Assessing agreement on classification tasks: the kappa statistic

Jean Carletta*
University of Edinburgh

Currently, computational linguists and cognitive scientists working in the area of discourse and dialogue argue that their subjective judgments are reliable using several different statistics, none of which are easily interpretable or comparable to each other. Meanwhile, researchers in content analysis have already experienced the same difficulties and come up with a solution in the kappa statistic. We discuss what is wrong with reliability measures as they are currently used for discourse and dialogue work in computational linguistics and cognitive science, and argue that we would be better off as a field adopting techniques from content analysis.

1. Introduction

Computational linguistic and cognitive science work on discourse and dialogue relies on subjective judgments. For instance, much current research on discourse phenomena distinguishes between behaviours which tend to occur at or around discourse segment boundaries and those which do not (PL93; KID92; LH90; Cah92). Although in some cases discourse segments are defined automatically (e.g., Rodrigues and Lopes' (RL92) definition based on temporal relationships), more usually discourse segments are defined subjectively based on the intentional structure of the discourse, and then other phenomena are related to them. At one time, it was considered sufficient when working with such judgments to show examples based on the authors' interpretation (paradigmatically, (GS86), but also countless others). Research was judged according to whether or not the reader found the explanation plausible. Now, researchers are beginning to require evidence that people besides the authors themselves can understand and make the judgments underlying the research reliably. This is a reasonable requirement because if researchers can't even show that different people can agree about the judgments on which their research is based, then there is no chance of replicating the research results. Unfortunately, as a field we have not yet come to agreement about how to show reliability of judgments. For instance, consider the following arguments for reliability. We have chosen these examples both for the clarity of their arguments and because taken as a set they introduce the full range of issues we wish to discuss.

(1) Kowtko et al. (KID92), in arguing that it is possible to mark conversational move boundaries, cite separately for each of three naive coders the ratio of the number of times they agreed with an "expert" coder about the existence of a boundary over the number of times either the naive coder or the expert marked a boundary. She does not describe any restrictions on possible boundary sites.

* Human Communication Research Centre, 2 Buccleuch Place, Edinburgh EH8 9LW, Scotland

(2) Once conversational move boundaries have been marked on a transcript, Kowtko et al. argue that naive coders can reliably place moves into one of thirteen exclusive categories by citing pairwise agreement percentages figured over all thirteen categories, again looking at each of the three naive coders separately. Litman and Hirschberg (LH90) use this same pairwise technique for assessing the reliability of cue phrase categorisation, using two equal-status coders and three categories.

(3) Silverman et al. (SBP+92), in arguing that sets of coders can agree on a range of category distinctions involved in the TOBI system for labelling English prosody, cite the ratio of observed agreements over possible agreements, measuring over all possible pairings of the coders. For example, they use this measure for determining the reliability of the existence and category of pitch accents, phrase accents, and boundary tones. They measure agreement over both a pool of highly experienced coders and a larger pool of mixed-experience coders, and argue informally that since the level of agreement is not much different between the two, their coding system is easy to learn.

(4) Passonneau and Litman (PL93), in arguing that naive subjects can reliably agree on whether or not given prosodic phrase boundaries are also discourse segment boundaries, measure reliability using 'percent agreement', defined as the ratio of observed agreements with the majority opinion among seven naive coders to possible agreements with the majority opinion.

Although (1) and Kowtko's use of (2) differ slightly from Litman and Hirschberg's use of (2), (3) and (4) in clearly designating one coder as an "expert", all these studies have n coders place some kind of units into m exclusive categories. Note that the cases of testing for the existence of a boundary can be treated as coding "yes" and "no" categories for each of the possible boundary sites; this treatment is used by measures (3) and (4) but not by measure (1). All four approaches seem reasonable when taken at face value. However, the four measures of reliability bear no relationship to each other. Worse yet, since none of them take into account the level of agreement one would expect coders to reach by chance, none of them are interpretable even on their own. We first explain what effect chance expected agreement has on each of these measures, and then argue that we should adopt the kappa statistic (SNJC88) as a uniform measure of reliability.

