• *ABSTRACT*

The resource estimates of M. King Hubbert, and the method he used to generate these estimates, are described. When Hubbert made his first estimate it reflected the consensus of opinion within the industry. The conclusions he drew about the future of the US oil industry, however, contradicted the conventional wisdom within both industry and government. His estimates were criticized and rejected, and a number of markedly higher estimates soon appeared. In 1974, the political economy of the oil industry changed, government and industry estimates fell, and a consensus of opinion returned. Since Hubbert's methods and estimates remained constant throughout the period 1956–82, changes in scientific practice cannot explain the historical shifts in the treatment of his estimates as 'valid', 'invalid', and eventually 'valid' again. Through an examination of the scientific controversies involving Hubbert's estimation technique and its results, it is argued that the 'validity' of resource estimates is socially constructed through an attributional process tied to the political economy of the oil industry.

The Social Construction of Validity in Estimates of US Crude Oil Reserves

Gary Bowden

How much oil is there in the ground? Though geological, geophysical and engineering information can be used to arrive at a scientifically acceptable answer to that question, estimates frequently contradict one another.[1] How do scientists arrive at a consensually accepted 'true' value? This article examines the socially constructed nature of scientific opinion about the size of recoverable crude oil resources[2] and, in doing so, links previous work on the social construction of validity[3] with the 'interests model' of explanation.[4]

The analysis is divided into four sections. The first explicates the concept of interest and develops the notion of validity as an attributional status. The second, third and fourth sections describe a

Social Studies of Science (SAGE, London, Beverly Hills and New Delhi), Vol. 15 (1985), 207–40

case study which illustrates links between the two. The second section describes the claims made and methods used, by M. King Hubbert to estimate the magnitude of petroleum resources in the United States, and shows that these have remained unchanged since 1956. The third section describes the social constructions of validity that have accompanied Hubbert's estimates at particular points in time.[5] Taken together, these two sections make it possible to correlate changes in the interpretation of Hubbert's claims with changes in the political economy of the oil industry, while controlling for the content of the knowledge claim. The fourth section draws upon concepts developed in the first section to explain both how and why the changes in attributional status occurred.

Structural Interests and the Attribution of Validity

Many current explanations in the sociology of scientific knowledge use models of two basic types: the 'interests model' and the 'attribution model'. The interests model uses the critical Marxism of Habermas[6] to establish the epistemological basis of an instrumental perspective designed to explain why communities of individuals share a belief in a particular piece of knowledge, and why such shared knowledge changes through time.[7] The attribution model draws upon the insights of ethnomethodology and post-Wittgensteinian philosophy in order to explain how objectified knowledge is constituted.[8] Most research within the attributional framework has focused upon the constitution of knowledge in localized settings (that is, laboratories). Thus, the notion of knowledge central to the interests model (that is, knowledge as a belief shared by a community of individuals) appears in the attributional model only as the aggregate result of a series of local acts.

This paper links the attribution model with the interests model by using organizational interests to explain the timing and consistency of the transitions in locally attributed status that have accompanied Hubbert's constant claims at different points in time. According to Barnes,

> interests inspire the construction of knowledge out of available cultural resources in ways which are specific to particular times and situations and their overall social and cultural contexts. As for the relationship of interests and social structure, it is accepted that some interests are indeed structurally generated and ultimately

> attributable to social-structural categories; and individualistic criticisms of structural explanations are held to be misconceived. But no general theory which sets even particular social interests into direct correspondence with classes or other categories is assumed or advocated.[9]

My analysis imputes historically contexted interests to social structural actors (that is, complex organizations). Though there is no necessary correspondence between oil and gas companies as organizational actors and profit maximization as an organizational interest, within the US, where oil companies are private corporations, they have usually gone together. These interests are 'enacted' through relations with a network of other organizations.[10] Thus changes in the relationships within an interorganizational network may result in a realignment of the best means for achieving a constant organizational interest.

While I am indebted to Barnes for the theoretical conception of interests, the focus on complex organizations distinguishes my usage from that in previous research. First, organizations are empirical, institutionalized bodies with a much more concrete existence than the social classes to which structural interests have often been imputed.[11] Secondly, viewing interests as enacted gives them a firmer grounding. They are not theoretical concepts imputed by the analyst, but members' categories. Enacted interests (that is, organizational goals) are created and recognized by and structure the behaviour of members of an organization. Finally, individuals are employed within organizations, and organizations therefore possess power over them. This suggests the possibility of a close coupling between the interests of the organization and the scientific discourse and practice of its employees.

As Barnes has noted,[12] the interests model has little to say about how knowledge is constituted. The strongest discussion of such constitutive problems is found in Brannigan's attributional model of discovery.[13] Brannigan suggests that the attribution of certain statuses to four variables (substantive possibility, motivational frame of reference, precedence, and local validity) provide the necessary and sufficient conditions for constituting an event as a discovery. By extension, an attributional model of validity would specify the variables and associated statuses that provide the necessary and sufficient conditions for constituting an event as valid within the local context.

Collins's work on gravity waves[14] can be read as an illustration of the working of one such variable. According to Collins, the only way for a scientist to demonstrate that he has the knowledge necessary to replicate an experiment is to make a replica that is counted as 'working' by the scientific community. However, the problem posed within the gravity wave field (as of 1972) was that what counted as a 'working gravity wave detector' was also a contentious matter. Thus negotiations about which set of experiments in the field would count as competent experiments were, in effect, negotiations about the character of the phenomenon. The central theoretical problem in this study (although Collins does not label it as such) is the phenomenological problem of identity: what are the grounds for treating two things as equivalent — that is, for identifying one as a replication of the other? This suggests that one of the variables underlying the attribution of local validity is a conception of the identity of a phenomenon — and, hence, the grounds for establishing equivalence. If this is true, then changes in the grounds used to constitute estimates as equivalent should result in changes in the validity attributed to the estimates. Thus a primary feature of the attributional model is its ability to account for the changes in objectified status that have accompanied constant claims through time.[15]

To summarize, we are faced with three connected problems: (1) How is the objectified status of validity associated with a knowledge claim locally constituted? (2) Why do actors in quasi-independent local contexts arrive at similar conceptions of the validity of a knowledge claim? (3) Why do these shared conceptions change? The following case study illustrates a theoretical linkage of the attribution and interests models designed to connect these problems within a single explanatory framework.

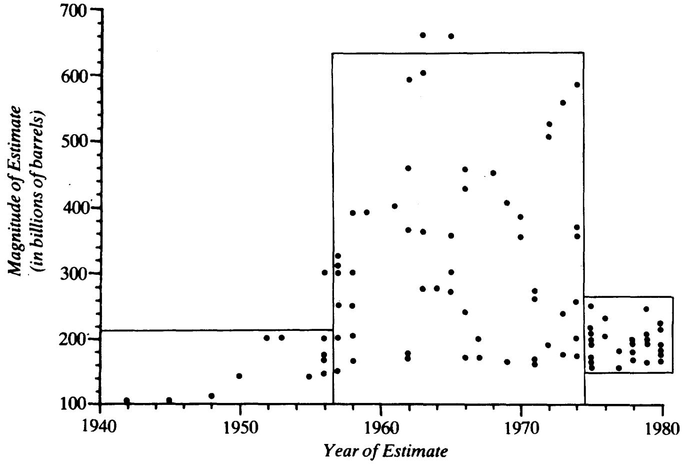
The Crude Oil Estimates of M. King Hubbert

Hubbert's scientific work has been wide-ranging.[16] The topics he has researched include: geophysical exploration for oil, gas and various minerals; petroleum geology and engineering; structural geology and the physics of earth deformation; and the physics of underground fluids, including the motion of groundwater, the entrapment of petroleum under hydrodynamic conditions and fluid behaviour in petroleum reservoir engineering.[17] In addition, he has produced a series of assessments of US and world energy resources.

It is this latter aspect of Hubbert's scientific work which concerns us here. The earliest of these assessments appeared in a presentation to the American Association for the Advancement of Science in 1948. In 1977 he received the prestigious Rockefeller Public Service Award for his early and persistent attempts to bring the 'energy crisis' to public attention.[18]

As can be seen from Figure 1, the estimates of ultimately recoverable US crude oil resources fall into three historical groupings. The overall pattern is defined by three characteristics: (1) the mean magnitude of the estimates within a period; (2) the variation among the estimates within a period; and (3) the timing of the transitions between periods. In general, the estimates rise after 1956 and fall after 1974. Repeated estimates made by the same individual show the same pattern. Hubbert is the one exception to this rule: his estimates have remained constant.[19] Thus if the magnitude of an estimate is to be treated as a function of interests, then Hubbert's estimates are the product of interests which differ from those that account for the estimates of others. Here, however, our concern is not with the interests that led Hubbert to produce his claims but, rather, with those of the individuals who evaluated his claims.[20] It will be argued that the magnitude of Hubbert's estimate

FIGURE 1
Historical Pattern among Estimates of
Ultimately Recoverable, Conventional US Crude Oil Resources



Source: Revised from Bowden (1982) op. cit. note 19.

was constructed as invalid when it contradicted the interests of the organizations employing other estimators, and was constructed as valid when it could be used as a resource in support of those organizational interests.

