KEEPING 'JUNK' HISTORY, PHILOSOPHY AND SOCIOLOGY OF SCIENCE OUT OF THE COURTROOM: PROBLEMS WITH THE RECEPTION OF *DAUBERT v MERRELL DOW PHARMACEUTICALS INC*[*]

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I. INTRODUCTION

The difficulties in integrating the practices of law and science have long been acknowledged.[1] In 1993, the US Supreme Court produced a judgment which appears to have radically transformed the admission of expert opinion in United

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1 L Hand, "Historical and Practical Considerations Regarding Expert Testimony" (1901) 15 *Harvard Law Review* 40. This claim is ubiquitous and has a long lineage in the relevant literatures. Detailed contemporary scholarship has questioned the exclusiveness of the two categories and noted that they are not rigorously separated in all circumstances. See R Smith and B Wynne (eds), *Expert Evidence: Interpreting Science in the Law*, Routledge (1989) and S Jasanoff, *Science at the Bar: Law Science, and Technology in America*, Harvard University Press (1995).

States federal courts. In *Daubert v Merrell Dow Pharmaceuticals*[2] the previous reliance by US courts upon scientific community acceptance for determining appropriate standards for the admission of scientific knowledge claims was replaced by a series of considerations which are primarily concerned with assessing the 'internal' practices of science. The new emphasis seems to concentrate upon an evaluation of scientific method by judges. *Daubert* involves the Supreme Court appropriating a questionable and highly contentious philosophy of science to assist in the determination of 'proper' scientific practice. Such an approach appears to be a response to two factors. First, a perceived rise in complex tort litigation with the attenuated difficulties of proving causation in conjunction with the admission of new types of psychological and psychiatric disorders; and second, a rise in the number of notorious trials involving highly publicised expert controversy in the United Kingdom, the United States and Australia. *Daubert* purports to provide a mechanism for maintaining a judicial 'supervision/surveillance' over knowledge claims which do not meet the lofty ascription of science. Indeed there are a number of 'groups' in the US and Australia who are alarmed by the 'increase' in supposedly dubious types of knowledge entering our courts under the aegis of science or in some scientific (dis)guise. *Daubert* and the image of science it legitimates appear to support particular social and political visions. This particular ordering has been an ongoing feature of the 'junk' science debate, especially in the US.[3]

Numerous commentators have discussed the implications of the *Daubert* judgment.[4] Literally hundreds of academic articles have been written with some

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2 61 USLW 4805 (1993); 113 S Ct 2786 (1993).

3 WP Huber, *Galileo's Revenge: Junk Science in the Courtroom,* Basic Books (1991) p 3. Huber was largely responsible for the creation of a perceived crisis over the alleged existence and use of junk science in courts. He describes 'junk science' as "a catalog of every conceivable kind of error: data dredging, wishful thinking, truculent dogmatism, and, now and again, outright fraud". See also DE Bernstein, "Junk Science in the United States and the Commonwealth" (1996) 21 *Yale Journal of International Law* 123.

4 A sample includes: R Allen, "Expertise and the *Daubert* Decision" (1994) *Journal of Criminal Law & Criminology* 1157; D Bernstein, "The Admissibility of Scientific Evidence After *Daubert v Merrell Dow Pharmaceuticals Inc*" (1994) 15 *Cardozo Law Review* 2139; B Black, "The Supreme Court's View of Science: Has *Daubert* Exorcised the Certainty Demon?" (1994) 15 *Cardozo Law Review* 2129; B Black, F Ayala and C Saffran-Brinks, "Science and the Law in the Wake of *Daubert*: A New Search for Scientific Knowledge" (1994) 72 *Texas Law Review* 715; K Chesebro, "Taking *Daubert's* 'Focus' Seriously: The Methodology/Conclusion Distinction" (1994) 15 *Cardozo Law Review* 1745; M Farrell, "*Daubert v Merrell Dow Pharmaceuticals Inc.* Epistemology and Legal Process" (1994) 15 *Cardozo Law Review* 2183; I Freckelton, "Contemporary Comment: When Plight Makes Right - The Forensic Abuse Syndrome" (1994) 18 *Criminal Law Journal* 29; P Gianelli, "*Daubert*: Interpreting the Federal Rules of Evidence" (1994) 15 *Cardozo Law Review* 1999; C Hutchinson and D Ashby, "*Daubert v Merrell Dow Pharmaceuticals Inc:* Redefining the Bases for Admissibility of Expert Scientific Testimony" (1994) 15 *Cardozo Law Review* 1875; E Imwinkelried, "The Next Step After *Daubert*: Developing a Similarly Epistemological Approach to Ensuring the Reliability of Nonscientific Expert Testimony" (1994) 15 *Cardozo Law Review* 2271, R Jonakait, "The Meaning of *Daubert* and What That Means for Forensic Science" (1994) 15 *Cardozo Law Review* 2103; M Klein, "After *Daubert*: Going Forward with Lessons from the Past" (1994) 15 *Cardozo Law Review* 2219; A Maskin, "The Impact of *Daubert* on the Admissibility of Scientific Evidence: The Supreme Court Catches up with a Decade of Jurisprudence" (1994) 15 *Cardozo Law Review* 1929; J Meaney, "From *Frye* to *Daubert*: Is a Pattern

law journals dedicating entire issues to the discussions of assessment and implications raised. The following discussion paper will critique an important commentary on the likely impact of the *Daubert* decision on Australian law by Odgers and Richardson. Odgers and Richardson enter the field as influential commentators and educators, championing the philosophical position outlined by the Supreme Court in their analysis of the appropriate conditions for the admission of so-called 'scientific evidence' into Australian courts.[5] In so doing they have, appropriately, raised questions surrounding the implications of the interpretation of the US Federal Rules of Evidence for the interpretation of Australian evidence law under the *Evidence Act 1995* (Cth). We agree that the US shift may well have profound implications for Australian law, but we do not share, nor accept as adequate, the representation of science and its attendant methodology as described in *Daubert* and represented by Odgers and Richardson.

