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Rejoinder

## Response to Biernacki, Reed, and Spillman

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And to the left, three yards beyond,  
You see a little muddy pond  
Of water – never dry  
I measured it from side to side:  
'Twas four feet long, and three feet wide.

– William Wordsworth, *The Thorn*

### Mapping the Field

We are grateful for the serious attention given to our work by Biernacki, Reed and Spillman, especially the sustained rebuttal of some of our specific analytic claims that comes from Biernacki. This sort of critique – indeed, deliberately *destructive* critique – is necessary if the methods we propose are to develop in a useful direction. We propose our technique in ‘Beta’ form, and we need users to try to break it. Interestingly, we think that while Biernacki *has* shown problems, overall, he has confirmed that it is – at least on its own terms, if not his – workable. More generally, we have formidable critiques all around – indeed, amounting to total 117 manuscript pages total – to which to respond.

But how to manage such a response, given our limited amount of space? We might select some aspects of each for a more focused response, but then readers might suspect that we ignored the central thrust of the critique or deliberately misinterpreted our critics. How can we quickly get an overall understanding of the contours of each's argument? Perhaps we can turn each into a concept map, as suggested by Lee and Martin (2015).

Figure 1 displays Biernacki's argument, reduced to an adjacency graph. Overall, we see a distinction between *mapping* and *texts*, with Lee and Martin in-between the two, but Biernacki himself on the other side, near *interpretation*. This seems to correspond to Biernacki's continued insistence that there is an unbridgeable divide between humanistic interpretation (good) and formal techniques (bad). In contrast, Reed's graph (Figure 2) is strongly focused around the single theme of *interpretation*. (Indeed, we had to alter the values for our drawing algorithm to prevent an unreadable star structure that obscured some of the relations among the various planets orbiting the sun of interpretation.) Although this theme of interpretation has been a focus of, for example, Reed's (2011) own work, it also proves to be central to the structure of our original article, which we reduce to a skeleton in Figure 3.

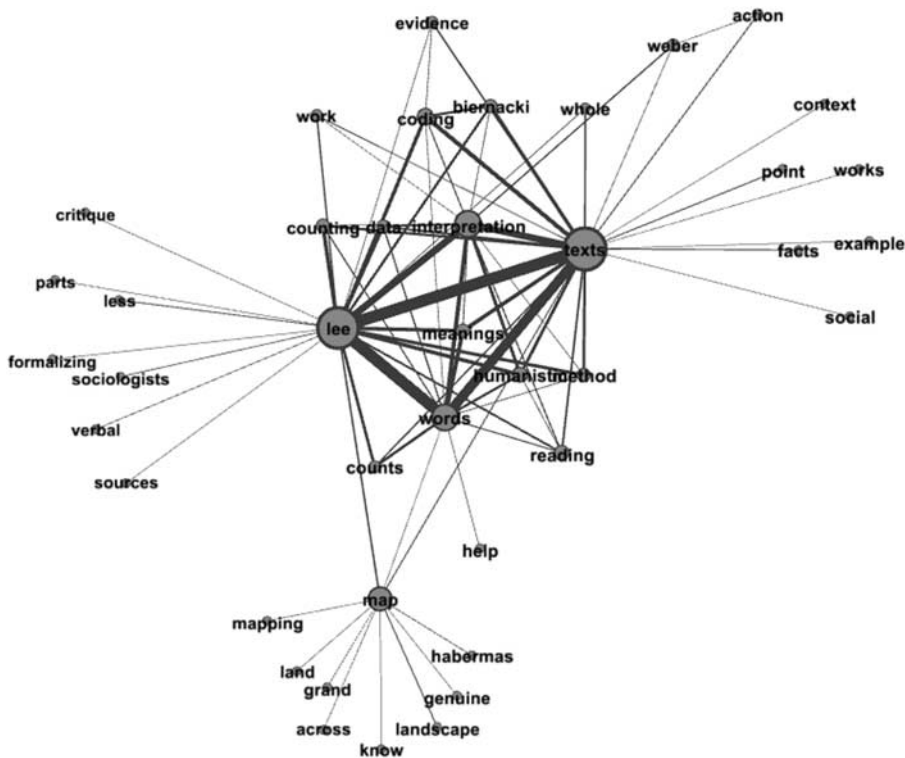


Figure 1: Concept map of Biernacki's critique.

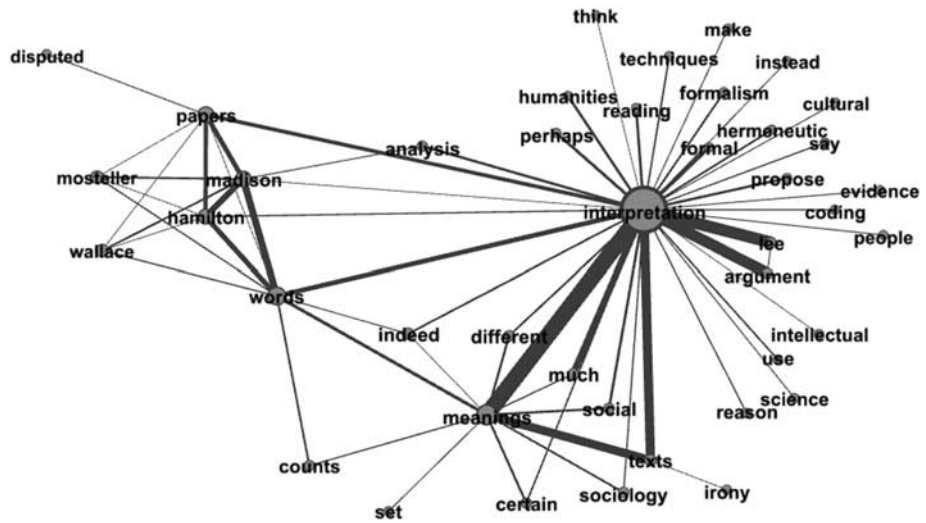


Figure 2: Concept map of Reed's critique.

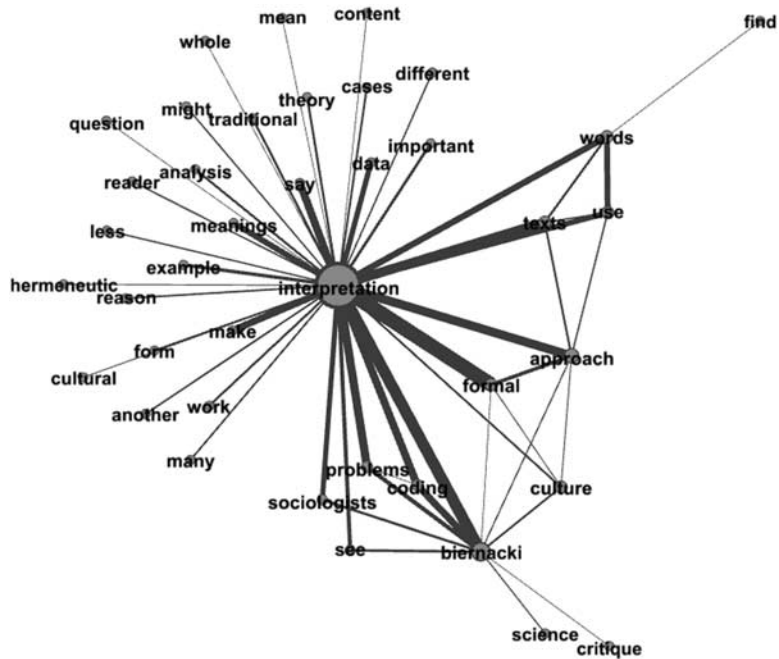


Figure 3: Concept map of Lee and Martin.

Spillman's argument (Figure 4) also has a 'star' structure, though here, the central element is somewhat different. As previously, we combine different variants of words with a single root (thus *meanings* and *meaning* are collapsed to *meaning*, though the latter could be noun or gerund). We also combine *interpret*, *interpretive*, *interpretation* and *interpretations*. In Reed's graph, we



interpretation. This is not what we intended. We therefore begin by laying out our own assumptions in some detail. Here we find it convenient to follow Morris (1938, p. 6f) and distinguish three aspects of semiosis.<sup>1</sup> First, there is that which connects the sign to its expresser, which for us allows us to speak of the *pragmatic* meaning of a text (say) – what the author *means to say* with it. Second, there is that which connects a sign to what it denotes, which gets at the *semantic* meaning – how the word ‘mule’ denotes a certain animal, though a speaker may *use* it differently. Third, there is the relation of signs to one another, which Morris called *syntactics*.

The essence of our approach is to focus on *syntactic* mappings that we believe to preserve *semantic* meaning. These mappings then can support interpretation, especially the *pragmatic* ones that are of most interest to social scientists. This is theoretically defensible, and indeed, compatible with some of the same theoretical works that Biernacki uses. This is because we begin from a resolutely structuralist approach, in the tradition not only of Saussure, but Levi-Strauss, Jakobson and the early Harrison White.<sup>2</sup>

Consider Biernacki’s incredibly effective demonstration that various uses of the word *Vernunft* which, at *best* would all be collapsed to a single point in our graph, have radically different meanings in the works of Horkheimer and Adorno (H&A) (Adorno (1997 [1970]); Adorno (1992 [1974]); Honneth (1991 [1989]) and Marcuse (1992 [1955]).<sup>3</sup> ‘Reason,’ or so H&A tell us, can be something that, at least, in its capitalist variety, has a key chain which jingles in place of the hat bells of the fool. It can be split into the varieties of pure and empirical, which then come into a contradiction resolvable in the conscious solidarity of the group. It can indicate that super-individual transcendental ego ... or, in its technological variety, can triumph over truth, as evidenced by the popularity of stunt films, and more.

How can we treat such a ‘reason’ as an atom? It is a very good question. But ... perhaps Biernacki’s evidence is *too strong*. We are likely to come away from this thinking that the use of *Vernunft* is basically meaningless. There seems to be almost no sentence in which the word reason, properly qualified (technical, pure, empirical, unscented and so on) cannot be inserted. Perhaps H&A are simply spouting nonsense.

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<sup>1</sup> We would not defend Morris’s scheme in all respects, and would not consider it in keeping with Peirce’s own approach, despite Morris’s sincere convictions that it was.

