**Erotetic Contextualism, Data-Generating Procedures, and Sociological Explanations of Social Mobility**[1]

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**ABSTRACT:**

Critics of the erotetic model of explanation question its ability to discriminate significant from spurious explanations. One response to these criticisms has been to impose contextual restrictions on a case-by-case basis. In this paper, I argue that these approaches have overestimated the role of interests at the expense of other contextual aspects characteristic of social scientific explanation. For this reason, I show how procedures of measuring occupational status and social mobility affected different aspects of one explanation Peter Blau and Otis Dudley Duncan offered in their sociological classic, *The American Occupational Structure*. I use the findings from this case study to meet objections to the erotetic model.

The erotetic model of explanation states that an explanation is an answer to a why-question. This liberal notion of an explanation has allowed it a certain notoriety in the philosophy of social science, since it serves as a way of legitimating a number of social-scientific explanatory patterns, among them functional, structural, and intentional-action explanations (Garfinkel 1981; Kincaid 1997; Henderson 1993, 2002; Risjord 1998, 2000), which otherwise might be out of place in theories of explanation designed primarily for the natural sciences. On the other hand, the legitimacy conferred on these explanations by the erotetic model is threatened by criticisms of its ability to judge what should count as an explanation. One response to these criticisms has been to impose contextual restrictions on a case-by-case basis. In this paper, I argue that previous contextualist approaches have overestimated the role of interests at the expense of other methodological conventions that inform social scientific explanation. For this reason, I will show how procedures of measuring occupational status and social mobility affected different aspects of one explanation Peter Blau and Otis Dudley Duncan offered in their sociological classic, *The American Occupational Structure*, described twenty-five years after its publication in 1967 as a "landmark" (Burton and Grufsky 1992) and "one of the mother lodes of social science" (Corcoran 1992).

## I.    The Erotetic Model of Explanation

Advocates of the erotetic model hold that an explanation is an answer to a question "Why P rather than X?" where P is the *topic*, the principle proposition to be explained. However, why-questions with the same topic may have different answers depending on what X stands for, what is commonly called the *foil* in the erotetic literature. For example, "Why is George W. Bush serving as the current president of the US rather than

Gore?" requires a different answer than "Why is George W. Bush serving as the current president of the US rather than McCain?" The first question might be answered by appeal to certain events in the 2000 election, while the second might mention events in the Republican primary. Taken together the topic and the foil form the *contrast class*. The third element of the erotetic model is the *relevance criterion*, which specifies features any acceptable answer must have. Minimally, the model demands that the person asking the why-question must take the topic as true, the foils as false, and the relevance criterion as capable of discriminating the topic from the foil.

## II. Criticisms of the Erotetic Model

Opponents of the erotetic model have argued that its ability to permit such a broad variety of contrast classes and relevance criteria renders its account of explanation too thin. Simply put, not all answers to why-questions appear to be explanations. Along with the host of social-scientific explanations, eroteticists must stomach what Risjord (2000) has called "green cheese" and "red herring" problems.

The green cheese problem arises from the erotetic model's liberal acceptance of explananda. Critics argue that the model admits scenarios in which the topic is true and the foils are not only false but also altogether impossible. Thus an answer to the question "Why is George W. Bush the current president rather than the moon made of green cheese?" would count as an explanation. Such answers would seem to be trivial and uninformative, and these, of course, are not desirable features of an explanation.

Eroteticists have been more concerned with the red herring problem, whose classic statement is found in Kitcher and Salmon (1987), arising from the liberality of relevance criteria. The red herring problem runs as follows: if there are no restrictions on

relevance criteria, then any true proposition A can explain any other true proposition P. For example, a true statement about the alignment of the planets can be an answer to a why-question about George W. Bush being the president if one's relevance criteria are astrologically informed, or the statement "Red herrings have gills" can be an answer if the relevance criterion stipulates something ridiculous like an answer must have three r's and two g's in it.