We will argue that Odgers and Richardson and the majority *Daubert* court avoid existing debate pertaining to the history and philosophy of science ("HPS") and the sociology of scientific knowledge ("SSK").[6] In particular, we have difficulties with Odgers and Richardson's naive conceptualisation of the law-science relationship which identifies legal distortion of the normal practices of science as a key problem to be solved. In addition, we have difficulty with Odgers and Richardson's uncritical embrace of Karl Popper's doctrine of falsification as one of the key defining features of legitimate scientific practice. In this embrace Odgers and Richardson have either ignored, or are oblivious to, longstanding philosophical criticism and debate concerning the plausibility and applicability of the doctrine of falsification to understanding scientific practice. Apart from failing to recognise falsification as philosophically flawed, Odgers and Richardson provide little evidence of understanding the intricacies involved in the application of the concept. For instance, they use falsification in a number of inconsistent ways when they attempt to apply it to examples where they contend it would resolve the issue of the admissibility of putatively 'scientific' evidence.

Unfolding?" (1995) 35 *Jurimetrics* 191; P Miller, B Rein and E Bailey, "*Daubert* and the Need for Judicial Scientific Literacy" (1994) 77 *Judicature* 254; D Nelkin, "After *Daubert*: The Relevance and Reliability of Genetic Information" (1994) 15 *Cardozo Law Review* 2119; B Scheck, "DNA and *Daubert*" (1995) 15 *Cardozo Law Review* 1959; RC Dreyfuss, "Is Science a Special Case? The Admissibility of Scientific Evidence After *Daubert v Merrell Dow*" (1995) 73 *Texas Law Review* 1779.

5 S Odgers and J Richardson, "Keeping Bad Science Out of the Courtroom: Changes in American and Australian Expert Evidence Law" (1995) 18 *University of New South Wales Law Journal* 108. For a similar perspective see J Richardson, G Ginsburg, S Gatowski and S Dobbin, "The Problems of Applying *Daubert* to Psychological Syndrome Evidence" (1995) 79 *Judicature* 10; S Gatowski, S Dobbin, J Richardson, C Nowlin and G Ginsburg, "The Diffusion of Scientific Evidence: A Comparative Analysis of Admissibility Standards in Australia, Canada, England, and the United States, and their Impact on the Social and Behavioural Sciences" (1996) 4 *Expert Evidence* 86.

6 Jasanoff notes this as a common trend in legal scholarship pertaining to the law-science relationship. See S Jasanoff, "Judicial Construction of New Scientific Evidence" in P Durbin (ed), *Critical Perspectives on Nonacademic Science and Engineering* (1991) 215 at 235 at footnote 4. See also R Allen, "Expertise and the *Daubert* Decision" (1994) 84 *Journal of Criminal Law & Criminology* 1157 at 1168-1175.

Most of the discussion of the implications of *Daubert*, including that produced by Odgers and Richardson, has been flawed because it has relied on a naive positivist image of science as a form of knowledge defined by its possession of a singular, transhistorical and efficacious scientific method and unique institutional structures. Over the past two decades, historians, philosophers and sociologists of science have created a more nuanced vision of science which challenges the accuracy of that image.

Whilst the majority of commentators appear to support the reform outlined in *Daubert*, there has been very little discussion or evaluation of the Supreme Court's proposal at the level of judicial application, despite Rehnquist CJ and Stephens J voicing serious disquiet in their dissent. Similarly there has been almost no evaluation or commentary upon the inherent problems in selecting one narrow philosophy of science as the exemplar of 'proper' or legitimate scientific practice.

II. THE US CASES

Before embarking upon a detailed examination of the article by Odgers and Richardson we will provide an overview of the two approaches to the admission of expert opinion evidence which continue to dominate US Federal and State law.[7] In developing their argument, Odgers and Richardson rely heavily upon developments in US evidence law, especially the recent 'changing of the guard'. *Daubert* effectively replaced the principle derived earlier from *Frye v United States*.[8] The *Frye* case addressed the admissibility of 'novel' scientific evidence. In contrast, the Supreme Court in *Daubert* considered the issue of scientific evidence more comprehensively, offering specific criteria with which to assess whether knowledge claims are entitled to be given the ascription 'scientific' and thus become eligible for reception in courts of law.[9]

A. *Frye v United States*

James Alphonzo Frye was convicted of second degree murder after the judge refused to admit novel evidence from a systolic blood pressure deception test. This test was putatively based upon "scientific experiments" which "it is claimed, have demonstrated that fear, rage, and pain always produce a rise in systolic blood pressure". Following from these premises it was contended that:

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7 Meaney note 4 *supra*, provides a brief 'map' of US states persisting with *Frye* and others which have modified their approach to accommodate the structure of the Federal Rules of Evidence and *Daubert*.

8 293 F 1013, DC Cir (1923).

9 The *Daubert* examination of science represents a radical departure from a judicial evaluation of whether opinions were widely held within expert communities. The approach adopted in *Daubert* extended the scope of the earlier *Frye* test from novel knowledge claims to all forms of scientific knowledge which claimed the ascription 'scientific'. For a more detailed discussion of *Frye* see Jasanoff note 6 *supra*, and P Giannelli, "The Admissibility of Novel Scientific Evidence: *Frye v United States*, a Half-Century Later" (1980) *80 Columbia Law Review* 1197.