Before 1956, estimates of ultimately recoverable US crude oil resources were generally made through volumetric analysis. This method involves the following steps: (1) gathering information on the geographical distribution of basins containing oil; (2) obtaining estimates of how much oil is contained per unit volume in the sediments of the better known areas (that is, well explored basins); (3) adopting the assumption that comparable amounts per unit volume will be found in geologically similar basins; and (4) calculating the expected amount of oil in the unexplored basin by multiplying the volume of unexplored sediment by the estimate of oil per unit volume determined from the better known basins. Despite the existence of interpretive flexibility in both the richness factor[21] and the constitution of geologically similar basins,[22] these estimates showed a high degree of reliability before 1956 (see Figure 1).

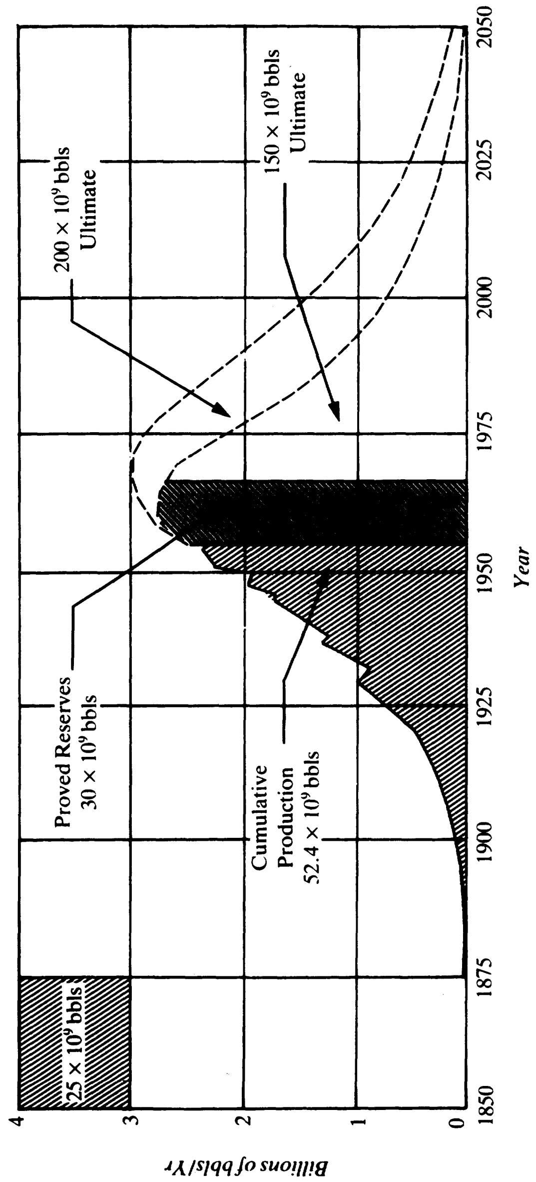
In 1956, Hubbert applied the theoretical insights of Hewett's examination of production statistics from the major mining districts in Europe to the question of oil and gas resource appraisal.[23] According to Hubbert

> Although not all of Hewett's criteria are applicable to the production of fossil fuels . . . the fundamental principle is applicable; namely, that like the metals, the exploitation of the fossil fuels in any given region must begin at zero, undergo a period of more or less continuous increase, reach a culmination and then decline.[24]

Such a theory defines the general shape of the expected curve, but does nothing to predict the area under it. Thus, by itself, Hewett's method cannot be used to estimate ultimately recoverable petroleum resources.

To remedy this situation, Hubbert reviewed the published estimates and made extensive inquiries among respected exploration geologists. From this research he concluded that the ultimately recoverable crude oil resource (for the lower 48 states and their adjacent continental shelves) would be between 150 and 200 billion barrels (bbl). Hubbert used these figures to construct the industry life cycle shown in Figure 2. On the basis of the life cycle curves he predicted that US crude oil production would peak sometime between 1966 and 1971.

FIGURE 2
Two Complete Cycles of US Crude Oil Production
Based Upon Ultimate Production of 150 and 200 Billion Barrels.



Source: Hubbert, op. cit. note 23, Figure 21.

In 1957, the consensus that Hubbert had used to justify the selection of specific life cycle curves vanished (see Figure 1). Hubbert turned to the statistics on annual production and proven reserves for objective data on which to base his analysis. From these data Hubbert took proved reserves (Q_r), and calculated cumulative production (Q_p) and cumulative proved discoveries (cumulative production plus proved reserves), Q_d. The theoretical shape of these curves and their mathematical fit (as of 1962) are shown in Figure 3. The rates of increase in cumulative proved discoveries, cumulative production and proved reserves are obtained by taking the time derivative of the equation used to compute cumulative proved discoveries. Thus

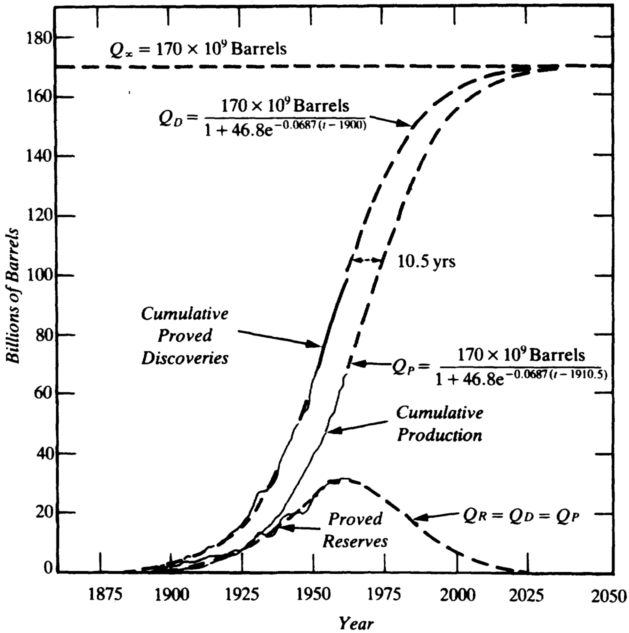
$$\frac{dQ_d}{dt} = \frac{dQ_p}{dt} + \frac{dQ_r}{dt}$$

These derivative curves and their empirical fit (as of 1962) are shown in Figure 4.

In essence, Hubbert's 1962 estimate[25] fitted empirical data to the theoretical curve he described in 1956, and to other theoretically related curves, in order to determine the point of inflection (that is, the year of peak annual production). Given this information, and the assumption that cumulative production before the point of inflection will equal one-half the ultimate production, the magnitude of ultimately recoverable resources can be calculated. From the curves shown in Figures 3 and 4 Hubbert concluded: (1) the rate of proved discoveries passed its peak about 1957; (2) the peak in proved reserves would occur in 1962; (3) the production peak would occur in the late 1960s; and (4) the ultimate production of the continental US and its associated offshore areas would be 170 billion barrels. Hubbert also estimated ultimate recovery through reference to expected production from large fields. This estimate, based upon geological information, yielded an estimate of approximately 170 billion barrels and, hence, corroborated the estimate derived from production statistics.[26]

Hubbert's later publications[27] focused upon three things: (1) updating his estimate as new production and discovery data became available; (2) rebutting the methodological validity of estimation techniques that yielded substantially greater estimates; and (3) developing a second method of estimating ultimately recoverable resources. In each of these publications, using either the method of

FIGURE 3
Cumulative Proved Discoveries, Cumulative Production, and Proved Reserves of US Crude Oil to End of 1961, and Mathematical Equations for Best-Fit Logistic Curves

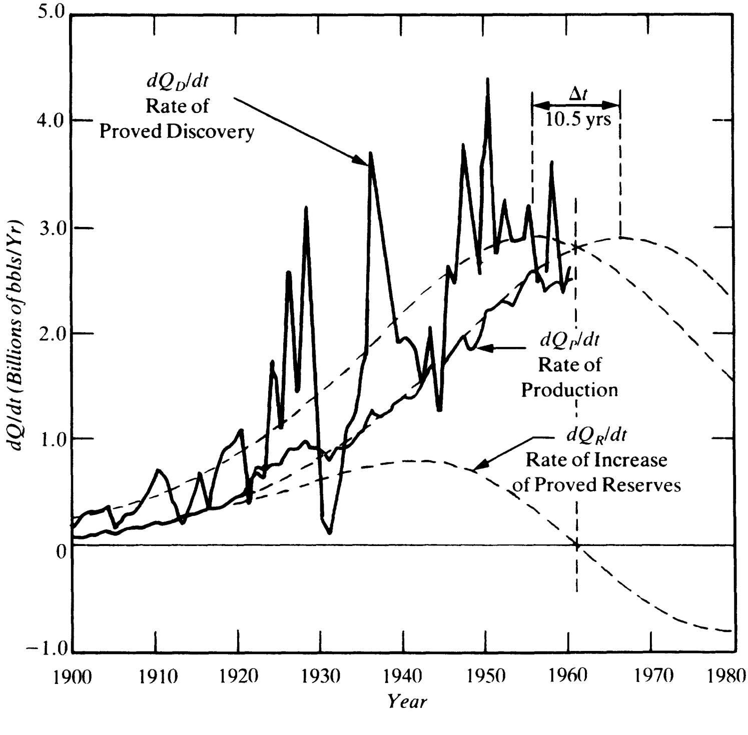


Source: Hubbert, op. cit. note 25, Figure 27.

production statistics described above, the method of discovery index described below, or both, Hubbert estimated that the ultimate production of the continental US and its associated offshore areas would be between 165 and 175 billion barrels. He still holds that view today.

