energy scale, one can vary the window rather than the energy scale by the appropriate amount, and redo only the final part of the analysis. Note (for future reference) that in such a case the upper and lower edges of the window are varied together, coherently, and that they are changed by a prescribed amount.

3.1 Evaluation: the error on error paradox

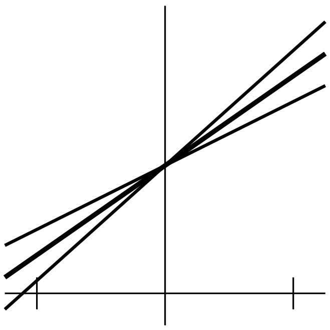

Figure 2 Errors on errors

In a typical experiment there may be a large amount of Monte Carlo data generated at the central value a_0, but less at $a_0 \pm \sigma_a$. So the estimate of R' may itself have an error, $\sigma_{R'}$, due to finite Monte Carlo statistics. How does this affect the systematic uncertainty on R? There are 3 suggestions.

1 $\sigma^2 = (R'\sigma_a)^2 + (\sigma_{R'}\sigma_a)^2$
The uncertainty in R' is another uncertainty so it should be added in quadrature.

2 $\sigma^2 = (R'\sigma_a)^2 - (\sigma_{R'}\sigma_a)^2$
This value R' has been modified from true R' in such a way that $\langle R'^2 \rangle$ is increased. (If R is independent of a, these errors will force R' away from zero.) Subtraction in quadrature compensates for this.

3 $\sigma^2 = (R'\sigma_a)^2$
There is no point messing about with such subtleties. This correction is going to be small unless both $\sigma_a R'$ and σ'_R/R' are large. In that case then to do a decent job on the measurement you have to go back and generate more Monte Carlo data.

If the estimate of R' varies symmetrically about its true value, and there is no reason to doubt that it does, then the estimate of σ as $R'\sigma_a$ is unbiassed. The problem is that this estimate is incorporated in the total error by addition in quadrature; what matters is not the standard deviation but the variance. And if our estimate of σ is unbiassed then our estimate of σ^2 is certainly not unbiassed.

To avoid some of the unnecessary complication we consider an illustrative example. Suppose an integer x is generated with uniform probability over a large range. It is then either increased or decreased by 1, with equal probability, to give $y = x \pm 1$. You are given the value y, and you need the best estimate of x^2. (This represents the need to know the variance rather than the standard deviation.)

There is a (Bayesian) viewpoint which argues: suppose y has a particular value, say $y = 5$. This could have come from $x = 6$ or $x = 4$, and the probabilities are (by symmetry, ignorance, etc) equal. Your value of y^2 is 25, but the true value is 16 or 36. The midway point is 26, and that value will be unbiassed. So add 1 to the value of y^2. This is the first of the 3 methods above.

There is a (frequentist) viewpoint which argues in the reverse direction. Suppose x has a particular value, say $x = 5$. This could give $y = 4$ or $y = 6$ with equal probability. The true value of 25 becomes 16 or 36. On average this is 26, so subtract 1 to remove the bias. This is the second of the 3 methods above.

Algebraically, these two arguments take the two equations

$$y = x \pm 1 \qquad x = y \mp 1$$

square them to get

$$y^2 = x^2 \pm 2x + 1 \qquad x^2 = y^2 \mp 2y + 1$$

and then argue that one can take the average, which cancels the $\pm$ or $\mp$ term.

We can test these arguments against the special case of zero. Suppose you are given a y of 0. Argument 1 gives $0^2 + 1 = 1$. Which is spot on, as we know $x = -1$ or $+1$ so $x^2 = 1$ either way. Argument 2 gives $0^2 - 1 = -1$, which looks crazy. How can our 'best' estimate of x^2 be a negative number?

Continuing the testing, suppose you generate $x = 0$. This will give $y^2 = 1$ so argument 2 is spot on and argument 1 is out by 2. Argument 1 will never give 0. So argument 2 wins this test, but not so dramatically, as you can never know whether x was zero, but if y is zero this is obvious.