> conscious deception or falsehood, concealment of facts, or guilt of crime, accompanied by fear of detection when the person is under examination, raises the blood pressure in a curve which corresponds exactly to the struggle going on in the subject's mind.[10]

The remarkably brief judgment handed down by Van Orsdel AJ contributed the 'general acceptance test' as the basis for the admission of novel expert testimony. The crux of the test is as follows:

> Just when a scientific principle or discovery crosses the line between the experimental and demonstrable stages is difficult to define. Somewhere in the twilight zone the evidential force of the principle must be recognised, and while courts will go a long way in admitting expert testimony deduced from a well-recognised scientific principle or discovery, the thing from which the deduction is made must be sufficiently established to have gained *general acceptance* in the particular field to which it belongs.[11]

The 'general acceptance test' required that to be admissible, expert opinion evidence needed to conform to methods and principles which had received widespread acceptance in a particular field of knowledge. The 'general acceptance test' might be described as an external or extrinsic evaluation of scientific knowledge claims. It makes no attempt to consider the content or the methods of the construction of scientific knowledge adduced, but instead focuses attention upon the extent to which certain opinions are received and accepted within the appropriate profession or scientific community. In contrast, *Daubert* represents a shift toward an internal inspection of the process of the construction of scientific knowledge.[12]

B. *Daubert v Merrell Dow Pharmaceuticals Inc*

Before *Daubert* was decided, US Federal Courts had been divided between those championing the application of the traditional *Frye* principle and those applying the apparently less restrictive Federal Rules of Evidence introduced in 1975, although some states and litigants attempted to read the *Frye* 'general acceptance test' into the Federal Rules of Evidence. These rules allowed, at the discretion of the judge, for the admission of scientific evidence which might not have satisfied the 'general acceptance test'.[13] The entire Court in *Daubert* argued that its authoritative interpretation (for US Federal Courts) of the Federal Rules of Evidence should supersede *Frye*.[14] *Daubert* addresses what had been

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10 Note 8 *supra* at 1013.

11 *Ibid* at 1014 (italics added).

12 In mentioning internal and external categories of science we do not wish to enter extant historiographical debates. There is an old historiographical debate in the field of the history and philosophy of science concerning the constituting influences in the development of science. Externalists preferred societal and economic explanations whereas internalists, who were principally historians of ideas, preferred to examine the scientific and intellectual traditions and assumptions for the explanation of the development of modern science. Those interested in the debate could refer to B Barnes, *Scientific Knowledge and Sociological Theory*, Routledge and Kegan Paul (1974) Chapter 5. See also G Basalla, *The Rise of Modern Science: External or Internal Factors*, Heath (1968).

13 C Zatz, "Defenses on the Frontiers of Science" (1992) 19 *Litigation* 13 at 15.

14 For an overview of the US states currently adopting *Frye* or some analogue to the Federal Rules interpreted in *Daubert* see Meaney note 4 *supra*.

recognised as a problem in evidence law: "determining when a 'scientific principle or discovery' actually is 'scientific'".[15]

The case of *Daubert* required the Supreme Court to determine the admissibility of expert evidence relating to the role of Bendectin in the aetiology of birth defects.[16] The appeal to the Supreme Court was the culmination of attempts to have expert scientific evidence, which was deemed not to comply with the *Frye* 'general acceptance' test, heard under the apparently 'less stringent' Federal Rules of Evidence. Below are the relevant sections of the Federal Rules of Evidence directly concerned with expert opinion evidence:

> Rule 702: If scientific, technical, or other specialised knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or by education, may testify thereto in the form of an opinion or otherwise.

And,

> Rule 703: The facts or data in the particular case upon which an expert bases an opinion or inference may be those perceived by or made known to the expert at or before the hearing. If of a type reasonably relied upon by experts in the particular field in forming opinions or inferences upon the subject, the facts or data need not be admissible in evidence.

The issue raised for consideration in *Daubert* was whether a small group of "impressive[ly] credential[ed]" scientists and doctors could present evidence which seemed to be scientific but was of too recent origin to have been generally accepted. The Supreme Court ruled unanimously that the *Frye* general acceptance test had been superseded by the Federal Rules of Evidence.

Despite unanimity in overruling *Frye,* there was strong division between a large majority and the joint judgment of Rehnquist CJ and Stephens J over the implications and interpretation of the new rules. From the enthusiastic tone of Odgers and Richardson's article one might think that there had been no serious dissent or problematisation of the apparently clear majority judgment.[17]

(i) Majority

The majority judgment delivered by Blackmun J, joined by White, O'Connor, Scalia, Kennedy, Souter and Thomas JJ, placed two criteria on the admissibility of expert evidence under Rule 702. Those criteria are relevance and reliability.[18] Relevance was given by Rule 402 as:

> ...all relevant evidence is admissible, except as otherwise provided by the Constitution of the United States, by Act of Congress, by these rules, or by other

15 I Freckelton, *The Trial of the Expert*, Oxford University Press (1987) p 172.

16 L Lasagna and SR Shulman, "Bendectin and the Language of Causation" in KR Foster, DE Bernstein and PW Huber (eds), *Phantom Risk: Scientific Inference and the Law* (1993). Previously Bendectin held a contentious court record, no doubt enhanced by the infamous scientific 'fraud' of Dr William McBride. See also Huber note 3 *supra*.

17 When mentioning Chief Justice Rehnquist and Justice Stephens' dissent, Odgers and Richardson describe it as merely a pedagogical concern, rather than a legitimate problem associated with the majority's philosophy of science. Odgers and Richardson note *supra* 5 at 121.

18 Supreme Court note 2 *supra* at 2795.

rules prescribed by the Supreme Court pursuant to statutory authority. Evidence which is not relevant is not admissible.

The majority believed that the determination of relevance could be assisted by the common law even though they acknowledged that the Federal Rules covered the field. For the remainder of this article it is our intention to focus primarily upon reliability because, unlike relevance, it is not entirely determined *in situ*.