<sup>2</sup> At a number of places, Biernacki cites technical work by computational linguists and others that seems to support a claim as to the impossibility of our approach. Having struggled, somewhat unsuccessfully, with these, we still see nothing that is anything like a strong argument against such possibility. We also note that Biernacki says that ‘the consensus among some computer analysts’ is that frequency is irrelevant. It seems to us that a ‘consensus’ among *some* is exactly when we want to have some counts!

<sup>3</sup> Biernacki points out that there are times when an English idiom employs ‘reason’ where the original German does not have *Vernunft*, leading a somewhat non-random, if not obviously biasing, error in our graphs.

We think that this conclusion is somewhat hasty. There must be some reason (as it were) why the word *Vernunft* is used, and not *Verstand* or *Seele* or *Chocolat*. Does *Vernunft* have ‘a’ meaning? We argue that it does, and here draw upon the same principled structuralism as some of Biernacki’s sources.

To explain this, we must quickly review structuralism, as we suspect that there are readers who are no longer familiar with the details. This is largely because structuralism is in sociology often used in a vague sense, basically meaning anything that is ostentatiously peppered with the word ‘structure.’ But in intellectual history it began as a relatively coherent way of approaching social life.

Most discussions of structuralism would start with Saussure, although the basic insight goes back at least to Plato (*Theaetetus* 202a, b) (1989 [1961]) who argues that we cannot understand elements, but only compounds.<sup>4</sup> But it is de Saussure (1959 [1915]) who developed a coherent and defensible approach to linguistics from this basic idea. Up until that time, most work in linguistics had been carried out according to the principles of what was called historical philology – the study of how languages develop, a very shaky and speculative enterprise.

It was frustration with this historical approach that led Saussure to make his breakthrough: to concentrate on language as a static ideal structure, as opposed to attempting to reconstruct its historical evolution. In particular, language could be understood as composed of a set of signs (words) that pointed to things in the world, and rather than attempting to determine *why* one sign as opposed to another was used in any one case, Saussure proposed that to understand a sign meant to understand its position in a larger structure.

This focus on the structural relations between linguistic components, however, was not as generative for sociological theory as were later developments that arose in phonetics, the study of language as a set of sounds. Phonetics makes a distinction between two relations between elements. First, there is *structure*, which pertains to relations between elements in a single unit (which are called *syntagmatic* relations – that is pertaining to syntax). For example, in *cat* the ‘K’ sound has a structural or syntagmatic relation to the ‘AT’ (Abercrombie, 1971, p. 72).

Second, there is system, which pertains to relations between units across elements (which are called paradigmatic relations). Thus the ‘K’ in *cat* has a systemic relation to ‘B’, ‘F’, ‘H’, ‘M’, ‘N’, ‘P’, ‘R’, ‘S’, ‘T’ and ‘V’ because all are simple consonants that could be substituted for ‘C’ and make an acceptable word. You’ll notice that since the ‘K’ in *kit* can’t be substituted for the ‘K’ in *cat*,

<sup>4</sup> In fact there is no formula in which any element can be expressed; it can only be named, for a name is all there is that belongs to it. But when we come to things composed of these elements, then, just as these things are complex, so the names are combined to make a description [*λόγος*], a description being precisely a combination of names. Accordingly, elements are inexplicable and unknowable, but they can be perceived, while complexes [“syllables”] are knowable and explicable, and you can have a true notion of them’ (203a, b). Plato further has Socrates argue that explaining what something is involves verbalizing its difference from other things (209a).

we can consider them the same; they differ predictably according to the environment of other sounds.<sup>5</sup>

This way of thinking proved very influential for sociology. And that is in large part because it turned out to dovetail – structurally – with other distinctions made in anthropology. One is Sir James Frazer’s distinction between *contagious* magic and *sympathetic* magic, another is the distinction between *marriage* or *affinal* relations on the one hand and descent or agnatic relations on the other. This distinction then entered network analysis as that between *direct ties* on the one hand, and *structural equivalence* on the other. Finally, these were also seen to correspond to the distinction between *metonymy* and *metaphor*. In all cases, the first relation is one of direct contact, and second, one of implicit equivalence in a structure of relations.

Harrison White’s pivotal contribution was the notion that these two types of relations might be envisioning the same graph in two different ways. Let us work this through, but applied to Saussure’s problem, namely the relations of words to one another. Let  $W = \{a, b, c, \dots\}$  be a set of words, and  $A$  and  $E$  two binary relations that are symmetric and reflexive. The first,  $A$  is a relation of ‘structure’ in the Jakobson sense, or what we will emphasize as metonymical connection, a sort of abutment or adjacency. ‘Nature’ has such a relation to the word ‘peaceful’ if the two are juxtaposable (for example, ‘nature is peaceful’). (Of course, in a real language, the relationships between elements transcend mere abutment; most simply, the true relationship is asymmetric, since *you kick* and *kick you* have different meanings. For the purposes of simplicity of exposition, we ignore this for now; all of our points may be generalized to more complex relations.) The second,  $E$ , is a relation of substitutability. If you can take out the word ‘nature’ wherever it is placed, and instead substitute ‘essence,’ then the two words are equivalent. It will be noted that in our article, we only considered  $A$  relations. Here we are proposing that  $E$  relations give us one sense of meaning. If, for some  $a, b \in W$ ,  $aEb$  (read, for two words,  $a$  and  $b$ ,  $a$  is equivalent to  $b$ ), we say that  $a$  means  $b$  (and  $b$  means  $a$ ).

How do we determine  $E$  relations? They are nothing other than identical patterns in the graph of  $A$  (or, more technically, the graph  $G = \{W, A\}$ ; the set of words and all the relations of abutment between them). If we were to blockmodel  $G$ , the resulting image matrix would collapse structural equivalents.

This understanding of the  $E$  relation as one way of defining ‘meaning’ is probably intuitively acceptable. But were our dictionaries restricted to giving meanings only by exact equivalents, we would be in poor shape, as there are few

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<sup>5</sup> For example, when one of us was working to hook up an early allophone chip to an Apple II computer (the General Instruments SP0256-AL2), it came with three K sounds – one for *kit*, one for *cat*, and one for *coop*. (Try making all three; you form your mouth into a different filter for each depending on what is to follow.)

exact duplicate terms. Let us generalize somewhat. We know that if  $aEb$ , then  $a$  means *the same thing* as  $b$ , but we may describe its meaning somewhat more generally. (Here we draw loosely on formal concept analysis for our approach to formalization, though our basic approach is a general one in logic and is well explicated by Peirce [for example, 1982/1866, see especially lecture VII]; further, we adapt this for the case of a symmetric relationship.) Given the graph  $G = \{W, A\}$ , we may define for any  $v \in W$ ,  $\Gamma(v) = \{w \in W \mid vAw\}$  (read, for any word  $v$ , we construct the set Gamma that is equal to all the words that  $v$  abuts). Since  $aEb \Leftrightarrow [\Gamma(a) = \Gamma(b)]$ , we may say that  $\Gamma(a)$  is (in Morrisian terms) the syntactic meaning of  $a$ .

This is of course an immensely simplified scenario. Still, we can imagine successively relaxing some of the simplifications. First, imagine that we allow the  $A$  relation to be asymmetric. We then have a system that works rather well for the ‘bigram’ speech that tends to characterize the first linguistic productions of 2 year olds (‘get book,’ ‘more juice,’ ‘give ball’). If we distinguish subjects and predicates, we approach that sort of logic explicated by Peirce where we can use an asymmetric generalization of  $\Gamma()$  and its reverse to define the denotation and connotation of words. We can (as in the work by Franzosi (2005) cited approvingly by Biernacki) introduce a grammar by changing  $A$  from a graph (with an edge between two positions) to a hypergraph (with an edge constituting a relation between more than two positions, such as one that distinguishes between *subject*, *verb* and *direct object*). We can include more contextual determination if we feel it necessary. Finally, we can substitute quantitative degrees of relation for the binary ( $= \{0, 1\}$ ) relation we began with.

If we were to keep making these relaxations and allowing for more and more complications, we would, at the limit, have a hypergraph that necessarily (if uselessly) mapped exactly on to the existing use of the terms in our text or corpus of texts in question. Because this hypergraph of, say, *Moby Dick*, would be far more cumbersome and complex than the (already lengthy) original, there would be no reason to construct it. But imagine that we do construct the complete syntactic hypergraph from a text (call it  $G^*$ ). And that we use whatever techniques we normally would to construct a semantic interpretation of some or all parts of this. (This is what we do when we just sit down and read *Moby Dick*.)

The essence of our approach is to impose simplifications and collapsings of equivalence classes so that we have a much simpler graph  $G$  with much simpler syntactic structure. Let  $\varphi(G^*)$  denote an acceptable interpretation of the full structural graph that we would make of some text, say, with  $\Phi = \{\varphi(G^*)\}$  the set of all such interpretations. This set may also include interpretations of the *pragmatic* meaning. Now with our reduced graph  $G$  formed by our mapping, our argument is simply that any  $\varphi(G) \subseteq \Phi$ ; any semantic interpretation that we make with  $G$  is one that is adequate to the original; we do not deny that there may be elements of  $\Phi$  that cannot be retrieved from  $G^*$ .



In this light, we may understand our reductions as *functions* that map the original text ( $G^*$ ) onto a simplified representation. We do not carry out our functions directly on the meaning, but on the text; our hope is that certain functions on the text preserve certain forms of meaning *without bias* (which is not to say *without loss*).<sup>6</sup> We can insist that the function itself may (for some purposes) be hermeneutically indeterminate; the *outcome* of applying the function to a source text is not.

Such a reduction may be non-objectionable for large classes of intended interpretations, though not for all of them. The function on your computer's music player that allows you to speed up the music without raising the frequency is similar; up to a point, for conventional popular music and for the mid-range, you are unlikely to notice a difference. The function is *content neutral*. But where extreme frequencies are necessary to appreciate the music, the function may become problematic.

What we find so important about this technique is that if our assumption of interpretive adequacy holds, then we are able to make comparisons of the collapsed structures *without* carrying out the interpretation (what we called a formal interpretation of form). For example, we may say (and we have said) something of the order 'whatever Horkheimer and Adorno's work means, that meaning is closer to whatever Honneth's work means than it is to whatever Habermas's work means.'

In other words, we in no way assume that there is a single meaning to any of our reductions. The reductions are not themselves meanings or interpretations, but ways of facilitating interpretation. Just as a map allows you to go *many* places, and does not insist on there being only one route, so our reductions can support *many* interpretations.