More generally, these two problems suggest that the erotetic model ignores the fact that certain contrast classes and relevance criteria are not found in questions characteristic of scientific inquiry. An obvious remedy to them is looking at what scientists do. This strategy, however, runs the risk of collapsing into what Henderson (1993) has called 'wimpy laissez-faire contextualism,' in which philosophers merely report on the contrast classes and relevance criteria that arise in different social-scientific pursuits without any aspiration to general criteria that might delineate some of these pursuits as producing better explanations than others. Risjord finds this to be no criticism at all, and has defended a form of laissez-faire contextualism based on the idea that a social scientist's interests determine the contrast classes and relevance criteria of the why-questions she seeks to answer.

While I am sympathetic to Risjord's contextualist gesture, the details of that strategy deserve some scrutiny. First, if contextualism is understood only as a meta-philosophical doctrine that a theory of explanation should be constructed by attending to the epistemic situations in which scientists offer explanations, rather than as an epistemological doctrine stating that the only proprieties of explanations are local ones, then there is no need to disregard Henderson's challenge. Contextualists can aspire for

wide-ranging pronouncements on why-questions, and furthermore, have greater authority

than more *a priori* approaches in virtue of the case studies supporting those claims.

Nothing about starting from a particular context rules out the possibility that certain

features of that context reemerge in a meaningful pattern in many other explanatory

contexts. In light of these contextually grounded generalizations about explanations, one

might then make pronouncements on certain explanations that challenge those

generalizations. In other words, contextualists need not be laissez-faire.

However, Risjord's treatment of interests as the constitutive features of

explanatory contexts seems to force him to embrace precisely the version of

contextualism that precludes the articulation of such broad-ranging epistemological

criteria. If Risjord's account of explanations is correct, then anyone critical of social-

scientific explanatory patterns (for example, explanations appealing to social structures)

simply has different interests than those who find them plausible. Since Risjord offers no

systematic procedure for choosing or assessing explanatory interests in these scenarios, it

is difficult to see how his view discriminates between mere disputes in taste and more

principled debates.

In Risjord's defense, certain examples motivate the idea that no such procedure is

to be had. For example, what reasons could be offered to one anthropologist for why she

ought to be interested in studying hominid skeletons rather than religious rituals in

Mediterranean Africa? Both pursuits seem justified in their own right. On the other hand,

one of the heroes of this paper, Otis Dudley Duncan, came up with a socioeconomic

index that provided good reasons for why sociologists studying occupational status ought

to be more interested in the incomes and educations of various occupations over and

above the attitudes people harbor towards those occupations. This suggests that, at a certain level of analysis, there appear to be norms for choosing interests about why-questions.

**III. Reconsidering Contextualism**

But how can the eroteticist discover these norms? The answer is not to discourage contextualism, as Henderson suggests, but rather to engage in a more thoroughgoing contextualism by shifting attention from *a priori* argumentation to closer analysis of actual social-scientific practices. A more detailed contextual analysis than Risjord's, one that pays attention to things other than interests—namely the practices of measurement, data analysis, hypothesizing, and observation—can motivate, confirm, falsify, or refine philosophical hypotheses about what many scientists deem relevant in formulating explanations. I will call this approach *hands-on contextualism* in order to distinguish it from Risjord's laissez-faire variety. I will refer to the aforementioned practices of measurement, data analysis, and the like as *methodological conventions*. To this end, I will outline how a hands-on contextualist strategy would resolve the green cheese and red herring problems by considering how Blau and Duncan narrowed their contrast class and relevance criteria in their path breaking 1967 study of occupational status.

Before doing this, it is useful to explicate the concept of methodological conventions in greater detail. Within the hands-on contextualist project, a methodological convention is a frequently occurring activity that is productive in scientific practice and more specifically, that influences scientific explanation. Hacking (1992) provides a useful inventory of many of these conventions (though he does not use this locution). Among the conventions he lists are "ideas" such as questions, systematic theories, topical

hypotheses, and models, as well as "marks and manipulations of marks" such as data, data assessment, data reduction, data analysis, and data interpretation. He also mentions "things" such as tools and detectors that are more germane to the laboratory sciences he is concerned with than the social sciences examined here. Note that while I described methodological conventions as activities and Hacking's list does not expressly state them as such, translation is easy enough, e.g., asking questions; modeling; gathering, assessing, reducing data; using tools; etc.