In their discussion of reliability the majority reinforced and justified their shift from *Frye*, noting that nothing in Rule 702 establishes "'general acceptance' as an absolute prerequisite to admissibility".[19] The displacement of the *Frye* rule purported to shift the 'screening role' of 'gate-keeper' from the scientific community to the judge. The majority introduced judge based determinations for not only relevance, but significantly, the reliability of specific scientific claims. The "judge must ensure that any and all scientific testimony and evidence admitted is not only relevant, but reliable".[20]

The reliability of 'scientific knowledge' which the majority acknowledged did not equate with certitude could, they argued, be assessed using a number of indicators. The four non-exhaustive and flexible means they suggested for assessing whether a theory or technique is scientific are: whether the claims can and have been tested (falsified); whether the theory or technique has been subjected to peer review and publication; the known or potential rate of error; and whether there has been 'general acceptance' within a relevant scientific community.

The feature which the Supreme Court postulated as ordinarily distinguishing science from most other forms of inquiry was the ability to test or falsify theories. This criterion, especially its relevance and application, is the key to understanding our criticisms of Odgers and Richardson. Indeed the following passage was described by Odgers and Richardson as the "key passage of *Daubert*":[21]

> Ordinarily, a key question to be answered in determining whether a theory or technique is scientific knowledge that will assist the trier of fact will be whether it can be (and has been) tested. "Scientific methodology today is based on generating hypotheses and testing them to see if they can be falsified; indeed, this methodology is what distinguishes science from other fields of human inquiry." Green at 645. See also C Hempel, Philosophy of Natural Science 49 (1966) ("[T]he statements Conjectures and Refutations: The Growth of Scientific Knowledge 37 (5th ed 1989) ("[T]he criterion of the scientific status of a theory is its falsfiability, or refutability, or testability").[22]

19 *Ibid* at 2794. Others have made the point that there is a difference between the accepted reliability of a type of test or practice however novel and the particular application of that test or process by one or more practitioners. For example J Bourke, "Misapplied Science: Unreliability in Scientific Test Evidence - Part I" (1993) 10 *Australian Bar Review* 123 at 127; L Clark, "The Scientist as Expert Witness" (1990) 22 *Australian Journal of Forensic Science* 68 at 74.

20 Note 2 *supra* at 2795.

21 Odgers and Richardson note 5 *supra* at 115.

22 Note 2 *supra* at 2796-7 quoting from M Green, ìExpert Witnesses and Sufficiency of Evidence in Toxic Substances Litigation: The Legacy of *Agent Orange* and *Bendectin* Litigation" (1992) 86 *Northwestern University Law Review* 643 at 645; C Hempel, *Philosophy of Natural Science*, Prentice-Hall (1966) p 49; KR Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, Routledge and Kegan Paul (1963) p 37. It should be noted that of the many texts and articles cited in the judgment,

The majority emphasised that "the focus of course, must be solely on principles and methodology of a science, not on the conclusions they generate".[23] The absence of fuller explanation of the supposed internal mechanism routinely involved in the practice of the sciences was noted by Rehnquist CJ and Stephens J in their dissenting judgment.

The second of the factors the US Supreme Court suggested could be used in assessing the reliability of scientific knowledge was whether the theory or technique had been subjected to peer review and publication. The majority accepted that publication is not a "*sine qua non* of admissibility" and they acknowledged that it "does not necessarily correlate with reliability".[24] This is why the category 'publication' is linked to peer review - publication reputedly affords some type of enhanced scrutiny of scientific claims.[25] "Submission to the scrutiny of the scientific community is a component of 'good science', in part because it increases the likelihood that substantive flaws in methodology will be detected."[26]

Thirdly, the court discussed the known or potential error rate. The final non-definitive factor was that the Court could consider the 'general acceptance' of a theory or technique. The majority cited *United States v Downing*[27] to describe the relation between general acceptance and reliability:

A 'reliability assessment does not require, although it does permit, explicit identification of a relevant scientific community and an express determination of a particular degree of acceptance within that community.'[28]

The majority also attempted to allay any concern over the new interpretation of the rules inaugurating a "free-for-all" in which all manner of "absurd and irrational pseudo-scientific assertions" would be entertained.[29] A combination of faith in the jury and the adversary system with its own "testing" of evidence through "vigorous cross-examination" was seen to provide the necessary safeguards to protect against gratuitous knowledge claims gaining admission.[30] The majority in *Daubert* acknowledged that both *amicus* briefs and the petitioners exhibited disquiet in relation to the introduction of a more pro-active screening role for the judge. They were concerned that such a process might not only prevent new forms of "questionable" knowledge from entering the courtroom but would also "sanction a stifling and repressive scientific orthodoxy and will be inimicable to the search for truth".[31] The majority conceded that regardless of how flexible the criteria available to the judge in their capacity as

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only Farrell offers any summary or consideration of the work of Carl Hempel; see Farrell note 4 *supra* at 2190. Also Green merely reinforces the status of Popper's falsifiability and offers nothing new.

23 *Ibid* at 2797. For an extended consideration of this distinction see K Chesebro note 4 *supra*.

24 *Ibid.*

25 See the eight factors discussed in "Conclusion: Phantom Risk - A Problem at the Interface of Science and Law" in Foster, Bernstein and Huber note 16 *supra* at 431-43.

26 Note 2 *supra* at 2797.

27 753 F 2d (1985) 1224 at 1238.

28 Note 2 *supra* at 2797.

29 *Ibid* at 2798.