### Enter Reed

With this exposition, we think that we are better able to respond to the ever gracious and perspicacious points made by Reed. Most of his argument is incontrovertible, as his method is often to take our piece and delicately pull and tease apart strands to make them clearer, and we do not disagree with his claims (though, of course, we do push him to settle on a monovalent reading!). But one key issue is the relation of our work to that of Mosteller which we brought up. We certainly did not mean to imply that the same considerations that led his approach to work for authorial identification were those that supported our methods. Rather, it was to remind readers that the rage they may feel welling up in them when we propose deliberate impoverishment of the source materials was

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<sup>6</sup> Biernacki correctly points out that our mapping is not an *invertible* function (p. 331); but this does not mean that it is not a legitimate function.

also felt by those who first heard of Morelli's approach; the Mosteller technique applies the same logic to *words*.

Further, we recognize that we are unlikely to be able to have – at least, *now* – the same mathematical crispness of Mosteller's work, where he is able to conclude that, for example, the odds that Hamilton wrote Federalist #55 is 240 to 1. But we can propose something analogous. Imagine that, using the notation introduced above, we find that, say, for  $a = \text{'reason'}$ ,  $\Gamma_{\text{Honneth}}(a) = \Gamma_{\text{Horkheimer}}(a)$ ; that is, that the graph neighborhood of *reason* for Honneth is the exact same as that for Horkheimer. While this of course does not prove that 'reason' means the same thing to both, it does suggest that the claim that the meanings are different has lost a great deal of plausibility. It seems as unlikely that the two would have the same neighbors for different *reasons* as Hamilton switching from *whilst* to *while*. Perhaps our odds are not 240 to 1. But 2 to 1 is pretty good.

Why are the odds not higher? Because we have relied on a *default* interpretation. By assuming the validity of our collapsings from  $G^*$  to  $G$ , we have treated all edges as equivalent. This assumption, as Biernacki in particular has demonstrated, is not always correct, and sometimes may be deeply problematic. Our recommended technique is structurally equivalent to the current state of the Google translator. This uses a similar formal method of structural equivalence to translate terms. Compared with a human translator, it is abysmal. Compared with the state of computerized translation 25 years ago, it is astounding. Most computational linguists would have been quite skeptical that something so simple could work as well as it does. There is every reason to expect that further refinements will increase the accuracy of its translation exponentially. That does not mean that we would prefer it to a professional translator if we had to translate, say, Horkheimer and Adorno's *Philosophische Fragmente*. But it might be better than using 1000 high school students to translate 100 000 short documents, if we had to. And we do have to.

## Defensible Simplifications

### Wholesale, not retail; mass, not craft

Theodor Abel, the German-Polish sociologist who studied and then taught at Columbia in the 1930s and 1940s, was on a commuter train, accosted by a typical American businessman who wanted to know what this egghead did. 'Sociology?' queried the companion. 'What is that? Like psychology?' To provide an analogy within his interlocutor's realm of experience, the sociologist replied, 'Yeeesssss, zomevhat. But psychology iz retail. Sociology iz wholesale.' It is our argument that while sociologists may of course engage in traditional interpretation as part of their sociological work, a specifically sociological contribution to

interpretation must needs be a wholesale one. If we all agree that traditional methods cannot be 'scaled up' for such wholesale analysis, the question is whether other approaches may prove adequate. As we see it, this can be rephrased as follows: given the full structuralist conception of meaning (which we hold to be true), are there defensible simplifications – collapsings of differences into indistinguishable classes – that would make a formal approach tractable?

Note that we emphasize not the perfection of these simplifications, but their adequacy. No one doubts that in interviewing there is a trade-off between the breadth that the impoverished survey interview schedule makes possible, and the depth that can be reached in a less-formalized instrument. There is also good reason to think that the variance in quality of the latter exceeds that of the former: there is amazing potential for *truly wretched* in-depth interviews, in which the interviewer does nothing else but twist reality to fit a pre-formed prejudice. This is the nature of craftsmanship – less external control means that when craftworkers are good, they are very very good, but when they are bad, they are horrid.

In any case, our argument is that we must consider methods that can be scaled up. It is common to bemoan the loss of craftsmanship. By this we generally mean not that there is *no* craftsmanship, but that the artifacts that most of us have – machine-produced in large batches – lack this craftsmanship. There are two separable aspects of this. One, which we push to the side, has to do with the loss of uniqueness (something that may bother the esthetician but is irrelevant for the scientist). The second, and more defensible, critique has to do with the quality of the work. The IKEA shelves that we put together are ingeniously designed to hold together so long as they are never more than 8 per cent from vertical, at which point, the entire assemblage will pancake. The shelves are, esthetically, about as pleasant as any other arbitrarily drawn rectangle. In contrast, the hand joined shelves next to them are both beautiful and incredibly sturdy.

No one doubts that workmanship of great craftsmen yields products superior to those of IKEA. But to bemoan the crudities of mass production is for each of us to wish for a world in which the odds are that we would have *no* desk or shelves, and certainly not the craft one. The combination of the increasing standard of living of workers, coupled with (in world-historical terms) a decreased appropriation of surplus by a tiny sliver allows us commoners to go from having *no* goods to having *adequate* goods. Here in the city of broad shoulders, we make no apologies for attempting to feed and clothe the world, nor to develop similar techniques for analysis in the sociology of culture.

Interestingly, the Frankfurt School was characterized by such a bemoaning; going far beyond the predictable petit-bourgeois reaction of intellectuals to massification, they actually identified with the nobility, albeit from the position of parasitic clerics supported by prebends. Whether or not this is a politically defensible position we leave for others to judge; we note this for two reasons. The first is that Biernacki is quite right: although the choice of the Frankfurt School

was originally made because it was a clearly bounded case of an intellectual *school*, offering a good ground for the investigation of the relation between cognitive and social organization, we took, and continue to take, great perverse delight in subjecting their critiques of instrumental rationality to instrumental rationality. Indeed, we will not rest until some day in heaven, Walter Benjamin turns to Theodor Adorno (or sends a telegram to him in hell) and says, ‘You know, Teddy, that computer has a much more accurate interpretation of my aesthetics than you ever did. You always wanted to *read things into it that weren’t there ....*’

The second purpose is more important. The Frankfurt School did not merely pine for the days of workmanship and creativity – they *scorned* and *feared* the *mass-produced* because they *scorned* and *feared* the *mass*. How good could something be, if *everyone* could have it, even the *hoi polloi* (Horkheimer and Adorno, 2007 [1947], p. 130, p. 212)? These disgustingly selfish sentiments were cloaked behind a barrage of double-talk and mystification, to the extent that it is still possible for a sympathetic analyst to try to ‘read’ their reactionary works as ‘liberatory.’ We must beware of any sentiments that could be used to justify a return to self-indulgent parasitism. We in no way suspect Biernacki of this: more than anyone else, he has demonstrated both his mastery of the sociology of labor, and the labor of sociology. But he must, we think, follow Weber’s ethic of responsibility and consider not only what *he* will do with his words, but what others will do as well.

### Reliability and speed, not perfection

We have – in good structuralist fashion – portrayed the relation of our approach to the humanist as analogous to mass production versus craftsmanship, and wholesale versus retail. We now wish to give two more specific and related analogies. First, consider the lowly thermometer. It is not that fever was undetectable before its invention. A skilled practitioner could probably detect even a mildly elevated temperature, though an unskilled one could not. Interestingly, it is probably the case that the invention of the thermometer changed what would have been understood as ‘temperature,’ collapsing it from a more multi-dimensional concept to a single point. Further, this involved a fiction of a single core body temperature, and, even more, the notion that there was a single, invariant, ‘normal’ one. We put that to the side for the time being, but do not mean to paper over potential deskilling and distortion involved.

Now consider an inventor who develops a different way of measuring ‘the’ core body temperature. This recently occurred with the development of the tympanic thermometer. The notion behind this is that the temperature of the tympanum correlates with the internal temperature, and can be measured via infrared radiation and reflection. Why the interest in inventing a newer form of measurement? The old one had two disadvantages. First, the unit was filled with

mercury, which led to a problem of contamination when thermometers were disposed of, or broken, especially when they were broken while still inserted in a human orifice. Second, they took some time to reach the core temperature, which means that if the school nurse leaves the office, the child is free to remove the thermometer and place it up against the radiator. The tympanic one was near instant and required no real cooperation from the patient. We will come back to this example.

Our final analogy is quality control. Agricultural products are now a very small proportion of the average American's budget. This is largely because their labor costs have been shooting downwards over the past 150 years. Many crops are planted mechanically, tended mechanically, harvested mechanically, cleaned and processed mechanically, and packaged mechanically. One important aspect of this is the removal of foreign particulate matter. If a person – even an exploited illegal immigrant – needed to comb through each bag of rice to remove, for example, moth larvae and eggs, the price would double. Instead, this is done by machine, to the rather generous standards of our government. For example, according to the FDA, The Food Defect Action Level (AOAC 981.19), in any 50 g of cornmeal, it is acceptable for there to be up to 1 whole insect, 50 insect fragments, 2 rodent hairs, or 1 rodent poop. Of course, chances are you would prefer fewer, and not more, insect fragments.

Consider the automated removal of pits from olives. It is acceptable to call olives 'pitted' so long as less than 1.3 per cent of the olives actually still have pits, or pit fragments 2 mm or longer. No one claims that the pitting machine is *superior* to a skilled olive-pitter. However, we think that it can be *adequate*, and allows for the processing of so many olives at so little cost that Kalamata olives can be incorporated in airline meals for economy passengers without fear of them choking on the pits.

We are attempting to put forward a technique that occupies an analogous position: one that is good enough for the processing of texts, and that can become better. Right now, we may be like the inventors of the wonderful automatic feeding machine from Chaplin's *Modern Times*, with glitches breaking out here and there. To continue the pitting analogy, we may only be getting 64 per cent of the pits. But that is usually enough to secure investment capital.

Our goal, at this point, is to seek formal techniques that are free from the problems of coding, and for the mass of cases that are relatively straightforward, but that would take time and effort, and which could be susceptible to downward interpretive permeation. Although we look forward to the day when this produces novel findings, we think that it is crucial now *not* to demand novelty from such analysts, for this would be to put them on the horns of a terrible dilemma. That is, Biernacki notes that our formal-aided interpretation adds nothing to what is well known about the relation between Honneth, Habermas and H&A. We totally agree – we intended this as a plausibility argument of our technique. If we had come to conclusions opposite to those conventional

interpretation had reached, our method would be discredited. It would be akin to claiming that the new thermometer, to be accepted, had to measure *different* temperatures.