Hands-on contextualists seek to identify how such conventions productively influence explanations through examination of historical case studies. To arrive at the desired broad-ranging epistemological criteria, hands-on contextualists are well advised to follow Thagard's (1988) schema for the 'historical philosophy of science'. This consists of selecting cases deemed as significant contributions to the growth of the discipline and assuming that in these exemplary cases the actual methods of the scientists were good approximations of what the discipline's methods ought to be. Then, the hands-on contextualist incorporates these conventions into a generalized erotetic model in order to apply it to further case studies for additional assessment and refinement. (Thagard does not apply this specifically to the erotetic model). I will focus on the class of methodological conventions Hacking calls marks and the manipulation of marks, which I will call data-generating procedures. One reason for this focus is since neither the erotetic literature nor the philosophy of social science (erotetic or otherwise) has examined these procedures in great detail, bringing them to bear on social scientific explanation helps to vindicate hands-on contextualism. Let us now turn to the case study.

*A. Blau and Duncan's Data-Generating Procedure*

For Blau and Duncan, the data-generating procedures in question involved measurement and statistical analysis of occupational status and social mobility. Blau and Duncan generated data on occupational status by defining occupational status as a function of the median income and median education of each of the 17 occupational groups they examined. These 17 groups were then hierarchically ranked according to the sum of these median values, allowing Blau and Duncan to use the common sociological parlance of socioeconomic *strata*. For example, farmers had the lowest sum of median income and education and thus were the lowest occupational stratum. (It should be noted that Blau and Duncan do not seem to think this hierarchy *ought* to obtain, merely that it accurately describes certain divisions in the society).

Blau and Duncan then generated social mobility data by characterizing it as movement from one stratum to another over time. This was done in two ways—intergenerationally, by measuring a respondent's current (1962) occupational status as a mathematical function of his father's occupational status, and intragenerationally, by measuring a respondent's current occupational status as a function of the status of his first occupation. It is important to note that the data-generating procedure for social mobility is parasitic upon the data-generating procedure for occupational status.

They took these procedures to be good measures of occupational status and social mobility not merely because it satisfied their interests, but because it was more effective than its predecessor in addressing other data-related problems, as well as some theoretical problems. This occasions two points about the hands-on contextualist approach. First, its commitment to the historical philosophy of science encourages deriving epistemic norms

(e.g., the reliability of a data-generating procedure) by comparing a successful scientific practice to its immediate predecessor. Second, since methodological conventions tend to be assessed according to their ability to solve multiple problems involving other conventions, the character of their epistemic normativity will be holistic in character, meaning that the propriety of one methodological convention depends on its relation to, and the success of, other conventions. Thus, despite the focus of this paper being on data-generating procedures, Hacking's "ideas"—notably hypotheses, theories, and of course, (why-) questions—will also play a prominent role in it. To synthesize these two ideas into a general dictum, the norms of a convention are a function of it being able to cohere with other conventions better than its predecessor. It is these norms that provide a promising alternative to interests in the laissez-faire contextualist's approach.

The foremost work on occupational status prior to Duncan's work in the early sixties was North and Hatt's (1947) study at the National Opinion Research Center (NORC). The NORC asked respondents to rank the standing of over sixty occupations on a scale from 1 to 5—1 being excellent standing, 5 being poor. Occupational ratings were measured as a weighted average score. The problem with the NORC study was that there were far more than sixty occupations to consider. Six years prior to the publication of *The American Occupational Structure,* Duncan (1961) addressed this problem in an article entitled, "A Socioeconomic Index for All Occupations." In contrast to North and Hatt's prestige score, Duncan constructed the socioeconomic index that would serve as the data-generating procedure for occupational status in *The American Occupational Structure*, i.e., that measured occupational status as the sum of median income and median education of a given occupation.

In order to establish the superiority of his index, Duncan argued that it supplemented other methodological conventions—specifically theorizing, replicating data, and producing new data—better than the NORC scores. Theoretically, Duncan could provide a plausible account of why the two independent variables conspired to create occupational prestige—as he succinctly puts it, "A man qualifies himself for occupational life by obtaining an education; as a consequence of pursuing his occupation, he obtains income. Occupation, therefore, is the intervening activity linking education to income" (Duncan 1961: pp.116-117). Second, the socioeconomic index replicated the results of the NORC study with 90% accuracy. Finally, the index facilitated the generation of new data. This was because the median income and median education of an occupation were two variables easily obtained from census data while further NORC-style empirical research seemed implausible since ranking the actual number of occupations—Duncan mentions the 270 occupational categories mentioned in the 1950 Census—would seem to be taxing the respondents' ability to distinguish the differences between occupations. As we shall see, these arguments also give the index more *explanatory* power than the NORC scores.