30 *Ibid.*

31 *Ibid.*

"gatekeeper", it will "inevitably on occasion...prevent the jury from learning of authentic insights and innovations".[32] In defending their position, the majority sought to distinguish the pragmatic and provisional requirements of law with the ongoing development of science:

> That, nevertheless, is the balance that is struck by Rules of Evidence designed not for the exhaustive search for cosmic understanding but for the particularised resolution of legal disputes.[33]

(ii) Minority

In contrast to the confidence exhibited by the majority, the dissentients Rehnquist CJ and Stevens J were far less sure of the utility of the majority judgment's recourse to the philosophy of science. In a short judgment Rehnquist CJ quickly drew attention to differences between the issues in *Daubert* and other cases:

> The various briefs filed in this case are markedly different from typical briefs, in that large parts of them do not deal with decided cases or statutory language - the sort of material we customarily interpret. Instead they deal with definitions of scientific knowledge, scientific method, scientific validity, and peer review - in short, matters far afield from the expertise of judges...it is to say that the unusual subject matter should cause us to proceed with great caution in deciding more than we have to, because our reach can so easily exceed our grasp.[34]

Chief Justice Rehnquist voiced alarm at the requirement that judges make practical use of the philosophical models to determine acceptable scientific evidence which had been proposed by the majority. His Honour Rehnquist was willing to accept some type of gatekeeping responsibility, indeed this had existed from at least the time of *Frye*. But that responsibility was to be duly circumscribed:

> I do not think it [Rule 702] imposes on them [judiciary] either the obligation or the authority to become amateur scientists in order to perform that role.[35]

Chief Justice Rehnquist implied that his own ignorance of the philosophy and practice of science would be shared by other judges in US Federal Courts:

> I defer to no one in my confidence in federal judges; but I am at a loss to know what is meant when it is said that the scientific status of a theory depends on its "falsifiability," and I suspect some of them will be, too.[36]

In summary, the recent shift in US evidence law derived from *Daubert* has transformed the criteria for admissibility of scientific knowledge claims, from

32 *Ibid* at 2798-9.

33 *Ibid* at 2799.

34 *Ibid.*

35 *Ibid* at 2800.

36 *Ibid.* Also see: AG Gless, "Some Post-*Daubert* Trial Tribulations of a Simple Country Judge: Behavioural Science Evidence in Trial Courts" (1995) 13 *Behavioural Sciences and the Law* 261; D Elliot, "Toward Incentive-Based Procedure: Three Approaches for Regulating Scientific Evidence" (1989) 69 *Boston University Law Review* 487 at 500-1, 511; R Schwartz, "There is no Archbishop of Science - A Comment on Elliot's *Toward Incentive-Based Procedure: Three Approaches for Regulating Scientific Evidence*" (1989) 69 *Boston University Law Review* 517 at 517-18, 520, 525; J Thames, "It's Not Bad Law - It's Bad Science: Problems with Expert Testimony in Trial Proceedings" (1995) 18 *American Journal of Trial Advocacy* 544 at 562. Contrast Dreyfuss note 4 *supra* at 1791.

one based on an expert community's 'general acceptance' of a particular theory to a focus upon the internal practices of the various sciences in combination with certain professional checks and balances such as peer review and error rate in conjunction with consideration of 'general acceptance'. This shift has transformed the role of the judge from relatively passive assessor to an active inquisitor searching for the underlying essence of scientific knowledge claims. Despite their apparently more liberal criteria, the Federal Rules of Evidence, as interpreted in *Daubert*, particularly through the criteria of reliability, may actually be more restrictive in the types of expertise that they allow into court than the 'conservative' *Frye* test.[37] A number of US and Australian authors express hope that this attention to the internal processes of science will provide a means of eliminating supposedly non-scientific knowledge claims which have been couched in scientistic jargon and given access to the courts. We contend that these appraisals, with which Odgers and Richardson appear to agree, reflect a push by the US science and industry lobby to remove the access, of what they have labelled 'junk science', to the courtroom. However, it is strongly argued that, the reliability tests created by *Daubert* will, over time, become as open to interpretation and manipulation as the *Frye* test.

III. IMPLICATIONS FOR THE AUSTRALIAN LEGAL CONTEXT

US jurisprudence is often influential, though not in a formal sense, in the development of Australian law. For example, our own Constitution reveals certain similarities with its US counterpart,[38] and indeed the recent reform to Australian evidence law indicates resemblances - including those provisions pertaining to the admission of expert evidence to the US Federal Rules of Evidence. The combination of new statute based evidence law for the Commonwealth and New South Wales, in conjunction with the US Supreme Court judgment in *Daubert*, has generated interest in the eventual interpretation of the new Australian legislation. One important concomitant is whether Australian courts will or should follow the US lead. Another is the degree of

37 Freckelton note 15 *supra* at 168-169; Miller, Rein and Bailey note 4 *supra* at 255: "The Federal Rules eschewed 'rigid' tests like *Frye* in favour of a 'liberal' approach to admissibility...". There is disagreement over whether the shift from *Frye* to *Daubert* actually represents a tightening or loosening of the entry requirement for expert scientific evidence. See D Bernstein note 4 *supra* at 2139: "While some analysts have argued that *Daubert* will encourage the trend toward more careful judicial scrutiny of scientific evidence, others have contended that the opinion will reduce the role of the courts in screening scientific evidence and permit a flood of junk science evidence into American courtrooms." The various positions championed seem to be divided roughly as follows: (a) *Frye* is adequate as a screening mechanism but was not consistently applied by US courts; or (b) *Frye* was inadequate as a screening mechanism; or (c) *Daubert* makes the Federal Rules of Evidence too restrictive (conservative) and burdensome, especially for plaintiffs; or (d) *Daubert* is restrictive but allows for the admission of new scientific claims if they conform to the Supreme Court's image of scientific method.