In resolving a paradox one has to scrutinise the way it is posed, and here the assertion of a 'uniform probability over a large range' is open to question; the nature of this prior in x affects the Bayesian argument 1 but not the frequentist argument 2. There is no scale in the original problem concerning R', so a uniform probability up to a known finite limit is inadmissable (and would introduce corrections at the limits). You have some belief about the limits $\pm L$, and you believe x is uniformly generated within these limits. This combines to give a prior which falls off at large $|x|$. Your subjective probability of a result between 2 and 5 is larger than that for 10002 and 10005. Given this fall, higher $|x|$ values are intrinsically less probable than low ones, so $y = 5$ must be slightly more likely to have come from $x = 4$ than $x = 6$. Any given y^2 value is more likely to be an upward fluctuation than a downward one. This argument appears inescapable, in that it cannot be deemed to be small and thus ignored. (If the fall in probability is very slow, then large values are very probable and the size of the correction increases.)

Thus the logic of argument 1 fails, and we are left with argument 2. This *is* the frequentist solution, and this *is* a valid frequentist problem: even if σ_a has a Bayesian nature the problem can be stated in terms of an ensemble in which the Monte Carlo is rerun many times. So it is technically correct. It gives the unbiassed estimate, in the sense that averaged over many such estimates the bias will be zero.

3.12 *Conclusions for errors on errors*

There is thus no justification for adding in quadrature, and there is a possible argument for subtraction. But to do this requires that measurements with $\sigma_{R'} > |R'|$ must contribute negatively to the systematic estimate, on the grounds this compensates for overestimation in other cases. (And the greater the inaccuracy, the greater the reduction in the error.) To be right in general you may have to do something manifestly wrong in an individual case (a feature well known in frequentist confidence levels near boundaries.)

If you have a large number of such corrections for parameters $a, b, c...z$ then this approach may be arguable. But not if it's unique. You will never get it past the referee: you investigate an uncertainty and as a result you *reduce* the systematic error, on the grounds that you might have increased it (or, perhaps, that in many parallel universes you increased it?)

No, at this point statistical sophistication has clearly gone too far for plain common sense. We therefore recommend Argument 3: that this error on error correction should not be done as there is no sensible way of doing it. It can be left out of the reckoning if small, and if large then more work is needed to make it small.

4 CHECKS: FINDING MISTAKES

Finding mistakes is done by thinking through the analysis in a critical way, something often best done by consulting colleagues or presenting results at seminars. Such a critique looks at what could go wrong, and at what checks can be done on the analysis which could reveal mistakes. These checks are variations of the analysis, for which the correct outcome is known, either absolutely or in relation to other results.

You can never prove that an analysis is perfect, but the more checks you perform successfully, the greater the credibility of the result.

Such checks commonly include:

1. Analysing separate data subsets
2. Changing cuts
3. Changing histogram bin sizes
4. Changing parametrisations (including the order of polynomial)
5. Changing fit technique
6. Looking for impossibilities

This approach is shown, for example, in the BABAR CP violation measurement[9]

'... consistency checks, including separation of the data by decay mode, tagging category and B_{tag} flavour... We also fit the samples of non-CP decay modes for $\sin 2\beta$ with no statistically significant asymmetry found.'

4.1 What is a significant difference?

If an analysis is performed in two ways (say, using two different forms to fit a background function) then one hopes that the difference between the two resulting values will be small; a large difference would suggest that the background subtraction was not being done properly. However it would be unrealistic to expect them to be identical. The question arises as to what 'small' means in this context.

It does not mean 'small with respect to the statistical error'. The statistical error is probably dominated by the sampling process. But these two analyses are done on the same data (or their datasets share a lot of elements), and so should agree much better than that.

Suppose the standard analysis gives $a_1 \pm \sigma_1$. A different method done as a check gives $a_2 \pm \sigma_2$ We consider the difference $\Delta = a_1 - a_2$. The error on this is

$$\sigma_\Delta^2 = \sigma_1^2 + \sigma_2^2 - 2\rho\sigma_1\sigma_2$$

Suppose firstly that the estimate is a mean of some quantity x, and that the check consists of selecting a subset of the data

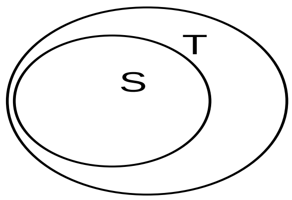

Figure 3. Many checks can be performed by analysing a selected subset of the total data.