We chose well-known texts so that knowledgeable readers could determine whether the method was reaching plausible conclusions. It is not a weakness of our technique that it, like others, ends any regress in informal, conventional interpretation. It is, as we go on to show, inherent in the nature of the beast.

## A Reliance in Informal Interpretation

### Anchors and anvils

We believe that we did not clearly communicate the nature of the relation of our proposed approach to traditional interpretation. First, we believe that Reed mistakes us as saying that theoretical interpretation lies wholly in the space of the humanities and should be, or could be, escaped. Although we do claim that our approach requires less initial theoretical guidance than does coding, it in no way allows us to dispense with interpretation. Similarly, we think that it is important that our (occasional and incidental) critiques of any method that leaves complex interpretation in the hands of unreliable methods in no way indicates that we think that traditional interpretation cannot work, because the same fundamental principles that anchor such approaches anchor our own.<sup>7</sup>

Second, we do not claim that our own approach is *superior* to humanistic reading. We do claim that it is (i) superior to coding; (ii) free from the flaws of the most deadly arguments against coding made; (iii) potentially more *reliable* (not more *valid*) than humanistic reading, especially humanistic reading in the hands of hurried analysts; (iv) improvable and (v) specifically in line with the core commitments of social science.

And most important, we in no way think that our method always allows for *dispensing with* the act of interpretation. Our argument is that it facilitates the making of valid, if simple, interpretations. So when Biernacki proposes that our method is parasitic on humanistic reading, we happily agree, as long as two things are understood. The first is that what we mean by 'humanistic reading' is simply the substantive interpretation of content, made with as much relevant accessory documentation as can be plausibly assembled. Whether this (which Biernacki himself proposes) is in fact the modal approach in the humanities we do not pretend to judge. The second is that what we mean is that *in the last analysis*,

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<sup>7</sup> Thus we never meant to 'demote' detailed historical work at all! We absolutely consider it the court of final resort when a finding is challenged, as in Biernacki's example of the debate over history of the use of *Beruf*.

where there are plausible doubts about an interpretation of a simplified diagram, we must increasingly zoom in, perhaps until reaching the text itself.

In our article, we did not return to the texts (see Biernacki, p. 333) merely because Lee's exhaustive research which is predominantly archival supports the arguments made. To some extent, just as we will see below that our interpretive *failures* bolster our points, so our *successes* undermine them; there is a slight of hand here, in that without the years of archival work, including reading the correspondence of the actors, as well as interviewing past and present Frankfurt school members, Lee would have been unwilling to believe the formal results. It was only when the outlines of the maps were reasonable that formal procedures such as the quantification of overlap could be judged reliable enough.

Reed's critique, however, is more specific: he also points out – correctly, we think – that when we used previous research to orient our search for the commonalities between authors by focusing on the subgraph connected to the term *reason*, we are already building in some of our conclusions. (This might seem similar to the problem Biernacki pointed to in the construction of a population of texts from which to sample, which can assume what is to be proven.) We in no way deny that this is an accurate gloss on *this particular practice*. But we do hold that it is not an intrinsic limit to our method, as it is for coding. Rather, this example shows that our method can go beyond the most naïve analysis; in this case, it works as a formal adjudication of potential interpretations that *begin* from a (presumably) shared presupposition that 'reason' is vital to the interpretation. Were we not at *this* level of agreement, we would not propose a technique that is only equal-handed *conditional* on the acceptance of the importance of 'reason.'

That our techniques can begin from places of consensus to explore the remaining uncertainties means that we can *cumulate* on the basis of previous adjudications. Now that we all accept, say, that Fromm and Marcuse see Freud as relevant to the same horizon of potential meanings, we can focus on these, and see if they embed these differently in wider conceptual structures.

### Limitations

We always emphasized that our techniques did not *replace* interpretation, but *delayed* it until that point at which reader and writer can survey the same reduced representation. There are three sorts of limitations to our technique, all of which Biernacki has pointed to. The first is that the reduction itself may lose information that is necessary for an accurate interpretation. The second is that the technique does not itself supply the context that is necessary for an accurate interpretation. The third is that the technique may actually introduce biases toward a certain type of direction. As we have already discussed the first critique, here we wish to briefly explore the second and third.

One of Biernacki's crucial points is that 'meaning and intention stand at a remove from bare expression.' We think that this dovetails with Reed's argument about our slipping into an analysis of 'what people mean when they write.' As Biernacki points out with our Borges example, our method does not reach what we called (following Morris) the pragmatic meaning – what the author means to do with the semantic meaning. To return to the example we used when sketching our structuralist approach to meaning, we there argued that what the word reason (*Vernunft*) means is nothing other than all the positions that it occupies in the texts produced by the Frankfurt school theorists. But we recognize that this is only a first cut; more important than what 'the term' means is what, say, H&A *mean by* reason at any point in the text.

To pursue this example, let us switch to a different (as well as better, more consistent, and more interesting) work, Adorno's *Negative Dialectics*. Here (1973 [1966], p. 317) we read,

The irrationality of the particularly realized *ratio* within the social totality is not extraneous to the *ratio*, not solely due to its application. Rather it is immanent to it. Measured by complete reason, the prevailing one unveils itself as being polarized and thus irrational even in itself, according to its principle. Enlightenment is truly subject to dialecticism: there is a dialectic taking place in its own concept.

What does Adorno *mean* by 'reason' here? What does he *use* the term to accomplish? This example shows us nicely something characteristic of his usage (including that with Horkheimer): 'reason' is what one deploys when one needs a place to stand – a place that one has, by the way, just shown to be wholly illusory – to criticize not only reality, but even ideality, at least, ideality in either the sense of any actually existing structure of subjectivity, or in the sense of any plausibly philosophically imaginable structure of subjectivity. All these are found wanting (irrational) in comparison with the standard established by \_\_\_\_\_. The problem Adorno has is that he has no term that can be used to defensibly finish the sentence. 'Reason' is what he deploys to avoid this difficulty.

*This* pragmatist notion of the meaning of reason is, we believe, the most important one to reach. Our semantic approach is useful as a means to that end; we reject any claim either that a 'subtler' semantic version is *necessarily* superior to ours, or that *any* semantic meaning should be preferred to the pragmatic one. In this case, we believe, the pragmatic approach shows that the reason *Reason* means so many things is not that it is a complicated term with many dimensions missed by our crude measures. Rather, it is something closer to a slur word or hate speech – a term that can be profitably *used* but is unlikely to be well *defended*. We look forward to a day when this interpretation becomes non-contested, perhaps by the establishment of a good point estimate of a coefficient of 'indefensibility,' with narrow error bars.



Thus, it is our hope that our proffered techniques can arrange the bare expression in such a way that the outlines of the intended meaning are visible. So far, we see no strong evidence against us here. However, that does not mean that we may not have an actual bias, even if it is of a rather formal kind, and still less pernicious than the problems of positive-bias associated with the downward permeability of coding. This bias, as Biernacki has emphasized, is a tendency to employ a single default interpretation of what an abutment means. It is not impossible, but it will take great presence of mind to avoid collapsing adjacencies to positive copulae (*is*, *connects to*, and so on). In particular, Biernacki is right that our procedure makes it easy for us to slip into indicative voice (p. 335).<sup>8</sup>

Biernacki busted us for (somewhat like Evans) taking a sentence from Chapter 8 of Habermas's ToCA to explicate our map from Chapter 6. Guilty as charged; we only plead the difficulty of condensing an originally larger, chapter by chapter treatment, into this form ... and, quite true to Biernacki's point, the inattentiveness to content that can strike those for whom this has become the least interesting part of the task at hand. Concentrating on making our *procedures* correct and replicable, we garbled the exemplary interpretation. We are, however, in a way fortunate that we made this embarrassing error. The problem is not just the squishing together of substantive arguments from different chapters. Biernacki makes a more important criticism of our reading: given the two sidedness of the relation between system and lifeworld in Chapter 6, why would we choose to stress the *unification* as opposed to *uncoupling*? Because, he says – and he may well be right – this constitutes the simplest possible substantive interpretation of the formal characteristics of an adjacency graph!

Thus we completely endorse what Reed calls our more 'modest' proposal – that the formal technique produces a structure that then must be interpreted. And it is for this reason that, for example, we cannot go from the fact that two authors use the same structure to concluding that they are saying the same thing. Two authors might both tie *infant* to *sadism*, *mother* and *attachment*, while one is saying 'contrary to old fashioned notions, we now know that the *infant's* relation to the *mother*, far from indicating *sadism*, is based in a reciprocal *attachment*,' while the other says 'sadly, the popularity of notions of *attachment* neglect that the relation of the *infant* to the *mother* is fundamentally one of *sadism*.' The same structure may need to be interpreted differently.

Even more difficult is when multiple perspectives are expressed by a single author. We confess, then, that our approach is biased away from ambiguity and polysemy. But that does not mean that this bias is *total*: a structural arrangement can do quite well at indicating ambiguity or ambivalence.

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<sup>8</sup> He notes that this shift to the indicative considerably flattens *Dialectic of Enlightenment* with its unique 'mode of communication.' But perhaps, just as some men are decidedly the better for being knocked down, so too some texts are improved by having their constative components laid bare, and their ornamental, posturing, phony filler and fluff disposed of. In any event, we shed no tears for the lost 'nuance' of DoE.

*Health – wealth – productivity – division of labor – specialization – one-sided – stupefaction – illness* is a simple chain (here a dumbed down version of Adam Smith's ideas) that suggests that the concept of the division of labor is equivocal. Of course, the same structure might appear if the text were both reviewing an opponent's position (to be dismissed), and putting forward an antithetical one. It is for this reason that we are interested in how references to interlocutors appear in the text. If, for example, the first four terms were all tied to SMITH and the last five all tied to MARX, we would suspect that our author was trying to use the two to bring out these clusters of arguments ... through whether she is accepting one, or both, or neither, we do not immediately know without interpretation, as we always confessed.