2. Chance expected agreement

Measure (2) seems a natural choice when there are two coders, and there are several possible extensions when there are more coders, including citing separate agreement figures for each important pairing (as Kowtko does by designating an expert), counting a unit as agreed only if all coders agree on it, or measuring one agreement over all possible pairs of coders thrown in together. Taking just the two coder case, the amount of agreement we would expect coders to reach by chance depends on the number and relative proportions of the categories used by the coders. For instance, consider what happens when the coders randomly place units into categories instead of using an established coding scheme. If there are two categories which occur in equal proportions, on average they would agree with each other half of the time; each time the second coder makes a choice, there is a fifty/fifty chance of coming up with the same category as the first coder. If instead, the two coders were to use four categories in equal proportions, we would expect them to agree 25% of the time (since no matter what the first coder chooses, there is a 25% chance that the second coder will agree.) And if both coders were to use one of two categories, but use one of the categories 95% of the time, we would expect them to agree 90.5% of the time ($.95^2 + .05^2$, or, in words, 95% of the time the first coder chooses the first category, with a .95 chance of the second coder also choosing that category, and 5% of the time the first coder chooses the second category, with a .05 chance of the second coder also doing so). This makes it impossible to interpret raw agreement figures using measure (2). This same problem affects all of the possible ways of extending measure (2) to more than two coders.

Now consider measure (3), which has an advantage over measure (2) when there is a pool of coders, none of whom should be distinguished, in that it produces one figure which sums reliability over all coder pairs. Measure (3) still falls foul of the same problem with expected chance agreement as measure (2) because it does not take into account the number of categories which occur in the coding scheme.

Measure (4) is a different approach to measuring over multiple undifferentiated coders. Note that although Passonneau and Litman are looking at the presence or absence of discourse segment boundaries, measure (4) takes into account agreement that a prosodic phrase boundary is not a discourse segment boundary, and therefore treats the problem as a two-category distinction. Measure (4) falls foul of the same basic problem with chance agreement as measures (2) and (3), but in addition, the statistic itself guarantees at least 50% agreement by only pairing off coders against the majority opinion. It also introduces an "expert" coder by the back door in assuming that the majority is always right, although this stance is somewhat at odds with Passonneau and Litman's subsequent assessment of a boundary's strength from one to seven based on the number of coders who noticed it.

Measure (1) looks at almost exactly the same type of problem as measure (4), the presence or absence of some kind of boundary. However, since one coder is explicitly designated as an "expert", it doesn't treat the problem as a two category distinction, but looks only at cases where either coder marked a boundary as present. Without knowing the density of conversational move boundaries in the corpus, this makes it difficult to assess how well the coders agreed on the absence of boundaries and to compare measures (1) and (4). In addition, note that since false positives and missed negatives are rolled together in the denominator of the figure, measure (1) does not really distinguish expert and naive coder roles as much as it might. However this style of measure does have some advantages over measures (2), (3), and (4), since these measures produce artificially high agreement figures when one category of a set predominates, as is the case with boundary judgments. One would expect measure (1)'s results to be high under any circumstances, and it is not affected by the density of boundaries.

So far, we have shown that all four of these measures produce figures which are at best, uninterpretable and at worst, misleading. Kowtko et al. make no comment about the meaning of their figures other than to say that the amount of agreement they show is reasonable; Silverman et al. simply point out that where figures are calculated over different numbers of categories, they are not comparable. On the other hand, Passonneau and Litman note that their figures are not properly interpretable and attempt to overcome this failing to some extent by showing that the agreement which they have obtained at least significantly differs from random agreement. Their method for showing this is complex and of no concern to us here, since all it tells us is that it is safe to assume that the coders were not coding randomly — reassuring, but no guarantee of reliability. It is more important to ask *how different* the results are from random and whether or not the data produced by coding is too noisy to use for the purpose for which it was collected.

3. The kappa statistic

The concerns of these researchers are largely the same as those in the field of content analysis (see especially (Kri80) and (Web85)), which has been through the same problems as we are currently facing and in which strong arguments have been made for using the kappa coefficient of agreement (SNJC88) as a measure of reliability.[1]

The kappa coefficient (K) measures pairwise agreement among a set of coders making category judgments, correcting for expected chance agreement.