The two values are given by

$$a_1 = \frac{1}{N_T} \sum_T x_i \qquad a_2 = \frac{1}{N_S} \sum_S x_i$$

and the errors by

$$\sigma_1 = \frac{\sigma}{\sqrt{N_T}} \qquad \sigma_2 = \frac{\sigma}{\sqrt{N_S}}$$

and the covariance between them is

$$Cov(a_1, a_2) = N_S \frac{1}{N_T} \frac{1}{N_S} \sigma^2$$

so the correlation is just

$$\rho = \sigma_1 / \sigma_2.$$

This gives the required error on Δ

$$\sigma_\Delta^2 = \sigma_2^2 - \sigma_1^2$$

showing that the error is found by subtraction in quadrature of the two separate errors.

If the check is more general, perhaps using a different method on the same data, it is still true that

$$\sigma_\Delta^2 = \sigma_1^2 + \sigma_2^2 - 2\rho\sigma_1\sigma_2.$$

The correlation ρ is not known, but limits can be placed on it [10]. Introduce (briefly) a weighted average

$$a(w) = wa_1 + (1-w)a_2.$$

This has variance

$$\sigma_{a(w)}^2 = w^2\sigma_1^2 + (1-w)^2\sigma_2^2 + 2w(1-w)\rho\sigma_1\sigma_2$$

By choosing w (differentiate the above, set it to zero, solve for w and put back in) one gets the smallest variance possible from a weighted sum.

$$\sigma_{min}^2 = \frac{\sigma_1^2\sigma_2^2(1-\rho^2)}{\sigma_1^2 + \sigma_2^2 - 2\rho\sigma_1\sigma_2}.$$

Now, in an estimation problem there is a limit on the variance of any estimator: the Minimum Variance (or Cramer-Rao) Bound. This limit applies irrespective of the estimation technique used. It depends only on the likelihood function, and its value σ_0 can be calculated from it. This bound means that

$$\sigma_{min}^2 \geq \sigma_0^2.$$

Inserting the expression for σ^2_{min} gives an expression which can be rearranged to give limits on ρ, and that translates to limits on σ_Δ.

$$\sqrt{(\sigma_1^2 - \sigma_0^2)} + \sqrt{(\sigma_2^2 - \sigma_0^2)} \geq \sigma_\Delta \geq \left| \sqrt{(\sigma_1^2 - \sigma_0^2)} - \sqrt{(\sigma_2^2 - \sigma_0^2)} \right| .$$

Notice that if $\sigma_1 = \sigma_0$ this again gives subtraction in quadrature. In many cases the standard analysis will be the most efficient possible, so this will be the case.

4.2 Checks that pass, checks that fail

The standard procedure for doing an analysis can be caricatured as follows

1. Devise cuts, get result.
2. Do analysis for random errors (likelihood or Poisson statistics.)
3. Make big table.
4. Alter cuts by arbitrary amounts, put in table.
5. Repeat step 4 until time/money/supervisor's patience is exhausted.
6. Add variations in quadrature.
7. Quote result as 'systematic error'.
8. If challenged, describe it as 'conservative'.

This combines evaluation of errors with checks for mistakes, in a totally inappropriate way.

Suppose a check is done, and a discrepancy emerges as some number of σ_Δ. You then have to decide whether it has passed or failed the test. Your decision will depend on the size of the discrepancy (less than 1 σ surely passes, more than 4 σ surely fails), the number of checks being done (if you do 20 checks you expect on average one 2σ deviation) and at some level on the basic plausibility and reasons that motivated the check (you might accept that data taken in the summer were different more readily than you would accept that data taken on Tuesdays were different.)

If a check passes then the correct thing to do is *nothing*. Put a tick in the box and move on. Do not, as is practice in some areas, add the small discrepancy to the systematic error.

1 It's an inconsistent action. You asked 'is there an effect' and decided there wasn't. If there was no effect then you should not allow for it. Remember that this is a check and not an evaluation of an effect.

2 It penalises diligence. The harder you work and more thorough you are, the bigger your systematic error gets. A less careful analysis will have a smaller quoted error and get the citations.

3 Errors get inflated. Remember how the LEP experiments appear to agree with each other and the Standard Model far *too* well.

One has to be careful. Contrast moving mass cuts by a defined amount to compensate for energy uncertainty (this is an evaluation and included) and changing mass cuts by an arbitrary amount to check efficiency/purity (this is a check and not included if successful.)