This suggests that more generally, one data-generating procedure is superior to another if it has greater coherence with theoretical claims as well as greater ability to replicate old data and to generate new data. Thus, if data-generating procedures illuminate the erotetic model, then they provide a more robust normative basis for the model than the interests posited by laissez-faire contextualism. Let us now examine how data-generating procedures illuminate the model, specifically how they resolve the green cheese and red herring problems.

*B. The Green Cheese Problem*

To reiterate, the green cheese problem challenges eroteticists to offer principles for narrowing the contrast class. Here the role of data-generating procedures is quite pronounced. Since the very structure of the erotetic model requires that the person asking the why-question take the topic as true, it was not surprising to find that social scientists often confirm the topics of their why-questions with data generated by reliable procedures. Thus one of the topics of the many why-questions Blau and Duncan raise in *The American Occupational Structure* is that African-Americans are less likely to experience upward social mobility than white Americans. It is not enough that this is perhaps consistent with our folk wisdom about social stratification in the United States; rather the force of this assertion stems from Blau and Duncan's rendering of terms like "social mobility" operational for empirical analysis by means of the procedures outlined above. With these operational definitions of occupational status and social mobility in hand, Blau and Duncan could measure and compare social mobility of whites and African-Americans.

Resolution of the green cheese problem requires a contextualist strategy for identifying not just the topic, but also the foils. The formal strictures of the erotetic model require that the foils be taken as false propositions. But solving the green cheese problem demands that these not be any arbitrary false propositions. Examining Blau and Duncan's work suggested further restrictions on the erotetic model to solve that problem. In their research, they first studied whites' social mobility and found it to be a function of two variables: the respondent's education and the socioeconomic status of his father's occupation. Blau and Duncan provisionally began with the hypothesis that African-

American social mobility was analogous to white social mobility, but revised it as the data bore out certain differences. Whites coming from the lowest socioeconomic origins had the highest chance of upward mobility. From this, Blau and Duncan initially conjectured that African-Americans should, on average, be more likely to experience upward social mobility than whites since a larger portion of the African-American community was concentrated in these lower socioeconomic strata. However, the data confirmed quite the opposite—African-Americans were more likely to experience downward mobility and less likely to experience upward mobility than whites. From this we get the following why-question: "Why do African-Americans have a lesser (rather than a greater) chance of upward mobility than white Americans?"

This suggests three complementary strategies for identifying the foils. First, at a semantic level, the erotetic model should be refined to reflect the fact that topics and foils tend to be about the same thing—what I will call a *relevant constant*—if there is a meaningful contrast between them. For example, in Blau and Duncan's why-question, both topic and foil refer to African-Americans, chance of upward social mobility, and white Americans. While the scope of this paper will not permit the semantic analysis required to explicate the role of relevant constants in why-questions in detail, the basic point is that in a significant why-question, topic and foils will share referents.

But a dilemma arises if one looks to identify relevant constants as the sole means for solving the green cheese problem. On the one hand, for there to be any contrast between topic and foil, there must be at least one variable term—otherwise topic and foil would be identical, leading to nonsensical why-questions such as "Why is George W. Bush president rather than George W. Bush president?" On the other hand, it must vary

in specific ways to avoid green cheese problems. For one could ask, "Why do African-Americans have a greater (rather than lesser) chance of upward mobility than they do of turning into green cheese?" Here the relevant constant would be African-Americans, but the contrast class is still too open-ended.

This is where the second strategy for identifying foils comes into play. Foils should be confirmable by the same data-generating procedure that confirmed the topic. Before performing their study, the foil, that African-Americans would have a higher chance of upward mobility than whites, and the topic, that they would have a lesser chance, were both confirmable by the same data-generating procedure. However, the spontaneous transformation of African-Americans into mint-colored dairy products was not. In general, data-generating procedures have relatively few possible outcomes. This is reflected in the fact that the variable term in the why-question could only take "greater" or "lesser" (and presumably "equal to") as its values, since this is all the data-generating procedure would license. Of course, since foils must be false, the erotetic model requires that they be disconfirmed by that procedure.