In 1962, an alternative estimation technique was developed by Zapp.[28] This technique, used by the US Geological Survey between 1962 and 1974, gave a series of very large estimates.[29] Zapp, in an attempt to remove flexibility from the estimation process, argued that exploratory wells with an average density of one well per each two square miles must be drilled to basement rock, or 20,000 feet, in

FIGURE 4
Rates of Proved Discovery and of Production of US Crude Oil to End of 1961, with Dashed Curves of Mathematical Derivatives



Source: Hubbert, op. cit. note 25, Figure 28.

all the potentially petroleum-bearing basins of the United States before the true extent of the existing resource can be known. He estimated that this would take five billion feet of exploratory drilling. By 1961, cumulative drilling footage amounted to 1.1 billion feet and 130 billion barrels of crude oil had been discovered. Thus, on an average, 118 barrels were discovered per foot of exploratory drilling. Assuming the same rate of return, 590 billion barrels of crude would be found by the time exploration was completed.

Hubbert has repeatedly criticized the Zapp method for its assumption of a constant rate of return per foot of exploratory drilling.[30] By developing a new system of coordinates in which the rate of discovery was expressed in barrels per foot (dQ/dh), as a function of the cumulative footage of exploratory drilling (h), and dividing the 1.5 billion feet of drilling that had occurred by 1965 into 15 segments of 100 million feet each, Hubbert showed that the rate of return per foot of exploratory drilling has not remained constant. Hubbert fitted a negative exponential curve to the change in discovery rate and extrapolated that curve in order to arrive at a second estimate of ultimately recoverable resources (see Figure 5). The results of this technique have yielded estimates in the range of 165–172 bbls, figures in close agreement with those produced using the method of production statistics.

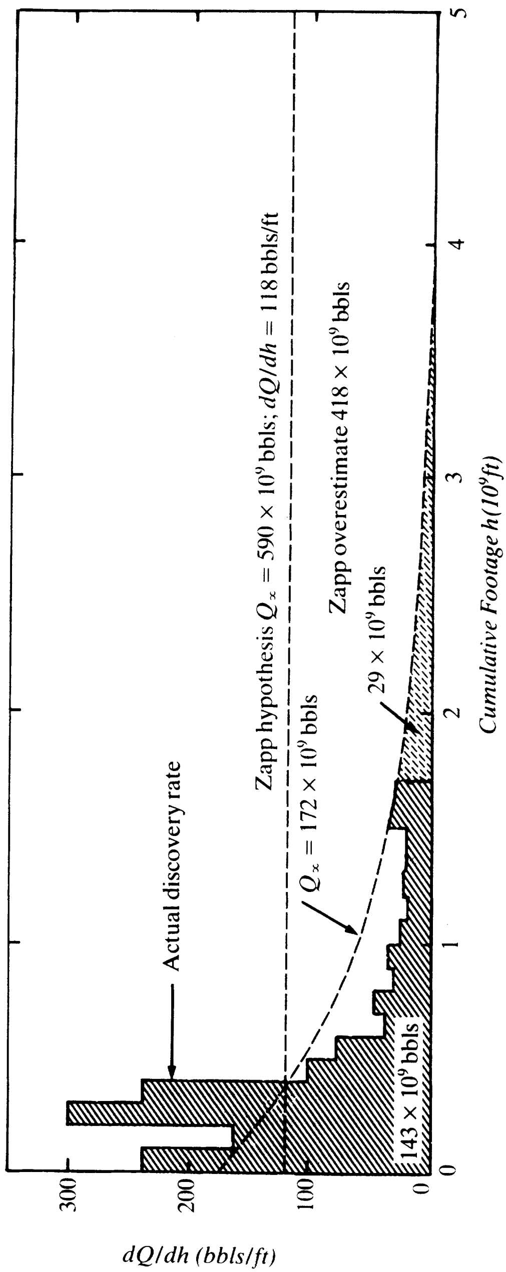
These estimates are, in essence, theoretical predictions about the future. The method of production statistics, unlike other methods of estimating ultimately recoverable resources, provides a number of intermediate predictions (for example, about when the peaks of various trend lines will occur). Although several of the predictions were born out in the 1950s and 1960s, it was not until the early 1970s that Hubbert's estimate was generally accepted. The following sections argue that a non-scientific aspect of the social context (that is, the Arab Oil Embargo) influenced the criteria that were used to constitute the validity of Hubbert's essentially constant predictions.

The Social Construction of Validity

This section uses the methodology of deconstruction[31] to display the interpretive flexibility of Hubbert's constant knowledge claims. For present purposes, 'validity' refers to the consensual acceptance of a knowledge claim within the community of crude oil resource estimators. Thus 'validity' is operationally defined by the presence or absence of widespread disputes.[32]

The most important dimension along which Hubbert's claims will be deconstructed is that of time. As shown in Figure 1 and discussed above, the history of US crude oil estimates can be divided into three distinct periods. Within each of these periods, validity is deconstructed along each of the following dimensions: (a) the value predicted; (b) the method used to generate the prediction; and (c) the implications attached to the prediction.

FIGURE 5
Estimation of Ultimate Crude Oil Production from Lower-48 States, Based upon Discoveries per Foot versus Cumulative Depth of Exploratory Drilling to end of 1971



Source: Hubbert (1974), op. cit. note 27, Figure 50.

Hubbert's First Estimate, 1956

As noted above, Hubbert's first estimate covered the range of informed opinion existing at the time it was made.[33] Thus there was no basis for contesting Hubbert's prediction on the basis of the number itself. Nor was there any special reason for criticizing the estimation methodology.

The paper, however, provided a new basis for drawing implications about the future of the US petroleum industry and the US as an oil based society. Before Hubbert's paper, the projection of 150–200 bbl recoverable served as a source of reassurance. Since the beginning of the industry in the late 1850s, a total of approximately 50 bbl of oil had been produced. Thus, in 1956, the industry was economically alive and looking forward to future production of two or three times that already achieved. The Independent Petroleum Association of America was, for example, drawing attention to a number of factors designed to foster belief in the vitality of the US oil industry: (1) the estimates of petroleum potential made prior to 1950 had already been surpassed; (2) the technologies of exploration and production were continually improving; (3) the rate of discovery exceeded the rate of production; and (4) production capacity exceeded actual production.[34]

Figure 2, however, calls such optimism into question. If one accepts Hubbert's industry life cycle theory, then the consensus estimates imply a peaking of the production rate about 1970. Thus Hubbert's interpretation implied that the US petroleum industry would enter a period of inexorable decline beginning in only 10–15 years. This view clearly contradicted the attitude of optimism held within the industry. The fact that Hubbert's employer (Shell) censored the paper illustrates the extent of that incompatibility. Passages within the preprint draft which made specific predictions about the future of the industry were replaced with much vaguer statements.[35] The published version received national attention in the petroleum press.[36] Despite the fact that its implications had been toned down, virtually everyone concluded that the analysis was seriously flawed.[37]

As suggested by its title, 'Nuclear Energy and the Fossil Fuels', Hubbert's analysis was more than a simple statement of pessimism about the future of the US oil and gas industry. It suggested that dependence upon fossil fuels, like earlier dependencies upon other non-renewable energy sources, must ultimately give way to reliance upon renewable sources of energy, specifically nuclear energy.[38] Hubbert explicitly argued for a reorganization of policy priorities in order to facilitate a smooth transition to a period when fossil fuels could no longer provide for the bulk of US energy needs. It is not hard to understand why an industry in the middle of its greatest period of growth would call such claims into question.

In short, Hubbert took consensually valid scientific knowledge and, through the use of an intuitively appealing technique, drew social implications that contradicted the conventional wisdom of the entire industry.[39] Since Hubbert had used consensually valid knowledge, however, his conclusions could not be criticized on the basis of their scientific foundation. In the following period attempts were made (a) to discredit the intuitively appealing life cycle interpretation, and (b) to discredit the results by producing new and larger estimates.

Hubbert and Contentious Science, 1957–74

Between 1957 and 1974, the controversy surrounding Hubbert's estimates became quite involved. For ten years following the 1956 article, oil company personnel were highly critical of Hubbert's position. After that, however, industry criticism subsided. By contrast, personnel employed by government agencies began to criticize Hubbert around 1962 and continued until 1974. The following cases illustrate the various levels at which Hubbert's views were contested.

The earliest dispute involved Morgan Davis (successively Vice President, President and Chairman of the Board, of Humble Oil) and Richard Gonzalez (Treasurer of the same company). In a series of addresses and articles beginning only days after Hubbert's presentation in 1956 and continuing until their respective retirement and resignation in 1963, Davis and Gonzalez repeatedly criticized Hubbert's forecast.[40] The dispute centred upon the basis for projecting the future behaviour of the production curve. Hubbert, using an analysis of changes in the *rate* of proved discoveries, believed that the production curve would soon peak and then begin to decline. Davis and Gonzalez saw no reason to impose such a peak; in their view the *magnitude* of discoveries suggested a straight line projection of the existing production trends.