38 H Gibbs, "Separation of Powers - A Comparison" (1987) 17 *Federal Law Review* 151. This tendency can be noted in areas such as environmental law.

divergence between *Frye* and *Daubert* and current Australian evidence law.[39] The potential significance of *Daubert* has been noted by a prominent Australian evidence scholar and barrister, Ian Freckelton: "Such an approach may well, in time, make its way into Australian law".[40] These sentiments are shared by Odgers and Richardson:

> *Daubert* will reinforce the trend of authorities in Australia...importantly, the criteria articulated by the majority in *Daubert* in assessing reliability are likely to be utilised by Australian Courts.[41]

The relevance of the *Daubert* decision may have been enhanced by the recent enactment of the *Evidence Act* 1995 (Cth) and the *Evidence Act* 1995 (NSW), which contain what Odgers and Richardson regard as "deceptively simple provisions with respect to expert evidence" bearing a close resemblance to the US legislation.[42] The Australian provision, s 79 of the *Evidence Act* 1995 (Cth) is as follows:

> If a person has specialised knowledge based on the person's training, study or experience, the opinion rule does not apply to evidence of an opinion of that person that is wholly or substantially based on that knowledge.[43]

Contrast the Australian provision with the American provisions, Rule 702, extracted at Part IIB above. Rule 702 makes no reference to a field of expertise nor a designated standard of specialised knowledge. There is no specific reference to scientific knowledge in the Australian legislation but rather it applies to 'specialised knowledge'. Whether a provision as broad as s 79 will encourage Australian judges to attempt to characterise 'science' or scientific knowledge remains to be seen. Some US commentators have pondered, post *Daubert*, whether the US Supreme Court will split the phrase, "scientific, technical, or other specialised knowledge", creating a weighted epistemological hierarchy for admissible evidence.[44] Such a move would elicit tensions as

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39 In a recent comment on the Canadian case *R v Mohan* (1994) 29 CR (4th) 243, Delisle suggests that, "the court [Canadian Supreme Court] in *Mohan* appears to have arrived at the same position as the court in *Daubert*, though with decidedly less fanfare". For a discussion of the Canadian position see RJ Delisle, "The Admissibility of Expert Evidence: A New Caution Based on General Principles" (1994) 29 *Criminal Reports* 267 at 272. See also DM Paciocco, *Evaluating Expert Opinion Evidence for the Purpose of Determining Admissibility: Lessons from the Law of Evidence*, National Judicial Institute [Canada] (1995); B McLachlin, *The Use and Misuse of Expert Evidence*, National Judicial Institute [Canada] (1995).

40 Freckelton note 15 *supra* at 45.

41 Odgers and Richardson note 5 *supra* at 124, 129: "*Daubert* represents a potential revolution"; Richardson, Ginsburg, Gatowski and Dobbin note 5 *supra* at 16, employ an identical descriptive vocabulary.

42 Odgers and Richardson *ibid* at 109.

43 The Australian relevancy provisions are represented by the *Evidence Act* 1995 (Cth), s 135.

44 S Jasanoff, "What Judges Should Know About the Sociology of Science" (1993) 77 *Judicature* 77. Jasanoff raises this point and it is developed in the following cases: *People v Young*, 391 NW 2d 270 (1986) at 274-5; *People v Reilly*, 242 Cal Rptr 496 (1987) at 503-4 cited in footnote 18. See also Imwinkelried note 4 *supra*. This point is raised by Imwinkelried in his discussion of scientific and non-scientific evidence. Interestingly (and surprisingly) Imwinkelried at 2276-2277 employs a strange and idiosyncratic view of the history of science where the 'scientific method' was created by Locke and Newton. Imwinkelried at 2290 suggests that courts could look to Hume for the basis of admission of non-scientific expert evidence.

different and possibly competing bodies of knowledge, such as scientific and non-scientific specialties, would somehow have to receive differently weighted evaluation from a judge and/or jury. In such circumstances, the status of knowledge claims would be unclear. It would be possible that purportedly 'scientific' claims might be excluded for not meeting the *Daubert* standard or some equivalent. Meanwhile evidence presented below the lofty scientific threshold as 'specialised' or 'technical', though not susceptible to testing or not complying with *Daubert* might be admitted unimpugned.

At present, there is no Australian equivalent to *Daubert's* methodological prescriptions. Current Australian law involves external evaluation rather than an internal examination of the processes of science. Some commentators, such as Freckelton, note the differences between the Australian common law and the *Frye* test. Contrary to Freckelton, we would suggest that they bear a relatively close resemblance.[45] The Australian approach has been to assess the existence of a 'field of expertise' and the qualifications and/or experience of the particular expert.[46] Whilst distinguishable from *Frye*, this approach bears certain structural similarities. Aronson and Hunter, in their leading Australian evidence text, contend that there are many Australian analogues to *Frye*:

> Established disciplines tend to produce their own criteria for recognition of a person claiming specialist knowledge based on study or experience. Peer recognition in such disciplines will generally guarantee court recognition of the witnesses's expertise...[47]

Legal categories such as 'field of expertise', 'general acceptance' and 'expert qualifications' are inextricably interdependent.

The Australian common law has traditionally emphasised individual credibility and judges have tightly controlled the demarcation of fields of expertise as a means of restricting the admission of certain types of evidence. For instance, the requirement of a field of expertise and qualifications pertinent to expertise in that field usually requires formal training and practice. Whilst we would acknowledge that such a test might be broader than a restrictive application of the *Frye* criterion, the Australian position more closely resembles *Frye* than the *Daubert* examination of internal methodological practice(s) of science. The application of *Daubert* to Australian contexts would represent a significant conceptual shift.

The *Frye* test and the more recent *Daubert* judgment have generated interest from Australian evidence commentators.[48] *Frye* has received occasional recognition from the Australian bench.[49] As yet there has been no public consideration of the *Daubert* judgment. The position is somewhat different

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45 Freckelton note 15 *supra* at 60.

46 *Casley-Smith v FS Evans & Sons Pty Ltd* (1988) 49 SASR 314

47 M Aronson and J Hunter, *Litigation: Evidence and Procedure*, Butterworths (5th ed, 1995) p 965

48 Bourke note 19 *supra* at 145. Bourke claims that: "The American approach is mentioned because many Australian lawyers think the *Frye* test is either part of Australian common law or, if not, it should be".