Thus, in addition to the generic admissions of imperfection that we made previously, Reed and Biernacki identify areas where we should expect weakness – not only a difficulty in recognizing ambivalence or polysemy, but a tendency toward interpreting all adjacencies as positive in some way. But we do not see this as fatal for our proposals. When Biernacki points out – correctly – that we basically argue that our method should pass scrutiny even if it is far from perfect, he bases his dislike on an absolutism that we, in general, share and respect. But given that our technique is akin to the automatic olive pitter, we accept it knowing that its accuracy will always be less than 100 per cent. Whether people adopt presumably will involve a tradeoff between the gains (transparency and speed) and the losses (inaccuracy, simplism). Let us examine some of the losses identified by Biernacki.

## Critique and Meta-Critique

### True or false

Before starting, one small correction: Biernacki points out that we inadvertently forgot to put the English versions of the texts used in the reference list; we do so here, and we also have now placed all the code used to carry out the analyzes on a website ([monicamlee.wordpress.com/home/code/](http://monicamlee.wordpress.com/home/code/)). We cannot place the texts themselves there, because they are still under copyright. But others' results, if they use the same processing we did, should be identical.

First, we would like to address a criticism in which we think our method actually has demonstrated an interesting, and perhaps unanticipated, strength, and this has to do with the reference to interlocutors. It is quite true that we argue that the lack of a specific mention of another thinker is – as Biernarcki correctly notes – preliminary evidence of a lack of influence. Biernacki suggests it is 'otiose scholasticism' to reference an important predecessor if the influence saturates the work; while we do not accept this as a generalization (look at how frequently,

say, Wolfgang Schluchter cites Weber!), we do accept that there are certainly *times, places* and *genres* where this is correct. In early eighteenth century England, where even in diaries and letters, names were never written out entirely, it was quite common for even an explicit reference to be unnamed. ‘An ingenious gentleman has recently written ...’ This does indeed frustrate any attempt like the one we proposed here, but we still do not accept Biernacki’s more specific claim. We actually believe that – like Spillman says – there is something about the Frankfurt school’s way of working, one that turns on theory building via explicit engagement of previous texts, makes the application of our still admittedly word-adjacency approaches plausible in a way they might not be for other cases.

But is it so obvious that Marx *is* used in *Dialectic of Enlightenment*? We think not. There is a vague flavoring of Marx thrown in, but if Marx’s *specific* ideas were to be used, perhaps he would be referenced. Certainly, one can never have read a page of Marx and yet be aware that society has classes, that some people are manual workers, that commodities are exchanged, that some capital is fixed (an idea Marx gets from Ricardo) and so on. But these are the sorts of statements that would support the reading of *DoE* as having received influence from Marx. Because the Frankfurt theorists are known to be ‘Marxists,’ and indeed, lived in a Marx-saturated intellectual world, perhaps Biernacki is simply *assuming* that they are referencing Marx – he *says* they are, but if we doubt it, as we do, what evidence does he bring to bear on the question?<sup>9</sup> Our criteria, in contrast to Biernacki’s, for making this judgment may be simple – the authors actually have to type the word ‘Marx’ – but at least you know what they are.

Relatedly, we actually think that there is an important finding in our argument about the use of Freud by Fromm and Marcuse. We claim *both* that this agreement is a real agreement, and that there was antinomy and antipathy (as seen in the *Dissent* debate Biernacki references). We are not claiming that the animosity comes because they are splitting hairs (as in the fervor with which Old Believers and New Believers in Russia argued and were forced to divide over the question of whether the sign of the cross should be made with two fingers or three

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<sup>9</sup> It is interesting that the critical theorists, although starting with Marx in their youth, quickly abandoned any serious engagement with core issues of dialectical materialism. While they are often remembered as trying to integrate Marx and Freud, this is quite inaccurate, as can be noted by comparing them to the intellectuals at the time who *did* make such an effort (for example, in the United States, the circles around the journal *The New Masses*). Critical Theorists instead proposed a two-tier system: Hegel for the elite, and Freud for the *hoi polloi*. Note that we do not deny that early in their history, the Frankfurt school included many serious Marxists, nor that there were later occasions of real engagement with small aspects of Marx’s work. But this supports our claim: such real engagements are found in the proximity of the word ‘Marx.’ For example, Adorno (1973 [1966], p. 189f) needed to determine the relation of his ‘negative dialectics’ to notions having to with the objectification of relations; here, he turned (explicitly) to Marx’s chapter on fetishism, which demonstrates that he meant this understanding in particular, and not close relatives (such as Lukács on reification or Simmel on crystallization).

**Table 1:** Most common words near ‘Freud’

<i>Marcuse</i>	<i>Fromm</i>	<i>Adorno</i>
instincts	man	art
civilization	human	language
principle	character	like
reality	individual	work
repressive	society	dream
theory	others	form
pleasure	sexual	made
individual	person	die
freedom	psychology	hope
life	nature	lines
man	drives	myth
historical	psychological	spirit
new	certain	way
ego	child	works
mental	important	expression
human	love	fact
development	phenomena	it
domination	satisfaction	letters
eros	assumption	love
work	development	man
psychoanalysis	essentially	Orestes
social	like	point
death	—	time
father	—	words
sexuality	—	—

Eliminates articles, conjunctions, prepositions, auxiliaries (for example, should) and names of references; combines forms (instinct/instincts/instinctual).

fingers). Rather, our point is that the two are forced into conflict because they are indeed trying to use Freud for the same purpose – to make statements about human nature, childrearing, and the implications for social reform. That is, our *pragmatic* interpretation is, in cartographic terms, that ‘Freud’ is being *used* to pull these concepts in and attach them in the proper place to the emerging concept map.

Table 1 shows the most frequent words found in the vicinity of ‘Freud’ for Fromm’s *Escape from Freedom*, Marcuse’s *Eros and Civilization*, and Adorno’s *Notes to Literature* (which drove our finding in Lee and Martin, 2015). (We shoot for 25 words, but to avoid breaking arbitrarily on ties, use 24 for Adorno and 22 for Fromm.) Marcuse’s concerns clearly turn on *Civilization and its Discontents* – the *theory* of the *repression* of *human sexuality* and the *pleasure principle* in favor of the *reality principle*, leading to a new *historical development* in which the *domination* of *eros* leads to a loss of *individual freedom*. Fromm’s concerns are closely related, but he seems more generally concerned with the *psychological* process of *character development* whereby a *child* with *sexual drives* finds a place in *society* as a *human individual* capable of

*satisfaction* and *love* in relation to *others*. The concerns overlap, although they are not identical.

One might think that this goes without saying – an engagement with Freud that did *not* deal with these issues is impossible. Yet we find that Adorno manages to use Freud without bringing in any of these themes. Instead, he connects Freud with issues of externalization – the ways in which *man* can *express* his *spirit*: in *art, language, myth, work* and even *dreams*. Our finding was a real one, then, even if at a very basic level. Thus the point that both Biernacki and Reed makes about the ease with which we may forget to interpret our reduced representations is quite valid here – Marcuse and Fromm may not exactly agree about what Freud *should* mean for critical theory, but they (and not Adorno) *do* agree as to what Freud's possible relevancies *are*.

Once again, as all our critics have emphasized, this is only the beginning, not the end, of an interpretation. Yet the overlap is an important place to start – and, interestingly, it was only the formal technique that clued Lee into the fact that the leaders of the school seemed more prone to try to expel marginal members when they *increased* their overlap with the leaders' own concept map than when they diverged from it. Whether this is a general pattern in the formation of intellectual schools of course remains to be seen.

### Reason and irrationality

Biernacki also argues that our approach, like those he previously criticized, atomizes a whole work and therefore is unable to deal with the meaning as it exists. This point may be generalizably valid (we go on to consider it more closely below), but merely enjoy with us the delicious irony of Biernacki criticizing us for reducing to fragments a work entitled .... 'Philosophical Fragments.' The 'reason' (but *not* in the sense of *Vernunft!*) that the Frankfurt School wrote in fragments was (here we give the briefest possible thumbnail sketch), that they saw, like the neo-Kantians, that every cognitive engagement with the world was partial and, directly or indirectly, bound up with the organic needs of life. But rather than embrace this, as did the Nietzscheans in Germany and the Pragmatists in America, they chose to, with the utmost verbal prolixity, complain and sulk about it, drawing it to others' attention as if it were a remarkable discovery, while documenting that it had been recognized with complacency by everyone since Fred Flintstone. To move ahead fearlessly *without* a system – that would mean abandoning the philosophical tools in which they had invested so much. To make a *new* system – that would be obviously invalid. To continue to circle around and criticize *others* – why, so long as one could secure continuous funding, that was a jolly way to run out the clock.<sup>10</sup>

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<sup>10</sup> Again, we think that the best justification for this approach is found in Adorno (1973 [1966], p. 32, p. 28): We have been forced to face the negation of all convenient systems of thought. But we do not

For this reason, the works of the critical theorists *themselves* contain that sort of ink-blot quality that allows for undisciplined, projective, and ‘creative’ reading. As we noted above, Lee (2013) argues that Benjamin was increasingly elevated to be a core member of this school because the aphoristic style of his writing made him better material for the creation of arbitrary re-interpretations, which is what Adorno wanted, as he completed the transition of the school from historical materialism to undisciplined esthetics. A serious interpreter of his work often finds herself puzzling over how to deal with a particular passage: brilliant insight? Absurd pretension? Embarrassing ignorance? Or drunken word vomit? Nothing is more deserving of being scanned, digitized and shredded.

### Lose the battle, win the war

We may not have well defended our particular interpretations. But note that the gravest critique that Biernacki (p. 323) makes of our approach is that it is basically impossible to even begin to try to evaluate a map to determine *how* off target it is. Yet here Biernacki does just what he said he could not do, namely challenge some of the particular interpretations that we made. (Similarly, where our interpretations are boringly unoriginal, he makes this critique as well.) Perhaps he was only being kind when saying that the results could not even be critiqued, to avoid finding other errors that we made, but we believe that he understated the *ease* with which our maps could be the focus of just that sort of intersubjectively valid contest that we proposed was the ultimate justification for our approach. In sum, we think that Biernacki’s often *successful* critique of our substantive claims demonstrates our more important meta-claim that these were to illustrate – that counting, unlike coding, does not necessarily put the interpretive moment back stage, where it cannot be seen.

Thus Biernacki’s negative critique of our interpretations does well to support our overall position; so, too, we believe, does his positive argument for the advantages of humanistic interpretation. While our argument was *not* against traditional interpretation, we do here wish to argue to sociologists that while Biernacki’s own interpretations may be impressive and defensible, the *defense* he makes of them is one that brings in too much garbage with it; he may be good at weeding out the insect parts, but not all of those whom we would replace with machinery are equally accurate.