> Until such time as there is clear evidence that the domestic industry cannot find more than a barrel of oil for every barrel produced, there is no reason to believe predictions that we will soon be running out of oil. As more oil is discovered, the estimates of ultimate production in the United States will continue to be pushed upward and the predicted date of running out of oil will be forced further into the future.[41]

Both Gonzalez and Davis held views that were incompatible with Hubbert's life cycle theory that resources are discovered, exploited and exhausted. Instead, they held a price-oriented perspective; economic, rather than physical, factors account for the existence of scarcity in the marketplace.

The most fundamental criticism lodged by industry personnel was that Hubbert did not refer to the geology of petroleum occurrence. According to Weeks, one of the earliest and most respected appraisers of petroleum resources,

> A rash of estimates appeared about the time of and shortly after the first publication of my figures. Some of these were not based upon geology, and others showed little appreciation of facts of oil occurrence and the very real factors of geological environment and history that control oil occurrence. Exercises like those that were based on statistics without regard for the controlling fundamentals should not be dignified as estimates of resources.[42]

Thus occupational training influenced the perception of what counted as a scientifically acceptable approach to the problem; geologists did not accept as valid an estimate that was not based upon geological evidence.[43] It should be remembered, however, that Hubbert had produced an estimate based upon geological evidence which corroborated his estimates based upon production statistics.[44] The existence of that estimate was uniformly ignored by the critics.

Another dispute, involving many of the issues raised earlier, took place between John Ryan (an economist for Standard Oil of New Jersey) and Hubbert.[45] Ryan questioned Hubbert's procedure in two ways. First, following Weeks, he claimed the analysis was purely a 'statistical exercise and is not the result of geological or engineering analysis.'[46] Secondly, he advanced a variety of technical objections to Hubbert's curve-fitting techniques. In general, these objections suggested that Hubbert's analysis ignored the role of economics.[47] Thus Ryan contested Hubbert's analysis both theoretically and methodologically. He did not, however, directly dispute the magnitude of Hubbert's estimate.

The most interesting exchange involved one of the technical issues. According to Hubbert, the discovery curve and the production curve should reach the same asymptote; the discovery curve, however, is more advanced. For this reason, Hubbert fitted his curve to the discovery data. Ryan argued that the method should be applied to production data rather than to discovery data because the former are more reliable and, hence, a more reliable curve can be derived from them. According to Ryan, however, this leads to a contradiction; the resulting estimate is less than the amount already produced and discovered. Hubbert shifted the criteria; instead of emphasizing reliability of fit he emphasized the length of the curve defined by the available data. In addition, Hubbert introduced the match between historical data and predictions from his theory as corroborating evidence. Ryan did not even mention the predictions. Soon after this dispute, Hubbert published the results of his second method, a method that avoided Ryan's economically based objections.[48]

The fourth and strangest dispute involved Vincent McKelvey (successively Assistant Chief Geologist, Chief Geologist, and Director of the United States Geological Survey). In 1962, Hubbert, the primary author of a National Academy of Sciences report, dismissed the USGS submission of 590 bbl, based upon the work of Zapp, as unfounded.[49] McKelvey, who had been responsible for organizing the USGS material, authorized an anonymous report which led to several articles questioning Hubbert's estimates.[50] McKelvey's later statements took a variety of forms: (a) support for Zapp's method and findings;[51] (b) support for other people's criticism of the Hubbert technique;[52] and (c) producing alternative estimates of his own.[53] Although McKelvey endorsed the methodological critiques of others, he focused his objections upon the magnitude of the estimate and the differential implications for public policy that a larger estimate suggested.[54]

In 1963, Hubbert left Shell Oil to take up employment as Research Geophysicist for the USGS, a position he held for nearly 15 years. As such, McKelvey was his nominal boss for much of that time. Although Hubbert produced several petroleum resource estimates while employed by the Geological Survey, these estimates were never published under the auspices of the USGS.

In sum, the initial objections to Hubbert's research came from the oil industry and focused upon his estimation methodology. Later objections came from government personnel and emphasized the disparity between Hubbert's estimates and those of the Geological

Survey. Despite the shift in focus, each critique provided a basis for dismissing the implications of Hubbert's analysis; if the method and/or results are incorrect, then the policy implications drawn from these results are also incorrect. Thus scientific argument provided a basis for denying Hubbert's pessimistic conclusions about the future of the US oil industry.[55]

All such controversies became academic by 1974; after the Arab Oil Embargo, Hubbert's pessimism appeared prophetic to everyone. In late 1973, the magnitude of imports from the Organization of Arab Petroleum Exporting Countries (expressed as a percentage of total US imports) dropped from over thirty percent to about two percent.[56] The resultant crisis in the delivery of short-term supply, coupled with the concomitant gas lines and price increases, produced a social environment in which optimistic resource estimates were called into question.

Hubbert and Consensus Science, 1974–81

In the early 1970s, despite the continued existence of high estimates by the USGS and others, a growing body of industry opinion held that Hubbert was basically correct. Although government distrust of the USGS estimates can be traced to mid-1971 (when the Senate authorized a study on national fuels and energy policy), that scepticism remained latent until the Arab Embargo. As noted by Omang, 'It was not until after the 1973 oil crisis that Hubbert's scientific achievement was recognized'.[57]

On 26 March 1974, McKelvey presented a new USGS estimate to the Senate Committee on Interior and Insular Affairs. That estimate, influenced by petroleum geologists who had recently left industry positions to join the Geological Survey, was lower than previous USGS estimates. This lower estimate was vigorously attacked by John Moody (Vice President of Mobil Oil). On 8 April 1974, Moody sent a letter to McKelvey stating that the USGS estimates for the continental US were ten times higher than any that Mobil could justify. Copies of the letter were sent to influential Senators, the National Academy of Sciences, and *Science*. Thus the controversy that had existed within the scientific community for over 15 years made a spectacular public appearance at the height of the Arab Oil Embargo.[58]

The resulting public controversy led to a conference on 5 June

1974 of the National Academy of Science's Panel on Estimation of Mineral Resources. The Geological Survey submissions suggested that unexplored sediments would continue to yield oil at 50–100 percent of the rate of sediments previously drilled. Hubbert and the industry representatives argued that the rate would be closer to 10 percent. The Academy concurred with the latter view.

In September of 1974, the expanded Oil and Gas Branch of the USGS undertook a new appraisal of US resources.[59] That report drastically lowered the USGS estimate and placed the figure in rough correspondence with other estimates made since 1974. A second Geological Survey estimate, written by an executive assistant of McKelvey's, was published almost simultaneously.[60] This publication, which included an estimate much closer to earlier Survey figures, did not go through the Survey's internal refereeing process. Subsequent Survey publications have treated the former figure as official and (with the exception of the latter) estimates made by the industry, the government, and independent estimators since 1974 have been relatively close to one another (see Figure 1). In 1977, the incoming Carter administration forced McKelvey to resign as Director of the Geological Survey.[61] According to some,[62] McKelvey's continued support of optimistic estimates was the reason behind his very unconventional removal from office. Roughly a week after the announcement that McKelvey would be replaced, Hubbert received a major public service award.[63]

The consensus estimate that emerged after the Arab Embargo was very near the figures that Hubbert had been asserting for nearly 20 years (see Figure 1).[64] Since 1974, Hubbert's methodology has also gained acceptance. Before 1974, only two individuals other than Hubbert had produced estimates using exploitation history methods: since 1974, ten additional individuals have produced estimates using them.

Hubbert's conclusions about the future of the domestic petroleum industry and the need for other energy sources have also gained acceptance. In 1975, Jack Carlson (Assistant Secretary for Energy and Minerals in the Department of the Interior) testified to the Joint Economic Committee of the US Congress as follows:

> [The] current range of estimates, using either low or high estimates, clearly indicates the need to consider other sources of energy in the long run. . . .
> [The] current range of estimates, using low or high estimates, clearly indicates the need to conserve oil and gas for the long run.[65]

Differences which had underpinned policy disputes for over 15 years now implied similar policies!

Social Context and the Reconstruction of Meaning

The previous section deconstructed the consensual 'validity' of Hubbert's estimates of ultimately recoverable US crude oil across a number of dimensions: time, method, magnitude and implications. The results of these decompositions are summarized in Table 1.

TABLE 1
The Consensual Validity of Hubbert's Estimation Technique, Resource Estimate and Social Implications as Socially Constructed at Various Points in Time

Chronological Period	1956	1957–73	1974–82
Estimation Technique	NA	–	+
Magnitude of Estimate	+	–	+
Social Implications	–	–	+

Key: (NA) : Not applicable.
(–) : Not constructed as valid; subject to controversy.
(+) : Constructed as valid; not subject to controversy.

This section examines how and why one objectified status (namely, that Hubbert's method and results are invalid) was destroyed and another objectified status (namely, that Hubbert's method and results are valid) was constructed. The analysis will be restricted to the 1974 shift — a shift that is not conflated with changes in Hubbert's scientific practice.