A possible objection is that this requirement is too stringent and prohibits the triangulation of data generated by different procedures. However, it only needs to be possible, not actual, that the data-generating procedure confirms both the topic and the foil. Thus findings from alternative (actual) procedures can still have a bearing on a why-question if those findings are confirmable by the current procedure. For example, while testimonies of African-Americans who achieved upward mobility could play a role in the same why-question that Blau and Duncan posed, testimonies of exiled Tibetan monks could not. More importantly, triangulation usually does not occur in framing or asking

why-questions so much as in answering them. For example, the aforementioned testimonies of African-Americans might play a part in answering Blau and Duncan's why-question while not contributing at all to the formulation of the topic or the foil. Indeed, Blau and Duncan mention "impressionistic observation" (p. 212) as a useful part of their answer to the why-question under discussion.

However, despite the fact that typically only a small variety of propositions are confirmable by a single data-generating procedure, even amongst this small class, only a few provide relevant contrasts to the topic. Blau and Duncan posed their why-question because the topic was *unexpectedly* confirmed by the data-generating procedure. The foil, that African-Americans have greater chances of upward mobility than whites, was what they expected. Thus expectations play a crucial role in identifying which propositions will be included in the contrast class. Blau and Duncan's expectation was informed by a provisionally held hypothesis that for all Americans, regardless of race, the chance of upward mobility is inversely proportional to father's socioeconomic status. This introduces the third strategy for identifying foils, namely that the most relevant foils are the expected results or implications of a plausible hypothesis. In this manner, it is the combination of data-generating procedures and hypothesizing, another methodological convention, that provides relevant foils.

It is important to emphasize that the claims that constitute a hypothesis and statements about its implications are not identical. At the most general level, were the data-generating procedure to confirm the latter, the former would explain it. Statements about hypotheses' implications, rather than the hypotheses themselves, tend to serve as foils in a why-question. Much of this can be explained by the dynamics of a scientific

community, for there tends to be greater consensus regarding the reliability of data-generating procedures, such as the statistical methods Blau and Duncan employed, than about the falsity of rival hypotheses, for example, about race and social stratification. Furthermore, if there *were* widespread agreement that the alternative hypotheses being entertained were false, then the why-question would not be of great interest to the intended audience. Thus, whereas the propriety of the data-generating procedure often rests on the research community's agreement on a substantive justification for its use (as well as the range of its use), the relevance of the hypotheses informing the foils rests on a healthy amount of communal disagreement, on competing justifications for competing claims.

A possible objection is that this engenders explanatory conservatism, since the topic of a why-question rests on communal agreement about data-generating procedures. Another, and I think, more historically accurate, moral to draw is that there are a lot of small and diverse communities in the social sciences. The superiority of a data-generating procedure and the soundness of topics founded on it may not have universal agreement across the field, but this only means that why-questions with topics, e.g., affirming the *sui generis* reality of *conscience collective*, might be accepted without reservation at the local Durkheim conference, but would require further justification in mixed company. A discussion about the nature of justification amongst social scientists with different theoretical commitments would lead us too far astray, but it is sufficient to note that since people have been persuaded to change their allegiances, it does not appear to be impossible *prima facie*.

To summarize the results so far, any eroteticist seeking to identify the contrast class of a particular scientific why-question should consider the following points. First, topics are data confirmed by reliable data-generating procedures. Second, these procedures have a limited set of possible outcomes. Since foils should be limited to propositions that are confirmable by these outcomes, most green-cheese problems are ruled out. Furthermore, of these propositions, the most interesting foils are claims about the implications of alternative hypotheses possessing some sort of common referent, a relevant constant, with the topic.

*C. The Red Herring Problem*

Similarly, the contextualist response to the red herring problem is to identify how scientists exclude certain true statements as answers to certain why-questions. Risjord's strategy makes good sense here for ruling out the absurd cases—scientists talking about occupational status simply are not *interested* in the fact that red herrings have gills. However, his work does not offer a principled way of choosing one's interests in particular relevance criteria. Furthermore, the more pressing red herring problems arise precisely when a fact that could, at first blush, be relevant is not mentioned in the explanation.