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want to abandon thought! Shall we make a *new* system? ‘But the responsibility of philosophical thought in its essential situations is not to play this game. A given alternative is already a piece of heteronomy.’ How, then, does one continue to kick a philosophy when it is down, without even offering a replacement? ‘Only a philosophy in fragment form would give their proper place to the monads, those illusory idealistic drafts. They would be conceptions, in the particular, of the totality that is inconceivable as such.’

## Biernacki's Uncertain Allies

### Paving with good intentions

Biernacki says that 'We seek operational transparency in experience rather than mechanical transparency in the abstract.' We think this is indeed a wonderful way of putting the goal of thorough and rigorous humanistic interpretation. Seeking and finding, however, are not the same thing. It is our argument that, first, the lack of intersubjectively valid standards or criteria for 'operational transparency in experience' has led, and will continue to lead, to serious interpretive problems in sociology.

Biernacki correctly points out that our approach promotes *reliability*, but does not guarantee *validity*; and neither he nor we think that reliability is a substitute for validity. And neither he nor we think that *coding* promotes *either* reliability *or* validity. But our concern is that he advocates junking an approach that might promise reliability (and no worse validity than others) because of a promise – one which we do not trust – of validity without reliability.

Biernacki's humbling brilliance here again is used never for self-aggrandizement but for the purposes of advancing serious scholarship. Were we all to follow, our technical proposal could safely be withdrawn. As James Madison wrote (or was it Alexander Hamilton? Well, there are two *whilsts* and no *while* ...), we do not write our constitutions for angels (Federalist #51); nor does one have to sully the reputation of Cincinnatus to justify a constitution designed to prevent a dictator from ever arising. We trust the conclusions that Biernacki himself would put forward. But as Biernacki begins enlisting his allies, some of them seem quite troubling to us – precisely the sorts of things we are trying to *keep out*.

Biernacki correctly points to some of the dangers of curve fitting – connecting dots opportunistically to make a pleasant pattern. But such false positives are a danger for all interpretation, and we are concerned that Biernacki is not obviously as vigilant at exposing them when they are surrounded by pleasing rhetoric than when they are simply dots and lines. Indeed, Biernacki himself seems unsure as to the status of those interpretations that he cannot defend but does not want to condemn.

Instead, he seems to stake out a strange compromise. Freud's *Totem and Taboo* is 'extravagant' (just as other interpretations Biernacki likes but knows are wrong are 'outlandish' [p. 44]). Still, Hunt's use of it as a 'frame of reference' lets her 'explore' 'new kinds of subjectivity.' This seems to justify, at least partially, both her interpretation and, indirectly, Freud's, as a means to the end of this 'exploration.' We find it hard to believe that without accepting the nonsensical extrapolations of Freud, based on no historical research and in flagrant contradiction of the scholarship of his own day, Hunt would have been barred from exploring subjectivity. We think the Biernacki here is divided against himself – on

the one hand, he believes in science and holding himself to the highest standards of dry scholarship, but on the other hand, he retains a soft spot for 'creative' (if wrong) work, so long as it is well written. Once again, we ask him, whether he stands with or against Weber; with or against science as a vocation: serve you Athena, at the expense of insult to Apollo?

We note that as Biernacki gets closer to the humanist interpretations that he wishes to defend, he drops the vulgar criteria of right and wrong for fuzzier ones. Sedwick's work demonstrates not that she is right about Sterne's text, but that thick description can 'sustain' a 'generalizable story about cause and effect.' We can be sure that, say, reading the concerns of liberal 1980s academics back into the world of eighteenth century writers can 'sustain' all sorts of things. But we notice that Biernacki does not evaluate it in the right/wrong terms that he finds able to apply to our simple diagrams (as well as to Weber's historical claims). We think that this in itself is an important point in our favor.

Similarly, we find his criteria of 'systemic coherence, contextual breadth, and ramifying power' (p. 341) hardly comforting. They seem to fit the aphorism of Nietzsche we quoted previously: people will believe anything that is *well said*. This is not the solution, this is the *problem*. We might say that until Biernacki can provide us examples of successful humanistic interpretation that are written *clumsily* but are still convincing, the examples will do more harm to his case than good.

We emphasized that while our argument is not that humanist interpretation is unable to reach valid interpretations, it *was* our argument (and we think of this as being one that is rarely contested) that such interpretation does not make itself amenable to the trials of strength via the mobilization of non-human allies as does science according to Latour. Biernacki himself increases, not decreases, our concerns here – he indicates that such interpretations can be judged by 'explanatory pay-offs,' but these seem a bit like early Soviet accounting: how much utility did your factory produce last month, comrade? Usually, the answer is 'lots.' We cannot really pursue this issue here, but hope to enlist Biernacki's own concern for intersubjectively valid standards of evaluation behind a project of formalization ... even if not this one.

### **Abduction and cocaine**

We think Biernacki tips his hand when he refers to 'abduction.' Scientific logic has for centuries struggled with the issue of what in statistics is called reverse probabilities; it is widely accepted that going one way (from a hypothesis to the likelihood of data) is easily handled by any variety of logic that has been developed, but going the other way is difficult, and the generation of hypotheses is unformalizable. (This was the point of Peirce, ignored by his would-be followers who claim to formalize abduction as a process.) Those who are *confident* in their abductive powers tend to be cocaine-addled (as in Freud and



Peirce) or fictional (Eco's William of Baskerville) or both (Sherlock Holmes) – hardly good role models for scientists. Those who trumpet abduction substitute their personal, unjustified, and unjustifiable *conviction* that they are right for intersubjectively valid evidence, whether formalizable or not.

Cocaine-addled though he may have been, Peirce did not start from the attempt to torture logic so as to appear to justify that hysteria that sees secret messages or revelations written in trash or mumbled by strangers; instead, he started with the Leibnizian problem that the same data can be explained by an infinite number of theories, and attempted to tie his notion of abduction to a strict conception of probability (see Martin, 2015). The 'aha' experience, as he knew, is identical in the true discovery and in the hallucination. Freud certainly had this experience when he *realized* the relation between his incorrect interpretation of Exodus and his incorrect interpretation of human prehistory, coming up with the – or so Biernacki seems to suggest – remarkably useful lens for studying eighteenth century British fiction. Aha! *Now* it all makes sense! We do not seek to deny anyone this pleasant feeling, but we count it as worth nothing when it is time to evaluate the evidence.

Our refusal to be swayed by the subjective confidence of the writer who has undergone the 'aha' experience, and our insistence on supporting evidence is, we recognize, a costly and perhaps unpopular strategy – for counting, as Spencer-Brown (1957) would have said, puts us in the realm of probability, and destroys that drunkard's luck that we might otherwise have. Indeed, we cannot help but notice how much *luckier* many humanists (for example, those in English departments) are than sociologists. They seem to be able to find strong evidence of just the point they wanted to make, in just the texts that they happen to specialize in, and make fascinating and strong theoretical claims about, say, eighteenth century female subjectivity, when in sociology we cannot find any evidence that would help us out here, even when we look in all the more obvious documents. We, unfortunately, must substitute replicable rigor for luck.

### **Presentism and futurology, past and future**

It is to make possible such rigor that, as Biernacki (p. 327) is quite right to say, we treat the meaning of a text as fixed. He, however, seems to argue that this is an error because readers' 'own construals help to define meaning' (we say 'seems' because he begins with a hypothetical that implies but does not state that this is so). It is indeed quite true that *if* this were true, our method would be greatly limited. But so would all historical work that attempts to avoid presentist readings, and this is how we have previously understood his own argument.

Indeed, if 'our own construals help to define meaning,' doesn't this imply that if the meaning of the word 'vocation' changes with the rise of professionalization, acquiring new somewhat negative connotations, we should feel free to read that back into Luther? Further, if the context of current reading is a valid aspect of

interpretation, Biernacki's argument is almost certainly greatly weakened because, although we cannot take pleasure in this fact, time is on *our* side. Most probably, in 20 years, our students will 'read' their assigned works by glancing over animated versions of the sorts of concept maps we have presented. They will go to Amazoogole, begin viewing the animation (paying 3¢ a second, automatically deducted from their trademark-of-the-beast all-in-one account), and then push a slider over to indicate how much complexity they want in the treatment ... from 'very little' to 'very *very* little.'<sup>11</sup>

In any case, we think that Biernacki's troubles here are not a minor or a personal matter – they get to the fact that there is indeed an epistemic problem in humanistic interpretation (one denied by Spillman), one which allows grace of exposition to influence scholars' determination of what to a sociologist can only be considered matters of fact. Yet Biernacki sees himself on the other side – defending science against rhetoric.

### Truth and sophistry

Biernacki suggests that by following Latour, we take sides in the ancient philosophical quarrel that began with the sophists which he – following the Platonists – casts as one pitting 'genuine knowledge' as opposed to the mere *appearance* of such knowledge to others. He is exactly right – and exactly wrong. We absolutely *do* follow Latour (and, to some degree, Arendt (1958), and her influences Heidegger and Nietzsche) in rejecting the Platonic approach, which was (rhetorically) couched as an opposition to the 'sophists.' 'Sophist' is like 'hipster' – it's a term used by one member of a set against all the others. Latour (1999) – in his very interesting analysis of *Gorgias* – argues that Plato's actual opponent is not the sophist, but the *political actor*: the free citizenry of Athens. To disempower them, it was necessary that Plato first construct an illusory world of eternity to counterpose to the fundamentally temporal realm of decision, speech and action. Second, it was necessary that he claim to have privileged access to this realm, large parts of which were to remain a proprietary secret. Third, this could then be used to justify the *de facto* disenfranchisement of the previous elites.

We note, by the way, that the critical theorists, largely inspired by Nietzsche, understood the Platonic power play in very similar terms to Latour, but they were, after Nazism, incapable of taking seriously the most obvious alternative, which would be to return collective decision making to the flesh-and-blood humans themselves, instead of the 'Reason,' which the critical theorists both debunked and deified depending on the needs of the paragraph in question.

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<sup>11</sup> Sociology is in need of the interpretive discipline that comes with formal procedures; formal procedures may safely be turned over to computers. But don't forget: computers are *not* your friend. The world they will bring in their rise to power is a humorless and memoryless one. If you want to try to take them down, be our guest.