How Hubbert's Estimate was Reconstructed

Before 1974, Hubbert was considered wrong. The current view is that Hubbert was right and the scientific community erroneously discounted his views before 1974.[66] Conventional wisdom holds that

Hubbert's ability correctly to predict the peaking of US crude oil production 15 years before its occurrence gained him a reputation as an 'oracle',[67] and brought about the change in objectified status. Thus the explanation given is positivistic: Hubbert's method became accepted as valid when it was shown to yield results that corresponded to the observable world. This could not occur, however, until the data of the predicted peak had passed and it was known that a peak had actually occurred.

This positivistic account reconstructs the debates over the validity of Hubbert's method. Its focus is entirely upon the technical arguments with Davis, Ryan and Gonzalez over the timing of the peak in the production curve. No mention at all is given to the more fundamental criticism that undiscovered petroleum cannot be estimated without referring to the geology of petroleum occurrence. The fact that Hubbert correctly identified the peaking of the production curve, however, does nothing to destroy the basis of that criticism.

Other lines of evidence also display the inadequacy of the positivist account. First, Hubbert's method had correctly predicted the peaking of several other less significant trend lines (those for rate of proved reserves and rate of discoveries) as early as 1962. Thus the method was corroborated long before the peaking of production in 1970, but these corroborations did nothing to establish the validity of the technique. Second, the peaking of production does not necessarily entail the accurate calculation of ultimate production. As noted by Martinez, many countries have experienced multiple peaks in their production curves.[68] Similarly, the production on either side of the point of inflection may not be symmetrical. Most astonishingly, Hubbert has abandoned the use of the technique as a means for estimating ultimately recoverable world crude oil sources because, by his own admission, it yields erroneous results![69] In short, Hubbert's technique is still open to criticism, even though these objections are not presently being deployed.

These facts suggest the importance of Collins's metaphor of 'interpretive charity'.[70] The problem is not to explain why Hubbert's ontologically correct claim was initially discounted but, rather, to understand how and why individual actors offered more or less charitable interpretations of Hubbert's constant claims. Selective reconstruction, resulting in a focus upon the timing of the inflection point as the sole basis for previous objections, was made possible by the intrusion of an external event — the Arab Oil Embargo. Without that event, the peaking of production at the predicted date may well have had no more effect in establishing the validity of Hubbert's method than had his correct prediction of earlier peaks in other trend lines. Unlike those other peaks, the peaking of the production rate was directly connected to the ability of the domestic oil industry to provide sufficient short-term supply, and occurred at a time of great short-term need. This focused the attention of policymakers and the public upon the production rate shortly after Hubbert's prediction was borne out — and, hence, lent particular weight to it.

The discursive techniques used to undermine previous objections were also important. Most significantly, the criteria for constituting validity shifted from an emphasis upon *method* to an emphasis upon *results*. Before 1974, the main thrust of criticism had been against the method of estimation rather than the estimate itself. Given the absence of agreement about the magnitude of ultimately recoverable resources during the period 1957–74, no technique, Hubbert's included, could use an 'accurate' estimate to validate itself. This situation parallels Collins's description of gravity wave measurement.[71] The debate centred upon method itself, and Hubbert's method was treated as invalid by the geological community because it was predicated upon criteria perceived by that community as irrelevant to the physical existence of petroleum.

In 1974, Moody shifted the focus of the debate from method to results. Previous objections to Hubbert's method were dropped because Moody reported the results of five different estimation techniques, each of which yielded results much closer to Hubbert's estimates than to those of the Geological Survey. Hubbert's technique thus became just one more method that justified a value of around 190 billion barrels. As in Collins's gravity wave case, the ability of a technique to reproduce results broadly consistent with others served as evidence for the validity of that technique.

The rhetorical strategies of Moody were not, however, solely responsible for the shift in emphasis from method to results. His input was effective because it played upon the concern among policymakers that resulted from the Arab Oil Embargo of 1973–74. The debate was transferred from its relatively isolated location within scientific journals to a publicly observable policy forum. The policymakers were primarily concerned with which estimates were correct — that is, with results.

If the Geological Survey is correct, then increased oil and gas production may

> grant the Nation a longer period to reduce dependence on oil and gas while still holding our oil imports to a prudent level. If the National Academy panel is correct, however, then it becomes far more urgent to find acceptable ways to mine and utilize more of the Nation's coal; to get more nuclear capacity into operation; to substitute solar power for fossil fuels wherever feasible; to push energy conservation more rapidly; and to accelerate research and development on revolutionary technologies to relieve our dependence on conventional energy sources.
>
> So the issue is drawn.[72]

Moody made no particular effort to establish the validity of Hubbert's estimation procedure. He was merely reporting the results of a number of estimates produced by Mobil, with the aim of demonstrating the necessity for governmental support of policies advocated by Mobil. Organizational interests were thus indirectly responsible for the reassessment of Hubbert's estimates.

Why Hubbert's Estimate was Reconstructed

The oil industry played the key role in the reconstruction of Hubbert's estimates. During 1957–74, the industry produced estimates that fell between those of Hubbert and the government. These estimates were interpreted in a manner that made them appear to harmonize with those of the government and to contradict those of Hubbert. Although the industry estimates did fall slightly after 1974, the primary change was in the interpretation that was placed upon them. By switching from an interpretation based upon a cost-price framework (that is, the view that economic changes will bring previously uneconomic portions of the resource base into the marketplace and, in effect, increase the magnitude of the presently existing estimate) to one based on a storehouse framework (that is, the view that physical factors limit the amount of the resource base that can be brought into the marketplace and, hence, present estimates represent the maximum expectable production) the industry continued to produce estimates of roughly the same magnitude, while making those estimates harmonize with Hubbert's estimates rather than those of the government. The reason behind this shift in interpretation can be traced to a change in the means used by the industry to pursue profits. And that change resulted from the disruption of the political economy of the oil industry associated with the Arab Oil Embargo.

Between 1957 and 1974, most oil industry personnel, while admitting that physical limitations must ultimately constrain any non-renewable resource that is subject to continuous demand, held that, given proper economic incentives, these constraints would not impair the production of oil from the US until well into the future. Three distinct lines of evidence document the importance and popularity of the cost-price view among industry employees.[73] First, as shown above, a number of the objections to Hubbert's estimation method (for example, those of Davis, Gonzalez and Ryan) were economically based. Secondly, critiques of estimates made before 1957 (that is, those that gave numerical results compatible with Hubbert's) held that the role of economic change and technological advance had not been properly integrated into those earlier estimates.[74] Thirdly, and perhaps most importantly, the individuals who revised their previous estimates upward after 1956 gave economic and technological advance as the reason.[75]

Industry personnel emphasized the significance of the relatively small numerical difference between their estimates and those of Hubbert, while minimizing the importance of the larger numerical disagreement with government estimates, because, in the 1957–74 context, such tactics facilitated the maximization of industry profits. One way of maximizing profits is to obtain economic concessions from the government (for example, tax concessions like the depletion allowance or changes in the then regulated price of oil and gas). Hubbert's view undermines the arguments for such concessions: if physical limits will soon constrain production, then it makes no sense for the government to provide concessions designed to facilitate exploration because the oil is not there to be found. Similarly, to hold that there exist large amounts of undiscovered oil which can be produced given existing economics and technology (the position adopted by the government) also undermines the argument for economic concessions. By contrast, the industry position that there exist relatively small amounts of oil available under current economic conditions but a large additional increment that could be recovered with more favourable economic conditions, justifies the industry's arguments for concessions.[76]

Given the industry's adherence to a cost-price framework, the increasing importation of foreign oil was seen not as indicative of a physical limitation on US supply but, rather, as a sign of the economic advantage of foreign oil. The government also held this view: beginning in 1955, it introduced first voluntary, and then mandatory, oil import controls as a means to support the price of domestic crude oil. Beginning in 1970, a series of major policy modifications enabled the importation of considerably increased volumes. By 1973, the OPEC countries had gained enough market power to raise the price of foreign oil to the approximate level of domestically produced crude, and Nixon scrapped the Mandatory Oil Import Program in favour of a tariff system.[77]

Against this backdrop, a 1971 report by the National Petroleum Council,[78] an industry organization which acts in an advisory role to the Secretary of the Interior, concluded that the continuation of existing government policies would lead to an increased dependence upon eastern hemisphere oil and an acute shortage of gas. However, given 'adequate economic incentive in careful coordination of effort between government and industry', higher growth rates in domestic supplies would be possible. As domestic production peaked and the economic health of the independent producers and refiners became problematic, the carrot of potentially secure petroleum supplies and the stick of potentially disastrous petroleum shortages was made more and more explicit.