49 For example: *R v Gilmore* [1977] 2 NSWLR 935 at 940, per Street CJ; *Runjanjic v The Queen* (1991) 56 SASR 114 at 119, per King CJ; *R v Lewis* (1987) 29 A Crim R 267 at 269, per Maurice J.

outside the courts. In a recent article, Freckelton invoked *Daubert* as the appropriate standard by which 'science' should be evaluated:[50]

> The *Daubert* criteria focus upon the falsifiability or refutability of scientific evidence and upon the known or potential rate of error and the existence and maintenance of standards. To date, calculation of error rates, application of predictive tests, the use of control samples and assessment of falsifiability of outcomes are concepts that have not been applied to syndrome theories. Until they are, syndrome evidence does not deserve a place on the expert evidence shelf comparable to that occupied by PGM testing, gunshot residue analysis or fingerprinting.[51]

In contrast to Freckelton (and Odgers and Richardson), Aronson and Hunter find the Supreme Court judgment less convincing. In a rather circumspect reference they acknowledged that falsification - upon which *Daubert* is largely predicated - is "a criterion roundly criticised by many as exhibiting ignorance of scientific theory since Popper".[52]

As in the US, much of the Australian concern with expert opinion evidence is apparently motivated by concern about potential miscarriages of justice via the admissibility of syndrome evidence and difficulties in establishing causation in tort litigation.[53] A number of highly publicised scientific-legal 'failures' involving forensic science, new areas of technical expertise and the perceived increase of scientifically dependent litigation have problematised the relationship of the scientific expert with the court.

Freckelton has voiced these concerns in the following way:

> It is not overstating the situation to say that a welter of new scientific techniques and theories threaten to besiege the courts in Australia and England. The attempts to introduce them in evidence in the United States has caused enormous controversy.[54]

Freckelton identifies the case of syndrome evidence as an example of a "forensic growth industry".[55] He perceptively acknowledges that:

> ...to a considerable degree, the syndromes represent a medical fiction constructed to deal with a stance of the law that insists upon supposedly objective notions of ordinariness and reasonableness.[56]

Freckelton appears to provide a 'platform' for the concern over reliance upon syndrome evidence subsequently expressed in the paper by Odgers and Richardson. He suggests that:

> The difficulty is that if syndrome evidence were subjected to the rigorous indicia specified by the majority in *Daubert v Merrell Dow Pharmaceuticals* to determine whether scientific evidence is sufficiently reliable to be admitted, it should fail.[57]

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50 Freckelton note 4 *supra*.

51 *Ibid* at 43.

52 Aronson and Hunter note 49 *supra*, p 965. Possibly the strongest version of the historical support for *Daubert* comes from Imwinkelried note 4 *supra* at 2277.

53 Examples from environmental tobacco smoke litigation and Electric and Magnetic Fields ("EMF") litigation include: *Tobacco Institute of Australia v Australian Federation of Consumer Organisations Inc* (1992) 111 ALR 61 at 118, per Hill J;
Warren v Electricity Commission of NSW, (unreported, Land and Environment Court of New South Wales, Cripps J, 31 October 1990). See also Miller, Rein and Bailey, note 4 *supra* at 254.

54 Note 15 *supra* at 165.

55 Note 4 *supra* at 32.

56 *Ibid* at 42.

In his earlier work, *The Trial of the Expert,* Freckelton identified the engagement of scientific expertise as one of the most pernicious and protracted issues for the legal system. He acknowledged the difficulty experienced by judges and juries in "distinguishing properly and accurately among a plethora of confusing and contradictory expert evidence led by the Crown and the defence".[58] He asserted that the "courtroom is not the proper venue for resolution of scientific debate".[59] By the time of writing a subsequent article, "When Plight Makes Right - The Forensic Abuse Syndrome",[60] Freckelton, in line with the position subsequently endorsed by Odgers and Richardson, emerged supporting the majority position advocated in *Daubert*. In the interim, the judiciary had emerged as the appropriate mechanism for filtering the admission of knowledge claims.

Some of Freckelton's views, though by no means all, are shared by Peter Huber, a highly influential commentator from the United States.[61] In his controversial book, *Galileo's Revenge*, Huber decries the leniency with which the *Frye* rule was applied in US courts from the 1970s.[62] In colourful, though disturbing language, Huber blames Charlatans, Calabresians[63] and lawyers for the introduction of quackery and pseudo-science into the courtroom:

> This was especially galling because the legal community's pessimism about technology was matched (paradoxically) by its optimism about liability science.[64]

Huber is very critical of lawyers assuming stewardship over science. Strict application of *Frye* "threatened to cut short the great Calabresian search for cheap, wide-ranging control".[65] Huber deplores the easy access which so-called experts have to US courts. He argues that by the 1980s "countless courts had opened their doors wide to claims based on methods and theories not generally

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57 *Ibid* at 43, 47. Freckelton also claims that various syndromes are not areas of expertise and "should fail the *Merrell Dow* test". However there is no requirement for "areas of expertise" from the *Daubert* judgment.

58 Freckelton note 15 *supra* at 151.

59 *Ibid* at 163, 168. Freckelton cited the Maryland Court of Appeals in *Reed v State* 283 Md. 374; 391 A. 2d 364 (1978), which determined that "judges and juries are not equipped to assess the reliability of scientific techniques when scientists are disagreeing on the issues...".