In any case, if Biernacki wishes to make a last stand with Plato, he is right that we are on the other side.<sup>12</sup> But we see the stakes quite differently from Biernacki. Plato did not achieve genuine knowledge of *anything*, nor was he particularly interested in doing so (though Socrates may indeed have been interested in removing false knowledge, it seems historically unwise to oppose him to ‘sophists’). Plato was, we remember, the author of the noble lie that was designed to give him and his beautiful (and rich) young noble friends (those possessing *kalon kai agathon*) control over others’ lives. We join not the sophist Gorgias but the political actor Callicles in rejecting the claim of some to have access to an invisible world of subtlety that allows them to make claims that others cannot (for example, Plato, *Republic* 500c). As Latour would say, Plato invented the most stunningly effective form of rhetoric ever devised – that which effectively denies its rhetorical nature. Once again, we emphasize that we do not believe that this characterizes Biernacki himself. Were the emperor to parade around naked, he would point it out. But his arguments now give aid and comfort to those who would attempt to silence him when he did.

Reed suggests that there is something wrong with such acknowledgments – that Biernacki may not need formal techniques to do rigorous works, but that others do. Or at least he takes issue with the idea that some people (such as Clifford Geertz) are excellent interpreters, but that they cannot be imitated. This is not quite what we are saying about Biernacki; one of us uses his first book precisely as an exemplar to be imitated. We are saying not that no excellence can be imitated, but that many people are *not to be trusted*. Clifford Geertz, for one. Not that we have any opinion about the claims made in ‘Deep Play.’ But the one of us who uses Biernacki’s book as an exemplar also sees many students claiming to take inspiration of this essay by Geertz, and wandering off into total gibberish. Why? Geertz misleads students as to what his actual method is – as if he, finding himself at this fascinating event, ‘traced out cultural associations’ ... and so on and so forth.

Geertz published this essay in 1972, after around *two decades* of intensive study of Indonesian culture. Anyone who wishes to imitate Geertz’s *actual* methods is free to. But someone who thinks that they can write an equivalent text based on their 1 month’s observation is likely to end up in a very bad place indeed. ‘Deep Play’ is a virtuoso display of distraction; like any good magician, Geertz makes it look easy. As with sawing a lady in half ... please do not try this at home.

Thus we reject one aspect of Reed’s critique that to him seems to be too obvious to require defense, and this is that there is such a thing as ‘hermeneutic

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<sup>12</sup> With one minor exception: we ask Biernacki if he agrees with Plato’s opinions on ‘The lowly business of distinguishing the one, the two, and the three. I mean by this, number and calculation.’ Plato (who is being ironic in calling these ‘lowly’) seemed to think that ‘*every* kind of art and knowledge is compelled to participate in them’ (emphasis added) (Plato, *Republic* 522c).

sociology.’ Here, at any rate, we stand side-by-side with Weber: in all sociology that attempts to address issues with ‘human meaning,’ there is a moment of interpretation. (There may be some specialized subfields that do not do this, but we leave that to the side for now; certainly, little of the sociology of culture will fall into this category.) If, say, Judith Blau’s formal approach to explain variation in the degree of cultural production at the city level is, as we accept, also an act that requires interpretation, the question would be, what does it mean to claim to be doing specifically hermeneutic sociology, as if this were a method?

Reed writes, ‘the essence of hermeneutic sociology is to bring some of these techniques of interpretation out into the social world more generally, to examine the complex meanings, not only of debates in critical social theory, but also of arguments about Super Bowl commercials, popular responses [to] the O.J. Simpson trial, conversations overheard at meetings of Church volunteers, and much else besides’ (p. 362).

Recall that Biernacki’s defense of traditional hermeneutics involves the claim that it involves a well-established and rigorous method for the treatment of texts (for example, Biernacki, p. 339). But how do we bring such techniques from the study of texts into the social world more generally, and apply them to something like ‘overheard conversation’? It seems that rather than speaking of *techniques* in the sense of a *means* to an end, Reed seems to mean the end of *claiming* that one thing means something else. Most worrisomely, it seems like a license we write ourselves allowing ourselves to make a little data go a long way.

Indeed, Reed thinks that these data like the overheard conversation and the superbowl commercial will allow us ‘... to examine the complex meanings ...’ therein. Here we borrow the formulation of Hotspur’s reasonable if teasing response (*Henry IV Pt 1, Act 3 Sc 1, line 54*) to Glendower’s claim to be able to call spirits from the vasty deep: ‘But will they come when you do call for them?’ We understand that Reed would *like* us to be able to examine the complex meanings in the arguments about Super Bowl commercials. But that does not mean that he, or anyone without clear methodological tools, *can* ... and certainly not that this has anything in common with what we call sociology. It sounds suspiciously like what people in English departments were doing circa 1988 – making big claims that sounded impressive with little data, less method, and no justification.

We are unhappy to have to report that, as far as we can see, the answer to the question we posed – what does it mean to claim to do hermeneutic sociology – is that one plans *not* to do sociology, but to *call* it such. One can pore over advertisements for Virginian Slims and ‘interpret’ out of it a claim about gender, and attempt to use this to intervene in sociological debates, but we hope that sociologists merely raise an eyebrow at a perhaps provocative, but untested, hypothesis, and neither publish it in our journals nor cite it as evidence. If there is a formula that allows sociologists to state a hypothesis as if it were fact, it has not been shared with the rest of us. To claim that interpretation is some

self-substituent methodological approach would be, at least again so far as we can see, to declare that one has an unlimited capacity to write checks that reality is unlikely to cash: enjoying the fruits of casual café theory without admitting that one is abnegating one's responsibility as a social scientist.

Once more, we do not claim that Reed, most obviously in his recent work, engages in such dilettantism. (Again, we use his work on resignification as an exemplary piece for students to learn cutting edge methods in cultural analysis.) This work could simply be described as 'historical sociology,' though of a particularly creative and theoretically informed sort. Reed and Biernacki suffer not from bad practice, but a bad theory of their own practice, one that aids and abets the enemies of the rigor that they espouse and exemplify. We hope that Biernacki and Reed do not, like Clifford Geertz, wittingly or unwittingly play the part of the pied piper, bringing yet another generation of promising minds off of the cliff of pseudo-science.

Once more, let us clearly stand with Weber – and again push Biernacki and Reed to indicate whether they reject his claim that you serve one God (such as science) and offend the others (such as estheticism). We are ethically bound to follow our *own* calling, and not make nighttime raids into neighboring disciplines because we want to easily bring back booty we are unable to produce ourselves. If we lack specifically sociological techniques to create such findings, we must work to create them, no matter how painful and long the process. It is this goal that inspires our continued efforts toward consistent formalism – even at the cost of initial simplisms that may seem as ridiculous as Woodworth's *Thorn*.

## Conclusion

### Our position

To summarize, we first briefly respond to Biernacki's main points on (p. 337) as follows: (i) While he argues that our reductions are not hermeneutically indeterminate, we argue that any meaning imposed is a generic one, though, as Biernacki says, biased toward the positive and indicative; (ii) We do not deny that the meaning is not preexistent in the word tables; our argument is that we can construct reduced depictions that can be used for interpretations, as can the original. The lack of meaning is therefore a plus, not a minus; (iii) There may indeed be difficulties in generalizing the technique to other authors and other questions, but this is an empirical problem and hardly unusual for researchers working with new techniques; (iv) Biernacki argues that isolated words are not parcels of meaning, which is of course true in an absolute sense. Still, words are certainly *something*, and they are probably as close as we get to parcels of meaning. That's why there can be dictionaries. (v) We absolutely agree that our

approach is parasitic on humanist interpretation; the reduced representations still need to be interpreted, and in the event of confusion or disagreement, we can only go back to the original texts; (vi) Biernacki correctly points out that our mappings are non-invertible; but this is intrinsic to their utility. Because they are many-to-one mappings, they simplify, but one cannot simply back up; (vii) Most importantly, Biernacki emphasizes that we still need an orientation to context for how to interpret counts or even a map, and that the map itself does not supply this. We see this as a very important issue. Combined with Spillman's point (see below), we would say that yes, we need a sense of 'what kind of a map we have' based – so far as we *now* can see – on other contextual information, before we know how to interpret adjacencies. This suggests that the approach is robust within equivalence classes of texts, and may be extendable; (viii) Biernacki argues that separation of quantification from interpretation makes interpretation more arbitrary, but here we think Biernacki uses 'arbitrary' to mean the opposite of valid, while we use it to mean the opposite of reliable. In our sense, subject to the qualification implicit in our response to (vii), our procedures are *less* arbitrary; (ix) Finally, Biernacki argues that word counts do not aid in explaining human action. We are happy to acknowledge this, but here, we are not explaining human action, but only the (reified) cultural product.

### The beachhead

We consider our work the successful establishment of a beachhead, and (not yet) a demand for unconditional surrender. We recognize that with innovation, it is often the case that the chutzpah of initial forays requires a somewhat insane underestimation of the complexity of the task ahead; no one realistically evaluating the rewards and risks would go forwards. But now that we are here, the question is 'Is there reason to lose heart? Is it obvious that we *cannot* go forwards?'

It might seem like special pleading, but we do think that it will be necessary to separate the admitted crudities of our first attempts here from the key issue of whether this is a way of developing techniques that are free from the fatal flaws that Biernacki has found in coding. We do not deny the importance of some of the critiques that Biernacki has made of our approach, but the question is whether these limitations are inherent and insolvable, or whether they can be transcended. Our structuralist vision suggests that – at least in the abstract, if not in practice – not only is there no inherent block to our capacity to further refine our techniques, but in a way, it is *necessarily* the case that our approach can be successively specified to approach a veridical mapping ... only at the cost of doing nothing (that is, we have a 1:1 scale mapping in which each word's meaning is simply how it is used). One might argue that with coding the same is true – in this case, the finest coding, where each text is in its own category, is tautologically a

perfect coding. But we believe that the *assignment* of meaning to the text makes this a different, and less promising, supremum.

Biernacki (p. 332) correctly points out that the mapping functions must be such as to preserve relational properties of arguments (though not, interestingly, of the texts themselves). It was and remains our belief that even the simplest graphs such as those we provided *do* this for the gross features of an argument. Spillman's point – that such simple approaches may only fit texts of a particular type – is an important caution. But we believe that moving to more complex mappings will be increasingly possible, and the application of the simpler ones may be more effective than we might fear. How will we tell? Only by exposing the results to destructive criticism from informed readers.