Such a problem presented itself in the case study. Blau and Duncan offered two complementary answers to the question "Why are African-Americans less (rather than more) likely than whites to experience upward mobility?" They argued: (1) African-Americans tended to have less education, and (2) well-educated African-Americans suffer greater discrimination than less educated African-Americans. The latter explanation is at odds with Westie's (1952) work on white attitudes towards African-

Americans with different occupational statuses. Westie's research indicated that whites, particularly those of higher socioeconomic status, regard African-Americans in high status occupations with greater prestige than those in lower status occupations. But if Westie's work were legitimate, then why, as Blau and Duncan argued, would highly educated African-Americans suffer greater discrimination?

With respect to the red herring problem, the question is why Blau and Duncan did not regard Westie's work as relevant in the construction of their explanation. The most plausible answer appeared to be that Westie's research relied upon the NORC data. The more general principle is that the relevance of an answer is directly proportional to the reliability and soundness of the data-generating procedure it is founded on. Central to this claim is the fact that the arguments raised against the NORC's data-generating procedure provided the basis for the relevance criteria by which Blau and Duncan selected their own explanation over Westie's. The index's role in theorizing, replicating data, and generating new data corresponded to three strategies that arose in Blau and Duncan's explanation: the positing of causal mechanisms, the explaining away of competing explanations, and the isolation of variables in abductive or hypothesis-forming inferences, respectively.

As discussed earlier, the theoretical consideration that prompted Duncan to favor his index over the NORC score as a reliable data-generating procedure was that he could provide a plausible account of why occupational status was a function of median income and education. Specifically, he showed why income and education were the *causes* of occupational status. The ability to talk of education as part of the causal mechanism enabling individuals to obtain occupational status factors into the first explanation that Blau and Duncan offer to the why-question posed earlier. Education was a defining

variable for occupational status, which in turn was a defining variable for occupational mobility. For all Americans, the likelihood of a significantly upward move increased with education. In 1962, the ratio of white men without any high school experience to those with some college experience was about 1:1; for African-Americans, it was 5:1. Thus Blau and Duncan could appeal to discrepancies in education as a contributing cause of the latter's lesser chances at upward mobility. In terms of the erotetic model, then, it is sufficient to say that the sentence "African-Americans tend to have fewer years of education" is an answer to the why-question posed above about their lesser social mobility, and that the relevance criteria seems to be statistical and causal in nature.

In contrast, the NORC's data-generating procedure posited the avowed attitudes of individuals as the only cause of occupational status. Further analysis would have been required if someone like Westie sought to employ education in his explanation of racial discrepancies in occupational status. While not impossible, the explanation would have been less economical than Blau and Duncan's, since it would have had to posit a relation between the education of a person in a given occupation and the avowed attitudes of those conferring status upon that occupation, which is bypassed altogether in Blau and Duncan's explanation.

Thus, the case study suggests that an explanation that can identify a plausible causal mechanism will, *ceteris paribus*, be more relevant than one that does not. The degree to which this feature of relevance criteria commits the scientist to specifically *causal* forms of explanation, rather than intentional-action, functional, or structural ones, is an open question. Note that a more pluralistic account is consistent with the intuition that different theories posit intentions, functions, and structures.

That being said, this *ceteris paribus* assumption is far from secure in many social scientific explanations. This is why replication of data plays an important role in relevance criteria. As mentioned earlier, Duncan's index replicated the NORC study with 90% accuracy. Thus, the index worked just as well as the NORC scores *and* had a more robust causal mechanism to explain why it worked. The significance of this for the explanation is that it largely allowed Blau and Duncan to regard the avowed attitudes of respondents as *irrelevant* to any explanation of occupational status. More metaphorically, it serves as a means of "explaining away," "countering," or "diffusing" Westie's claims, since in principle, all the explanatory power of the NORC scores could be found in the index. Thus, replicability stands as evidence of the *ceteris paribus* assumption mentioned in the discussion of causal mechanisms above.