The Arab Oil Embargo of 1973–74 had two major impacts upon the political economy of the oil and gas industry. First, it resulted in a quadrupling of the market price for oil. As a result, by late 1974, the existing price exceeded the highest price suggested by any of the NPC scenarios. Thus there was no longer any need for the government to provide economic concessions in order to reap the benefits that the NPC report claimed would flow from increased profitability within the industry. Indeed, profit levels had risen so much that the government was considering imposition of 'windfall profits' legislation. Second, the Embargo destroyed the conception commonly held within the US-controlled multinationals that foreign sources provided an adequate and secure source for supplementing US production. In other words, the multinationals lost the control over foreign production that they had previously taken for granted. Thus the attention of the multinationals came to focus upon domestic resources.[79]

By 1975, Moody and the rest of the industry had shifted to a storehouse interpretation of their estimates. Before the Embargo, such an interpretation would have undermined the industry's arguments for concessions on domestic oil.[80] As the following passage makes clear, however, Moody was not advocating measures designed to enhance domestic production from *conventional* sources when he called McKelvey's estimates into question:

> Any thoughtful analysis of petroleum-based energy resources shows the US in a weakened position and moving into a worse position for the future. World petroleum resources may last into the early years of the 21st century, but US resources will be substantially exhausted well before then. Though it is essential to continue to invest relatively modest sums to increase overseas petroleum production immediately, our obvious course is to make the more substantial investments in new technology and in a new syncrude industry which will enable us to capitalize on the generous fossil resources with which North America is endowed. Government indecision and even punitive decisions are unnecessary deterrents to this urgent, vital effort.[81]

This position represents a sophisticated response to the changes brought about by the Embargo: it justifies the industry's need to retain the increased profits, while providing a new basis for requesting government concessions and explaining why the industry cannot immediately increase domestic production in order to rectify the existing shortage in supply. As nearly everyone acknowledged, the development of a synthetic fuels industry would require large capital expenditures as well as extensive technological development — both of which would place heavy financial strains upon the industry.

The point is not that industry personnel consciously manipulated their data and interpretations, or that their estimates were made in 'bad faith' in order to influence the government toward particular policy choices. Such a reading of the events seriously oversimplifies the dynamics of the situation. Organizations, as places of employment, structure the affiliations of their employees and give rise to localized cultures.[82] These cultures influence the production of scientific knowledge by leading to selective perception and differential interpretation of particular pieces of evidence. Thus, before 1974, industry personnel discounted the utility of using discovery data and other sources of non-geologic information to draw inferences about future production rates, because the results of such analysis contradicted the conventional wisdom about the industry's ability to continue to satisfy increasing consumer demand. Organizational cultures, however, are not isolated from the changes that affect the political economic environment of the organization.[83] Thus when the Arab Oil Embargo brought about a realignment of the industry's ability to satisfy short-term demand, it also brought about a reinterpretation of Hubbert's estimates. Additionally, since such organizational interests exist upon a structural level defined by the dynamic relationships among a network of organizational types,[84] the interests impact consistently across the localized cultures developed within the various industry corporations or governmental departments. Thus organizational interests account for both (1) the shared perceptions of individuals employed by differing industry corporations or by differing government departments toward Hubbert's estimates at a particular point in time, and (2) the change in those perceptions through time.

Conclusion

Among the criteria underpinning the attribution of validity to a particular knowledge claim in the local context is the phenomenological notion of 'identity', or equivalence. Between 1957 and 1974, equivalence among resource estimates was established by reference to the methodology used to produce the estimate. Thus, during that period, Hubbert's unorthodox estimation methodology stood as evidence of the non-equivalence of his estimates with those made by other members of the estimation community. After 1974, the criteria for establishing equivalence shifted from method to results and, given the changing magnitudes of the estimates being produced and their changing interpretation, Hubbert's estimates came to be consistent with most others, while McKelvey's estimate was established as unorthodox. Although the equivalence or non-equivalence between Hubbert's method/results and those of other 'valid' estimates was established locally and relatively autonomously by each estimator, these individual attributions were similar among estimators operating in similar organizational contexts because organizational interests influenced the selection of criteria used to establish the local attributions. Thus the practice of science within an industrial context produced certain similarities of outlook that led industrial personnel towards commonly held conceptions of equivalence or non-equivalence between Hubbert's work and that which they considered 'valid'. The shift in attributional validity accorded Hubbert's estimate after 1974 resulted from a shift in the means of pursuing the industrial interest of profit maximization — a shift that occurred because of the Arab Embargo. Thus the attribution of validity by a community of scientists to a specific knowledge claim can be seen as the result of a series of causal linkages between social context, organizational interests, and the perception of estimates as equivalent. Through these linkages an aspect of the social context seemingly unconnected to the scientific validity of the knowledge claim has been shown to explain the change in attributional validity imputed to a constant knowledge claim at two points in time.

• *NOTES*

I would like to thank the following for reading and commenting upon earlier drafts: Gus Brannigan, Michael Dennis, John Law, Trevor Pinch and Dick Wanner. All errors, however, remain my responsibility.

1. T.H. McCulloch, 'Oil and Gas', in D.A. Brobst and W.P. Pratt (eds.), *United States Mineral Resources* (United States Geological Survey Professional Paper 820, 1973), 477–96; M. King Hubbert, 'US Petroleum Estimates, 1956–1978' (Annual Meeting Papers, American Petroleum Institute, Production Department, 1978).

2. 'Ultimately recoverable' refers to the total amount of petroleum resource expected to be produced from a given geographical area. These terms include the total of (1) past production; (2) presently known but unproduced petroleum reserves; and (3) projections of undiscovered, recoverable petroleum resources. Remaining recoverable resources refers to the sum of categories (2) and (3). The geographical area covered by the estimates examined in this paper included the continental US and its associated offshore areas. This is a conservative definition of the geologic US and, hence, one can examine changes in the estimates through time without having them conflated by changes in the definition (for example, inclusion or exclusion of Alaska).

3. H. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology*, Vol. 9 (1975), 205–24; Collins, 'Son of Seven Sexes: The Social Destruction of a Physical Phenomenon', *Social Studies of Science*, Vol. 11 (1981), 33–62; A. Brannigan, *The Social Basis of Scientific Discoveries* (Cambridge: Cambridge University Press, 1981).

4. B. Barnes, *Interests and the Growth of Knowledge* (London: Routledge and Kegan Paul, 1977); S. Shapin, 'The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes', in R. Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge* (Keele, Staffs.: University of Keele, *Sociological Review* Monograph No. 27, 1979), 139–78; D. MacKenzie, *Statistics in Britain, 1865–1930* (Edinburgh: Edinburgh University Press, 1981).

5. Data from two different sources have been used in arriving at the various social constructions involved: (1) an analysis of the relevant literature (including journal articles, papers presented at meetings, reviews, letters to the editor, and the like); and (2) interviews with a number of key individuals who were either involved in or observed the disputes.

6. J. Habermas, *Knowledge and Human Interests* (London: Heinemann, 1972).

7. For theoretical statements and empirical applications of the interests model, see the references cited in note 4.

8. The paradigmatic example of the attribution model is Brannigan, op. cit. note 3. Other authors who share similar concerns, but adopt somewhat different explanatory frameworks, include B. Latour and S. Woolgar, *Laboratory Life* (Beverly Hills, Calif.: Sage, 1979), and M. Lynch, *Art and Artifact in Laboratory Science* (unpublished PhD dissertation, University of California at Irvine, 1979; London: Routledge & Kegan Paul, 1985).

9. Barnes, op. cit. note 4, 58.

10. The concept of 'enactment' is developed in K. Weick, *The Social Psychology of Organizing* (Reading, Mass.: Addison-Wesley, 1979). For a treatment of organizational environments as network relations, see C. Perrow, *Complex Organizations: A Critical Essay* (Glenview, Ill.: Scott Forsman, 1979), Chapter 6. For specification of the organizational interests of actors in the political economy of the international oil industry, see M. Tanzer, *The Political Economy of International Oil and the Underdeveloped Countries* (Boston, Mass.: Beacon Press, 1969), 20–29.

11. See, for example, Mackenzie, op. cit. note 4; S. Shapin, 'Phrenological Knowledge and the Social Structure of Early 19th Century England', *Annals of Science*, Vol. 32 (1975), 219–43.

12. B. Barnes, 'On the "Hows" and "Whys" of Cultural Change', *Social Studies of Science*, Vol. 11 (1981), 481–98, esp. 482, 487.

13. Brannigan, op. cit. note 3.

14. Collins, op. cit. note 3.

15. Brannigan, op. cit. note 3, Chapters 6, 7.

16. M. King Hubbert received his scientific education in the 1920s from the University of Chicago. He obtained a BS, MS and PhD in geology, physics and mathematics respectively. During and following that period he worked as an oil geologist for Ameranda Petroleum Corporation; taught geology and geophysics for a decade at Columbia University; did summer geophysical exploration for several government agencies; and spent a year and a half during the second world war working on mineral resource studies with the Board of Economic Warfare. In the years following the war (1943–63), Hubbert worked for Shell Oil and Shell Development Companies as research geophysicist, associate director of exploration and production research and chief consultant (general geology). Beginning in 1963, he assumed the position of research geophysicist with the US Geological Survey, a position he held until his retirement in 1976. At various times during this period he also held appointments in Geology and Geophysics at a number of major universities. Hubbert holds membership in many major scientific and engineering societies. Among these memberships are the Geological Society of America (President, 1962); the American Association of Petroleum Geologists (Associate Editor, several times Distinguished Lecturer); Society of Exploration Geophysicists (Honorary Life Member, Editor); Society of Petroleum Engineers of the American Institute of Metallurgical Engineers (Distinguished Lecturer); American Association for the Advancement of Science; American Academy of Arts and Sciences; and the National Academy of Sciences/National Research Council.