$$K = \frac{P(A) - P(E)}{1 - P(E)}$$

where $P(A)$ is the proportion of times that the coders agree and $P(E)$ is the proportion of times that we would expect them to agree by chance, calculated along the lines of the intuitive argument presented above. (For complete instructions on how to calculate K, see (SNJC88).) When there is no agreement other than that which would be expected by chance K is zero. When there is total agreement, K is one. When it is useful to do so, it is possible to test whether or not K is significantly different from chance, but more importantly, interpretation of the scale of agreement is possible. Krippendorff (Kri80) discusses what makes an acceptable level of agreement, while giving the caveat that it depends entirely on what one intends to do with the coding. For instance, he claims that it is often impossible to find associations between two variables which both rely on coding schemes with $K < .7$, and says that content analysis researchers generally think of $K > .8$ as good reliability, with $.67 < K < .8$ allowing tentative conclusions to be drawn. We would add two further caveats. First, although kappa addresses many of the problems we have been struggling with as a field, in order to compare K across studies, the underlying assumptions governing the calculation of chance expected agreement still require the units over which coding is performed to be chosen sensibly and comparably. (To see this, compare, for instance, what would happen to the statistic if the same discourse boundary agreement data were calculated variously over a base of clause boundaries, transcribed word boundaries, and transcribed phoneme boundaries.) Where no sensible choice of unit is available pretheoretically, measure (1) may still be preferred. Secondly, coding discourse and dialogue phenomena, and especially coding segment boundaries, may be inherently more difficult than many previous types of content analysis (for instance, dividing newspaper articles based on subject matter). Whether we have reached (or will be able to reach) reasonable level of agreements in our work as a field remains to be seen; our point here is merely that if as a community we adopt clearer statistics, we will be able to compare results in a standard way across different coding schemes and experiments and to evaluate current developments, and that will illuminate both our individual results and the way forward.

1 There are several variants of the kappa coefficient in the literature, including one, Scott's *pi*, which actually has been used at least once in our field, to assess agreement on move boundaries in monologues using action assembly theory (GC86). Krippendorff's α is more general than Siegel and Castellan's K in that Krippendorff extends the argument from category data to interval and ratio scales; this extension might be useful for, for instance, judging the reliability of TOBI break index coding, since some researchers treat these codes as inherently scalar (SBP$^+$92). Krippendorff's α and Siegel and Castellan's K actually differ slightly when used on category judgments in the assumptions under which expected agreement is calculated. Here we use Siegel and Castellan's K because they explain their statistic more clearly, but the value of α is so closely related, especially under the usual expectations for reliability studies, that Krippendorff's statements about α hold, and we conflate the two under the more general name 'kappa'. The advantages and disadvantages of different forms and extensions of kappa have been discussed in many fields but especially in medicine; see, for example, (Ber92; Gol92; Kra80; SP86).

4. Expert versus naive coders

In assessing the amount of agreement among coders of category distinctions, the kappa statistic normalises for the amount of expected chance agreement and allows a single measure to be calculated over multiple coders. This makes it applicable to the studies which we have described, plus more besides. However, we have yet to discuss the role of expert coders in such studies. Kowtko designates one particular coder as the expert. Passonneau and Litman have only naive coders, but in essence have an expert opinion available on each unit classified in terms of the majority opinion. Silverman et al. treat all coders indistinguishably, although they do build an interesting argument about how agreement levels shift when a number of less experienced transcribers are added to a pool of highly experienced ones. We would argue that in subjective codings such as these, there are no real experts. We concur with Krippendorff that what counts is how totally naive coders manage based on written instructions. Comparing naive and expert coding as Kowtko does can be a useful exercise, but instead of assessing the naive coders' accuracy, it really measures how well the instructions convey what she thinks they do. (Krippendorff gives well-established techniques which generalise on this sort of "odd-man-out" result, which involve isolating particular coders, categories, and kinds of units in order to establish where any disagreement is coming from.) In Passonneau and Litman, the reason for comparing to the majority opinion is less clear.

Despite our argument, there *are* occasions when one opinion should be treated as the expert one. For instance, one can imagine determining whether coders using a simplified coding scheme match what can be obtained by some better but more expensive method, which might itself be either objective or subjective. In these cases, we would argue that it is still appropriate to use a variation on the kappa statistic which only looks at pairings of agreement with the expert opinion rather than looking at all possible pairs of coders. This could be done by interpreting $P(A)$ as the proportion of times that the "naive" coders agree with the expert one and $P(E)$ as the proportion of times we would expect the naive coders to agree with the expert one by chance.

5. Conclusions

We have shown that existing measures of reliability in discourse and dialogue work are difficult to interpret, and we have suggested a replacement measure, the kappa statistic, which has a number of advantages over these measures. Kappa is widely accepted in the field of content analysis. It is interpretable, allows different results to be compared, and suggests a set of diagnostics in cases where the reliability results are not good enough for the required purpose. We suggest that this measure be adopted more widely within our own research community.

6. Author Note

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