Finally, the same virtues that facilitated Duncan's using the index to generate new data also permitted Blau and him to isolate relevant variables when making abductive inferences. This was particularly important when Blau and Duncan ventured further than their first explanation by asking if *all* of the discrepancy in occupational mobility could be attributed to discrepancies in education. The answer to that was a resounding "no." Even with comparable years of schooling whites had a much higher likelihood of these upward moves than African-Americans. In addition to the decreased *likelihood* of an upward move, the *occupational status* was considerably lower for blacks than whites with comparable years of schooling. Indeed, "the difference between mean occupational status of whites and of nonwhites *increases* with higher educational levels," such that "the more education a nonwhite man acquires the further does his occupational status fall behind that of whites with comparable education" (Blau and Duncan 1967: 210-211).

Both the ability to isolate these variables and the ability to generate new data rest on the index's ability to operationalize terms. Unlike attitudes, education and income are easily quantifiable entities; this facilitates talk of greater and lesser social mobility. This is important for the second explanation, since the fact that education could not tell the whole story licensed the additional abductive inference that discrimination was the best answer to the why-question, as evidenced by the following claim in *The American Occupational Structure*:

> To be sure, whether observed differences in occupational achievement are results of discrimination, ability, or motivation can only be inferred from the data at hand. These inferences can be rendered more plausible, however, by taking into account intervening variables, such as first job and, particularly education, and by controlling the influence of correlated factors, such as father's occupational status. (207-208)

Blau and Duncan go on to offer a characterization of the college-educated African-American to help explain why education cannot tell the whole story. Since this segment of the population tended to come from significantly more modest socioeconomic origins than whites with comparable education, Blau and Duncan conjectured that they had to overcome more obstacles, and thus were probably a more selective and motivated group than their white counterparts. Yet, they did not achieve the same occupational levels as their white counterparts. While Blau and Duncan entertain the possibility that this stems from their lower socioeconomic origins, they note that this "cannot account for their lesser chances of achieving upward mobility compared to whites, because it provides *more* room above their origins into which to move than is the case for whites. It is very probable that discrimination plays an important role here" (Blau and Duncan 1967: p. 211).

Since the NORC did not generate occupational status data by appeal to education and income, explanations relying on this data, such as Westie's, could not isolate and control for the variables that Blau and Duncan employed in their explanation. Thus, the inferences leading them to identify discrimination as an answer to their why-question would not be licensed by Westie's work. Most notably, Duncan's index enabled them to target a discrepancy between social outcomes and avowed attitudes that Westie's work could not.

Thus, the considerations that factor into the reliability of a data-generating procedure have coordinate roles in explanations. Generally, the relevance of an answer to a why-question is determined by such considerations as its appeal to a causal mechanism, its ability to diffuse competing explanations, and the ease with which its variables can be isolated. These have a close relation to the data-generating procedure's confluence with theorizing, replicating data, and generating new data, respectively. Thus, while the role of data-generating procedures is far less direct in solving the red herring problem than in the green cheese problem, it nevertheless provides an occasion to highlight the commitment to holism in the hands-on contextualist approach. Indeed, holism appears to be a consequence of data-generating procedures' indirect role in the red herring problems.

## IV.    Conclusion

In summary, advocates of contextual erotetic approaches must acknowledge that many methodological conventions that have been ignored play a pivotal role in the asking and answering of why-questions. While I have focused on data-generating procedures, I have incorporated several other aspects of scientific practice along the way. While data-generating procedures indicate the topic to be explained, their relationship to hypotheses

is crucial in restricting the foils with which it is to be contrasted. Similarly data-generating procedures conspire with theories and hypotheses to structure relevance criteria. More concretely, topics are confirmable by a reliable data-generating procedure; foils are implications of a hypothesis that the procedure could have confirmed; relevant answers are informed by theoretical considerations such as the identification of causal mechanisms and the explaining away of competitors, as well as the ease of isolating variables in hypothesis-forming inferences. In these respects, hands-on contextual erotetic analysis depends upon factors that have been explicated by other studies of abduction and theory selection. Thus, the contextual approach that Henderson criticizes for being radically plastic, local, contingent, and "wimpy" turn out, on closer inspection, to be epistemically robust in exactly the ways he would take seriously. Perhaps non-contextualists will continue to object that these conclusions merely echo the idiosyncrasies of a thirty-five year-old sociological text. But this is no criticism at all; rather it reminds us of the hands-on contextualist's last obligation as a historical philosopher of science: to continue to look at what scientists do in order to refine the erotetic model.

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