17. Hubbert is most noted for his contributions to the latter area, having received the Arthur L. Day Medal of the Geological Society of America in 1954 for his work in geophysics and, in 1971, the Anthony F. Lucas Gold Medal Award from the American Institute of Metallurgical Engineers for his contributions to petroleum engineering.

18. Given the constancy of Hubbert's estimates and the fact that his view is now the consensual one, some individuals may wish to treat Hubbert as a heroic figure in

possession of the truth valiantly attempting to bring his message about the physical shortage of petroleum to a wasteful public. That is not the intended reading of the present analysis; the stance adopted here is one of agnosticism toward the ontological validity of the various estimates involved. For a defence of relativism, see Barnes, op. cit. note 4.

19. Previous research — for example, G. Bowden, 'Estimating US Crude Oil Resources: Organizational Interests, Political Economy and Historical Change', *Pacific Sociological Review*, Vol. 25 (1982), 419–48; Bowden, 'The Structural Basis of a Scientific Controversy: The Case of US Crude Oil Estimates, 1957–1974', paper presented to the Society for Social Studies of Science conference (Blacksburg, Virginia, November 1983); and M. Dennis, *Drilling for Dollars: The Making of American Petroleum Reserve Estimates, 1921–1925* (unpublished BA thesis, University of Pennsylvania, 1982) — has suggested that differences among resource estimates spring from differing organizational and professional interests. If this is true, and given the fact that Hubbert was employed in the same types of organizations as the other estimators, why didn't Hubbert's estimates change in the same manner as the others? Hubbert's biography provides a number of clues. First, unlike most other estimators, he was not a technocrat tied to one particular scientific problem. Second, he made his reputation in the study of petroleum hydrodynamics long before he began making resource estimates. Thus, Hubbert was able to draw upon his early prestige. (The case of Barkla exemplifies a similar process: see B. Wynne, 'C.G. Barkla and the J Phenomenon: A Case Study of the Treatment of Deviance in Physics', *Social Studies of Science*, Vol. 6 [1976], 307–47.) Third, unlike other estimators, Hubbert was frequently employed by more than one organizational type at the same time (for instance, Shell Oil, Stanford University, the USGS). Thus he was not subjected to a single, pervasive organizational setting as were other estimators. Fourth, while employed as a 'research geophysicist' by the USGS, a period that corresponds largely with the time that Hubbert's estimates were disputed, Hubbert was able to research whatever he wanted without pressure from the administration. Thus, due to a peculiar configuration of elements in Hubbert's personal biography, he was able largely to ignore and, to some extent, even avoid the organizational cultures that led other estimators to perceive evidence in a particular manner.

20. For an interesting historical analysis involving many of the same methodological issues (but not involving chronological change), see S. Shapin, 'Of Gods and Kings: Natural Philosophy and Politics in the Leibniz-Clarke Disputes', *Isis*, Vol. 72 (1981), 187–215.

21. H. Hedberg, 'The Volume of Sediment Fallacy in Estimating Petroleum Resources', in J. Haun (ed.), *Methods of Estimating the Volume of Undiscovered Oil and Gas Resources* (Tulsa, Oklahoma: American Association of Petroleum Geologists, 1975), 160.

22. J. Harrison, 'The Nature and Significance of Geological Maps', in C. Albritton (ed.), *The Fabric of Geology* (San Francisco, Calif.: W.H. Freeman, 1963), 37–63.

23. M. King Hubbert, 'Nuclear Energy and the Fossil Fuels' (Annual Meeting Papers, American Petroleum Institute, Section on Drilling and Production Practice, 1956), Vol. 36, 7–25; D.F. Hewett, 'Cycles in Metal Production', *Transactions of the American Institute of Mining and Metallurgical Engineers*, Petroleum Division, Vol. 82 (1929), 65–98.

24. M. King Hubbert, 'Survey of World Energy Resources', *Canadian Mining and Metallurgical Bulletin*, Vol. 66 (1973), 41–42.

25. M. King Hubbert, *Energy Resources* (Washington, DC: National Academy of Sciences/National Research Council, 1962).
26. Ibid., 50–73.
27. M. King Hubbert, 'Degree of Advancement of Petroleum Production in the United States', *Bulletin of the American Association of Petroleum Geologists*, Vol. 51 (1967), 2207–27; Hubbert, 'Energy Resources', in National Academy of Sciences/ National Research Council, *Resources and Man* (San Francisco, Calif.: W.H. Freeman, 1969), 157–242; Hubbert, 'US Energy Resources, A Review as of 1972, Part I', background paper prepared for US Senate, Committee on Interior and Insular Affairs (Washington, DC: US Government Printing Office, 1974).
28. A. Zapp, 'Future Petroleum Producing Capacity of the United States' (USGS Bulletin No. 1142-H, 1962).
29. See, for example, T. Hendricks, 'Resources of Oil, Gas, and Natural-Gas Liquids in the United States and the World' (USGS Circular No. 522, 1965); V. McKelvey and D. Duncan, 'United States and World Resources of Energy', Symposium on Fuel and Energy Economics, *Proceedings of the American Chemical Society*, Division of Fuel Chemistry, Vol. 9, No. 2 (1965), 1–17; P. Theobald et al., 'Energy Resources of the United States' (USGS Circular No. 650, 1972); McKelvey, 'USGS Releases Revised US Oil and Gas Estimates' (US Department of Interior, News Release, 26 March 1974).
30. Hubbert, op. cit. note 24; similar critiques can also be found in the references cited in note 27.
31. For an elaborate explanation of this methodology, see T. Pinch, *The Development of Solar Neutrino Astronomy* (unpublished PhD thesis, University of Bath, UK, 1982).
32. Use of citation counts to determine the proportion of positive reviews given Hubbert's work at different times leads to the same interpretation.
33. Hubbert, op. cit. note 23. Hubbert revised the published version of the paper to include the range of 150 to 200 bbl recoverable (over the preprint version of 150 bbl) when several estimates of 200 bbl were brought to his attention.
34. Independent Petroleum Association of America, *Petroleum in the Western Hemisphere* (Washington, DC: IPAA, 1952).
35. For a comparison of the wording of the preprint draft and the subsequent printed version, see Hubbert, op. cit. note 1, 4.
36. Anonymous, 'Is Oil Nearing a Production Crisis?', *Petroleum Week* (16 March 1956), 9–10.
37. Hubbert, op. cit. note 1, 5.
38. The tone of Hubbert's presentation is aptly illustrated by the following quote from Hubbert, op. cit. note 25, 132–33; 'If world-wide industrial collapse due to the exhaustion of the fossil fuels . . . is to be forestalled, there appears to be no possible way of accomplishing this except by a newer and larger supply of energy suitable to the requirements of large-scale industrial operations. We have already observed that, while solar power is of this magnitude, it does not offer much promise of concentration . . . Water power is of a lesser magnitude . . . The only remaining source of energy that does have the proper magnitude and does lend itself to large industrial uses is nuclear.' Hubbert's later writings shifted the emphasis from nuclear power to other sources of renewable energy, but the pessimism about the future of the fossil fuels industry remained.
39. The phrase 'intuitively appealing' is not meant to imply a universal grammar of

human cognition but, rather, to highlight how Hubbert's technique trades upon certain unquestioned assumptions shared by geologists. It treats oil as a finite physical stock that cannot be replenished. By contrast, economists treat price as the primary constraint upon availability. For discussions of these two approaches see Congressional Research Service, 'Are We Running Out? A Perspective on Resource Scarcity', US House of Representatives, Commitee on Interstate and Foreign Commerce, Subcommittee on Energy and Power, 95th Congress, 2nd Session, Committee Print 95–57, 1978; or A. Wildavsky and E. Tenenbaum, *The Politics of Mistrust* (Beverly Hills, Calif.: Sage, 1981).

40. See, for example, R. Gonzalez, 'Petroleum for our Future Progress', *Journal of Petroleum Technology* (March 1957), 44–51; or M. Davis, 'The Dynamics of Domestic Petroleum Resources', *Proceedings of the American Petroleum Institute*, Vol. 38, No. 1 (1958), 22–27.

41. Gonzalez, op. cit. note 40, 49. Empirically, there was no way to resolve the debate; throughout the 1960s the rate of petroleum production continued the almost linear increase that had begun in 1933.

42. L. Weeks, 'Estimation of Petroleum Resources: Commentary', *Bulletin of the American Association of Petroleum Geologists*, Vol. 50 (1966), 2009.

43. See D. Robbins and R. Johnston, 'The Role of Cognitive and Occupational Differentiation in Scientific Controversies', *Social Studies of Science*, Vol. 6 (1976), 349–68.

44. Hubbert, op. cit. note 25, 64–73.

45. J. Ryan, 'National Academy of Sciences Report on Energy Resources: Discussion of Limitations of Logistic Projections', *Bulletin of the American Association of Petroleum Geologists*, Vol. 49 (1965), 1713–20; M. King Hubbert, 'Reply', ibid., 1720–27; Ryan, 'Limitations of Statistical Methods for Predicting Petroleum and Natural Gas Reserves and Availability', *Journal of Petroleum Technology* (March 1966), 281–84; Hubbert, 'Reply to J.M. Ryan', ibid., 284–86.