If it fails then the correct actions to take are

1 Check the test. The mistake may well lie there. Find and fix it.

2 If that doesn't work, check the analysis. Find and fix mistake.

3 Worry. Maybe with hindsight an effect is reasonable. (Why are the results of my ruler measurements different after lunch? Hey, the temperature's warmer in the afternoons - I forgot about thermal expansion!) This check now becomes an evaluation.

4 Worry. This discrepancy is only the tip of the iceberg. Ask colleagues, look at what other experiments did.

99 As a last resort, incorporate the discrepancy in systematic error.

Just doing a whole lot of checks and adding up the results in quadrature to the systematic error is making a whole lot of mistakes, some too lenient, some too harsh.

4.3 Illustration: an inappropriate function

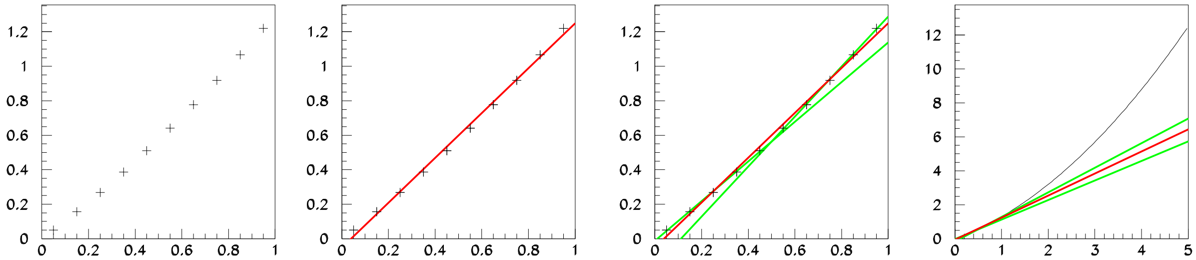

Figure 4 An inappropriate function. The plots show (i) The data. (ii) A straight line fit to the data. (iii) Additional fits to subranges. (iv) Extrapolation of fits and the true function - note changes in scale.

Suppose you are using a calorimeter for which the energy y corresponding to a signal x is actually given by $y = x + 0.3x^2$. Measurements are taken as shown in the first plot (measurement errors are suppressed for clarity). You fit it as a straight line $y = mx + c$ using data in the range $0 \le x \le 1$, and get $m = 1.3$ and $c = -0.05$, as shown in the second plot. This is what you use in your analysis.

As a sensible check you decide to calibrate the subranges $0 \le x \le 0.5$ and $0.5 \le x \le 1$ separately. The results are different (as shown in the third plot). The slopes are 1.15 and 1.45, and there is no possibility that this is a statistical error.

You follow the procedure above but for some reason fail to spot that a linear calibration is inadequate. You end up incorporating the difference of 0.15 as a systematic error on m (with perhaps a similar systematic error for c, and even a correlation between them.)

Notice what a terrible decision this is. As you can see from the figures, in the range $0 \le x \le 1$ this is far too harsh. The line with a slope of 1.3 actually follows the points pretty well and this extra error is inflationary.

On the other hand, if this calibration is to be extrapolated towards $x = 2$ or even $x = 5$, then even this extra variation far underestimates the calibration discrepancy in this region. The procedure is far too lenient.

This illustrates the point that there is no ‘correct’ procedure for incorporation of a check that fails. If you fold it into the systematic errors this is almost certainly wrong, and should only be done when all other possibilites have been exhausted.

5 CONCLUSIONS: ADVICE FOR PRACTITIONERS

The following should be printed in large letters and hung on the wall of every practising particle physicist.

- Thou shalt never say ‘systematic error’ when thou meanest ‘systematic effect’ or ‘systematic mistake’.
- Thou shalt not add uncertainties on uncertainties in quadrature. If they are larger than chickenfeed thou shalt generate more Monte Carlo until they shrink to become so.
- Thou shalt know at all times whether what thou performest is a check for a mistake or an evaluation of an uncertainty.
- Thou shalt not incorporate successful check results into thy total systematic error and make thereby a shield behind which to hide thy dodgy result.
- Thou shalt not incorporate failed check results unless thou art truly at thy wits’ end.

- Thou shalt say what thou doest, and thou shalt be able to justify it out of thine own mouth; not the mouth of thy supervisor, nor thy colleague who did the analysis last time, nor thy local statistics guru, nor thy mate down the pub.

Do these, and thou shalt flourish, and thine analysis likewise.

6 ACKNOWLEDGEMENTS

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