### Technocratic closure

In closing, we note that Biernacki's analysis of our tendency toward reified language (a preference for nouns, for passive constructions) assimilates to this to 'technocratic closure ... choking by dictatorship a language community in which meanings are constantly critiqued and renegotiated.' We *accept* this characterization, with small amendment. We *are* for the dictatorship ... of the *proletariat*, that is, of the value-creating scientific workers. Horkheimer and Adorno's attempt to rehabilitate the rule of intellectual clerics was as feeble as it was pretentious; while we all appreciate the virtues of petit bourgeois craftsmanship, it is simply unable to cope with the current forces of intellectual production. Absent the enlightened despotism of the best and brightest, which we acknowledge may be present in some historical interpreters, the *practical* alternatives to the coding that Biernacki has demolished are either the industrial production we offer, or the dictatorship of the lumpenintelligentsia of deconstruction workers. Social science – or barbarism. We agree with Biernacki – the center cannot hold.

### Response to Spillman

#### The critique of coding

Spillman, however, disagrees – there *can* be a center, and it is *interpretive* (see Figure 4, p. 381). One difficulty we face in response is that much of our argument began by accepting Biernacki's critique of coding: Spillman rejects our acceptance, but does not indicate where she finds Biernacki's previous analysis wanting. Indeed, she seems surprised ('this concern is hard to understand') that we make claims that follow directly from his. She says (note 4) that 'this is not the place to elaborate a challenge to Biernacki's conclusions,' but this reticence puts us in a difficult position: we cannot defend Biernacki's critique in addition to our

own response. Because it is really Biernacki who must respond to Spillman's arguments, our discussion here is somewhat shorter, focusing on the places where she engages our specific proposals.

Spillman is confident that there is no merit in the Biernacki critique and apparently trusts that she can give an existence disproof of the critique of coding by pointing to the success of her own work. Although we have no particular doubts about this (and, once again, point to her work as exemplary cultural sociology), unless we were to replicate her codings, as did Biernacki of others, we cannot really use this as strong evidence against him. We do note that such a critique would not be easy as Spillman says that 'following disciplinary norms,' she 'did not include any detailed consideration of ambiguities of coding' in published versions. This, of course, is exactly the problem pointed to by Biernacki and accepted by us.

To some extent, Spillman mis-reads us as saying that counting leads to analysis *sans interpretation*, when our argument is that the *counting* part is free of any but the most simple interpretation (for example, the dropping of standard lists of 'stopwords,' which, as Spillman would point out, is not a safe procedure in *all* contexts [for example, an analysis of Freud in German could not throw out *Ich* and *Es*]). Further, we seem to be interpreted as saying that to be *fair*, an analysis must be *thoughtless*, in the sense of never checking to see if matches are made on irrelevant material. We did not think it necessary to inform the readers that, for example, we do not include frontmatter in our analyzes, to avoid finding the perplexing connection between 'random' and 'house.' Most problematic, she seems to take our criticism as one directed against 'interpretive sociology' or 'qualitative' sociology, when it is specifically against *coding*.

At the same time, her reflections produce an important insight and qualification of our work: Spillman gives very reasonable suggestions as to when the simple version of the approach that we outline might be most useful. We believe that she has indeed identified the most important challenge to the generalization of our technique – some types of texts will make it easier for such cartographic techniques to reproduce meaning. For others, this approach – at least, in the simple form we outlined here – will not initially be adequate. We look forward to demonstrating that more sophisticated versions (such as those hinted at here) can allow the relaxation of such constraints. But she is quite right that we looked where the looking was good.

### **It's not so bad**

Thus where Spillman has engaged with our argument critically, she has discovered a real limitation. But (as Figure 4 suggests) most of her response bypasses our specific arguments, and indeed, is based on two strategies of dismissal. The first is to say that 'often' these problems do not arise, that 'many' cultural sociologists do very good work, and therefore we must be 'exaggerating'



to point to insoluble problems. It is very difficult for us to respond to such rebuttals. Should we take a random sample of cultural sociologists? How many is many? But even more important, our critique of coding (taken from Biernacki) has to do with its *logical* status; the use of counter arguments based on what are called ‘vague quantifiers’ (terms like ‘many’) do not engage with it. ‘Many’ astrologers had reasonable advice for their clients, and also could ‘often’ predict the motion of the planets. That does not mean that their failures weren’t enough to lead us to drop the whole endeavor kit and caboodle.

Her second way of dismissing our critique is through rhetorical strategies. The most important is to transform statements that we made about external problems of fact into expressions of our own subjective state. In a way, then, Spillman has demonstrated what we set out to claim far better than we could – that the epistemic problems of the sociology of culture lead it to need formal techniques to adjudicate claims because they will otherwise fall back on authority and rhetoric.<sup>13</sup>

### Epistemology and authority

Spillman rejects our argument that the coding approach is epistemically unstable, as is traditional interpretation, the stabilization of which requires an introjection of exogenous authority. Yet her argument against us is simply to invoke her own authority, as she informs us that she has done her coding and interpretation well. We should similarly trust that other cultural sociologists (‘many?’) will ‘include consideration of potentially contradictory evidence.’ We do not say that none do. But this is, after Biernacki’s work, hardly comforting.<sup>14</sup> She seems to imply that if an interpretive sociologist codes, but works *very hard* and is *very smart*, the problems that Biernacki identified may be dismissed. But she does not say whether she thinks Evans, Griswold, Bearman and Stovel were either lazy, or stupid, or both. We think they are neither, and hence believe that there is a serious epistemic problem here.

Our proposals were intended to address this problem: in interpretive analysis performed informally, claims are only stabilized by rhetoric, by appeals to authority, or both. Spillman rejects our analysis here, but her rebuttal is, first, to

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<sup>13</sup> Another rhetorical technique she uses is to take a sober if overly- simplistic example we provide (‘this is a case of Occidentalism’), equating it with farcical one (‘cows inverted apparent symbols of degradation’), and then dismissing the point made using the example.

<sup>14</sup> Nor is it comforting to learn that sociological audiences are more sophisticated than one might think, or that ‘sociologists presenting qualitative evidence are often [?] held to higher standards of literary or popular interest than sociologists presenting quantitative or formal evidence.’ No doubt, this is absolutely true – at least, it would be true *if* the statement is one that *could* be true or false (which we doubt). It seems just as plausible that the reverse is no less true, since ‘often’ (like most vague quantifiers) carries little meaning (since once we have more than a dozen events, it feels safe to say ‘often’). Spillman does not say that ‘sociologists presenting qualitative evidence’ (whatever this might mean) are *generally* held to higher standards, nor how one determines which standards are higher.

cite an authority (Michael Mann) that epistemology is a non-issue. This seems as good a piece of evidence for our claims as one would want, as is her emphasis on the subjective certainty of the interpreter.

Spillman concedes that a formal analysis may be justified in those unfortunate cases where the analysts suffer from a ‘lack of confidence.’ She seems to assume that a subjective feeling of confidence in one’s own arguments is a sign of their merit; she sees as a solution that which we believed was a problem. Take, as an example, her own interpretation of our piece: this is not only that it *exaggerates*, but that it is (at least in part) motivated by *anxiety*.

Imagine, for a moment, that Spillman, suspecting that this was a general trend in the sociology of culture, decided to code up a number of polemical pieces on methodology and epistemology. Two variables of interest would be ADVOCATES FORMALITY? and ANXIETY PRESENT? If Spillman were coding our article, it would get a ‘1’ as opposed to a ‘0’ for the each. With a large number of other texts, she might be able to demonstrate her point – that the interest in formal techniques is fundamentally motivated by anxiety.

Now, *were* this true (that it is anxiety that motivates our approach), it would certainly suggest that our own arguments are more symptoms of an underlying discomfort we have, as opposed to cogent criticisms of current practice that would need to be rebutted in detail. However, her interpretation seems to us to be a rather idiosyncratic and tendentious one. We note that neither Biernacki nor Reed seems to find it necessary to refer to this putative anxiety.

Indeed, we can be more precise. We note that Spillman uses terms connoting fear (including *anxious*, *anxiety*, *afraid*, *fear*, *worry*, *lose/lack/not confiden(t/ce)*) 29 times; when we inspect more carefully, we find that two of these are not used to characterize our position, but her own (‘I worry ...’) and so we subtract them. The other recurrent theme is exaggeration (including *exaggerated*, *overstate*), used 8 times, including *exaggerated fears*. Let us compare this distribution with the interpretations of Reed and Biernacki. Table 2 shows how often these words are used to characterize our position by all three commenters. (Where the terms

Table 2: Use of terms to characterize Lee and Martin

	<i>Spillman</i>	<i>Biernacki</i>	<i>Reed</i>
<i>Afraid</i>	1	0	0
<i>Anxious</i>	6	0	0
<i>Fear</i>	7	0	0
<i>Worry</i>	9(+2)	0(+2)	0
<i>Lose/Not/Lack Confidence</i>	4	0	0
<i>Total Fear Attributions</i>	27	0	0
<i>Exaggerated</i>	6	0	0(+1)
<i>Overstate</i>	2	0	0
<i>Total Exaggerated Attributions</i>	8	0	0

are used, but not to characterize our position, we place them in parentheses.) There is no evidence that this interpretation of our text is shared by the other readers. Of course, perhaps other terms are used than the ones we counted, or a more wholistic ‘reading’ will find attributions of anxiety in the other analyzes. But the burden of proof has shifted.

Perhaps Spillman’s interpretation would have been more reliable had she, for example, used a simple dictionary of emotion terms as a rudimentary way of quantifying the emotion of our piece. Of course – as we argued – this is only a datum, and *requires interpretation*. For example, despite Spillman’s consistent use of words about *anxiety* and *worry*, we know that this is not because her *own* text displays anxiety or worry (she has made it clear that such disabling lack of confidence is incompatible with her own approach). Rather, she is attributing anxiety to the object of her critique, which is a conventional technique of trivializing and dismissing arguments that one does not like, but cannot disprove (see Martin, 2011, pp. 15–18).

We know, to repeat, that Spillman has confidence in her own interpretive procedures. We, however, do not share this confidence. It is the search for ways of adjudicating this interpretive disagreement – ways other than the agonistic deployment of rhetoric – that motivates our approach. Even if our suggestions have inadequacies, we continue to believe it important to develop methods that facilitate that convergence of all honest, informed and critical inquirers that the pragmatists called truth.

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