46. Ryan (1965), op. cit. note 45, 1713.

47. Ibid., 1715–19. Among the objections levelled were the following: there is no *a priori* reason for choosing one asymptotic growth curve over another; the analysis violates the underlying assumptions of logistic projections; and the procedure ignores changes in pricing and technology that affect production.

48. Hubbert, op. cit. note 27.

49. Hubbert, op. cit. note 25; Zapp, op. cit. note 28.

50. Hubbert, op. cit. note 1, 13. Hubbert alleges that McKelvey wrote the anonymous report; I have been unable to verify this point. For an example of the critical reaction engendered by the anonymous report, see 'US Reserves Put at 600 Billion Barrels', *Oil and Gas Journal* (2 September 1963), 78–79.

51. 'Those who have studied Zapp's method are much impressed with it, and we in the Geological Survey have much confidence in his estimates': quoted in Hubbert, op. cit. note 25, 48.

52. 'John Ryan's fine analysis of recent attempts to estimate the extent of potential petroleum resources correctly identifies their weakness': quote taken from V. McKelvey, 'Reply to J.M. Ryan', *Journal of Petroleum Technology* (March 1966), 287.

53. McKelvey and Duncan, op. cit. note 29; McKelvey, op. cit. note 29.

54. McKelvey, op. cit. note 52. See also V. McKelvey, 'Contradictions in Energy Resource Estimates', *Proceedings*, 7th Biennial Gas Dynamics Symposium, The

Technological Institute, Northwestern University, 1967, 5–23; McKelvey, 'Mineral Resource Estimates and Public Policy', in D. Brobst and W. Pratt (eds.), *United States Mineral Resources* (USGS Professional Paper 820, 1973), 9–19.

55. Hubbert's position was not totally unsupported during this period. As early as 1958 Hubbert possessed hidden support among some technical personnel involved in resource estimation: see Hubbert, op. cit. note 1, 5. The rare examples of public support for Hubbert's position during this period include: W. Davis, 'Future Productive Capacity and Probable Reserves of the US', *Oil and Gas Journal*, Vol. 56 (1958), 105–19; and H. Hudson, 'Is the "Song of Plenty" a Siren Song?', ibid., Vol. 61 (1963), 131–36.

56. American Petroleum Institute, *Basic Petroleum Data Book*, Vol. 2, No. 3 (1982), 9.

57. J. Omang, 'Oil Prophet Cited: Geologist Saw Crisis in 1948', *Washington Post* (15 November 1977), A-1.

58. Moody's tactics show a certain resemblance to those of Galileo; he made his arguments accessible to the layman: see P. Feyerabend, *Against Method* (London: New Left Books, 1975). This also coheres with the role of rhetorical strategies in the destruction of gravity waves as a physical phenomenon and the reconstruction of Mendel's contribution: see Collins, op. cit. note 3, and Brannigan, op. cit. note 3, Chapter 6.

59. B. Miller et al., 'Geological Estimates of Undiscovered Recoverable Oil and Gas Resources in the United States' (USGS Circular 725, 1975).

60. B. Grossling, 'In Search of a Statistical Probability Model of Petroleum Resource Assessment' (USGS Circular 724, 1975).

61. R. Lyons, 'Interior Secretary Will Replace Head of US Geological Survey', *New York Times* (7 September 1977), 134.

62. T. O'Toole, 'Chief Geologist's Downfall', *Washington Post* (13 November 1977), A-6; B. Bartlett, 'Killing the Messenger: The Carter Administration and the Facts about Oil and Gas', *The Washington Monthly,* Vol. 10 (1978), 57–60.

63. Omang, op. cit. note 57.

64. The consensus which emerged was actually counter-intuitive to the logic generally used to account for the influence of economic changes upon resource estimates. According to that logic, an increase in price should lead to an increase in the magnitude of remaining recoverable resources by bringing into production a portion of the resource base that was previously sub-economic and, hence, unexploitable. The consensual magnitude which emerged, however, was lower than the estimates produced prior to the quadrupling of price in 1974.

65. US Congress, 'Adequacy of US Oil and Gas Reserves', Hearing before the Joint Economic Committee, 94th Congress, 1st Session, 1975, 3.

66. For an examination of the retrospective and prospective construction of appearances in relation to the documentary method of interpretation, see Brannigan, op. cit. note 3, Chapter 7.

67. R. Gillette, 'Oil and Gas Resources: Did the USGS Gush Too High?', *Science*, Vol. 185 (12 July 1974), 127–30.

68. A. Martinez, 'The Estimation of Petroleum Resources', *Bulletin of the American Association of Petroleum Geologists*, Vol. 50 (1966), 2001–08. Hubbert also notes the theoretical possibility of multiple peaks, but does not address the issue in his empirical work: Hubbert, op. cit. note 25, Figure 24b.

69. W. Stannage, 'Resource Estimates and Politics Don't Mix', *World Oil*, Vol.

189 (1979), 113–18.

70. Collins (1981), op. cit. note 3.

71. Collins (1975), op. cit. note 3.

72. Statement made by Senator Hubert Humphrey in Joint Economic Committee, op. cit. note 65, 1.

73. This view was also held by 'independent' estimators — that is, those employed in universities and foundations. See, for example, B. Netschert, *The Future Supply of Oil and Gas* (Baltimore, Md: Johns Hopkins University Press, 1958). A storehouse view consistent with the industry cost-price view, in that it foresaw no immediate physical limitations on production, was held by the experts within the government. These groups, however, were not crucial to the reconstruction of meaning that occurred and, hence, to chronicle their precise perspectives would merely clutter this paper. It should be noted that the present analysis contends that the positions documented for particular individuals are representative of a position shared by others employed in similar types of organizations at that time. Documentation of this contention extends beyond the bounds of this paper: see Bowden (1983), op. cit. note 19.

74. For an early (and highly influential) treatment from this perspective, see IPAA, op. cit. note 34.

75. See, for example, L. Weeks, 'Fuel Reserves of the Future', *Bulletin of the American Association of Petroleum Geologists*, Vol. 42 (1958), 431–41.

76. For an expanded treatment of these themes, see Bowden (1983), op. cit. note 19.

77. The best single source on US government energy policy is the series of articles in C. Goodwin (ed.), *Energy Policy in Perspective* (Washington, DC: Brookings Institute, 1981).

78. National Petroleum Council, *US Energy Outlook, 1971–1985* (Washington, DC: NPC, 1971).

79. Among the best books on the historical changes in the political economy of the international oil industry are the following: F. Al-Chalabi, *OPEC and the International Oil Industry: A Changing Structure* (Oxford: Oxford University Press, 1980); J. Blair, *The Control of Oil* (London: Macmillan, 1976); N. Jacoby, *Multinational Oil* (New York: Macmillan, 1974); P. Odell, *Oil and World Power: A Geographical Interpretation* (Harmondsworth, Middx: Penguin, 6th edn, 1981); A. Sampson, *The Seven Sisters: The Great Oil Companies and the World They Shaped* (New York: Bantam, 1975); and Tanzer, op. cit. note 10.

80. Gillette (op. cit. note 67) claims that Moody's estimates contradict industry interests by discouraging the type of exploration incentive advocated in the 1971 National Petroleum Council report, since his estimate suggests that exploration incentives will not have a substantial impact upon increased discoveries or production. Gillette fails to note, however, that other events had already destroyed the credibility of the NPC report's arguments for concessions.

81. J. Moody and R. Geiger, 'Petroleum Resources: How Much Oil and Where?', *Technology Review*, Vol. 77, No. 5 (March/April 1975), 45.

82. For an overview of the literature dealing with organizational cultures, see C. Lammers and D. Hickson (eds.), *Organizations Alike and Unalike: National and International Studies in the Sociology of Organizations* (London: Routledge and Kegan Paul, 1979). Recent laboratory studies have documented the existence of localized cultures of scientific practice: see, for example, Latour and Woolgar, op. cit.

note 8, or Lynch, op. cit. note 8. For an illustration of the theoretical and empirical linkages between the two types of culture, see R. Anderson, 'The Necessity of Field Methods in the Study of Scientific Research', in E. Mendelsohn and Y. Elkana (eds.), *Sciences and Cultures, Sociology of the Sciences Yearbook*, Vol. 5 (Dordrecht and Boston, Mass.: Reidel, 1981), 213–44.

83. An examination of the relationship between national political-economic context and the local culture of a physics research institute can be found in R. Anderson, *Building Scientific Institutions in India: Saha and Bhabha* (Montreal: McGill University Center for Developing Area Studies, 1975).

84. Perrow, op. cit. note 10.

Gary Bowden is a PhD candidate at the University of Calgary. In addition to research for his dissertation, *The Social Basis of Petroleum Resource Assessment*, he is currently studying the relationship between political jurisdiction and the diffusion of coalmining technology in Western Canada. *Author's address:* Department of Sociology, McMaster University, 1280 Main Street West, Hamilton, Ontario L8S 4M4, Canada.