60 Note 4 *supra*.

61 PW Huber, "Medical Experts and the Ghost of Galileo" (1991) 54 *Law and Contemporary Problems* 119; "Peter Huber" (1994) 29 *Educom Review* 29 at 30. For a perspective on Huber's influence in the US tort reform debate and critique of Huber's work see: K Chesebro, "Galileo's Retort: Peter Huber's Junk Scholarship" (1993) 42 *American University Law Review* 1637 at 1644-1650; R Hayden, "Neocontract Polemics and Unconscionable Scholarship" (1990) 24 *Law & Society Review* 863; G Edmond and D Mercer, "Trashing Junk Science: Peter Huber and the Cult of 'Junk Science'" (1996) (unpublished manuscript).

62 Note 3 *supra*.

63 Huber was referring to the proponents of Yale law professor Guido Calabresi's *The Costs of Accidents: A Legal Economic Analysis*, Yale University Press (1970). Huber's brief review of Calabresi's work runs as follows: "Accidents are costly...Liability's principal purpose should be to control their costs efficiently. The common law should be, above all, a far-reaching instrument of social control. And the most efficient way to control the costs of accidents is to charge each to the person who might have prevented it most cheaply...the 'cheapest cost avoider'". Huber note 3 *supra* at 11.

64 *Ibid* at 15.

65 *Ibid.*

accepted as reliable by any scientific discipline".[66] In later works, Huber again expresses concern at such issues as the flexibility of admission of expert evidence, the partisanship of witnesses, the huge financial stakes involved in litigation and laxity in the consideration and evaluation of scientific evidence generally. He also provides qualified support for *Daubert's* prescriptions for 'good' science.[67]

It is from this Anglo-American politically conservative response[68] to recent judicial and legislative trends that the article by Odgers and Richardson emerges. The shift from external to internal examination of science extends judicial authority concerning the admissibility of evidence from interpreting when there is a consensus over what constitutes valid science according to a relevant community of scientists, to a decision based upon conformity to a particular philosophy of science deemed acceptable by the legal system. Superficially, this shift appears to inaugurate a new era of more rigorous and active restriction upon the admissibility of expertise.

With these developments in mind we will now turn our attention to an examination of that article, expansive and celebratory in embracing the US Supreme Court interpretation of science.

IV. ODGERS AND RICHARDSON, *DAUBERT* AND "KEEPING BAD SCIENCE OUT OF THE COURTROOM"

The article by Odgers and Richardson provides a commentary on the developments in US expert evidence law and the implications that these US developments have for the future admissibility of various forms of legitimate ('good') and illegitimate ('bad' or 'junk') science in Australian courts. Odgers and Richardson believe that *Daubert* provides an important opportunity to clarify and improve the rules governing the admissibility of expert evidence in the Australian legal system. This belief hinges on their uncritical acceptance of the legitimacy of the philosophy of science underpinning the judgment.

Odgers and Richardson lament the perceived reluctance of the Australian High Court to "establish *the* applicable principles" for the admission of expert opinion comprehensively and restrictively.[69] Odgers and Richardson suggest that the flexibility in the rules for the admission of expert evidence, combined with a general judicial ignorance of science, has contributed to the "admission of

66 *Ibid* at 17.

67 Foster, Bernstein, Huber note 16 *supra* at 433-443. See also KR Foster, DE Bernstein and PW Huber, "Science and the Toxic Tort" (1993) 261 *Science* 1509.

68 Odgers and Richardson note 5 *supra* 114. However, we acknowledge, following the work of Jasanoff, that as far as the intersection of the legal system and regulation, particularly in the case of toxic torts, the US traditionally has used a more adversarial and transparent culture of decision-making compared to the less visible and more informal approach to decision-making in Britain. See S Jasanoff, *The Fifth Branch: Science Advisers as Policymakers*, Harvard University Press (1990); S Jasanoff and T Illgen, *Controlling Chemicals: The Politics of Regulation in Europe and the United States*, Cornell University Press (1985).

69 Odgers and Richardson note 5 *supra* at 108.

evidence of questionable reliability" to our courtrooms.[70] Odgers and Richardson contend that the gratuitous admission of expert evidence has led to "a number of apparent miscarriages of justice".[71] Understandably, Odgers and Richardson portray this position as unacceptable. But they characterise miscarriages of justice in terms of bogus or non-scientific expert evidence masquerading as science.[72]

Odgers and Richardson begin their article with an examination of the existing approaches to the admission of expert evidence in Australia. They suggest that judges have traditionally endeavoured to maintain a balance between, "a desire to admit relevant expert evidence against the dangers that it may be given excessive importance by the tribunal of fact or mislead the court".[73] In emphasising this weighting, Odgers and Richardson note that concern has historically been focused upon the jury rather than judicial comprehension of expert evidence.[74] They also describe, as a recent international trend, a tendency to show "greater confidence in lay tribunals" (juries).[75] Odgers and Richardson voice concern about the potential miscarriages of justice which occur if juries are left to decide on expert evidence of questionable quality. They argue that there should be greater concern with the judicial responsibility (implying high levels of judicial scientific comprehension) to direct or exclude expert evidence prior to exposure to the jury.[76]

In describing contemporary Australian evidence law, Odgers and Richardson cite the following passage adopted in *Clark v Ryan*, a case that is conventionally seen as the leading Australian authority on expert evidence:

> [T]he opinion of witnesses possessing peculiar skill is admissible whenever the subject matter of inquiry is such that inexperienced persons are unlikely to prove capable of forming a correct judgment upon it without such assistance, in other words, when it so far partakes of the nature of a science as to require a course of previous habit, or study, in order to obtain a knowledge of it.[77]

Whilst acknowledging that Dixon CJ provided no clear indication of scientific principles, Odgers and Richardson interpret the extract to support two propositions. First, that areas of scientific/expert opinions relied upon in court must comply with "scientific" principle and expertise and depend upon "a course of previous habit, or study". Secondly, they note that the High Court did not

70 *Ibid* at 108, 122.

71 Exactly what they mean by *apparent* in this quote is unclear. Understandably the idea of miscarriages has proven a popular site for attempts to criticise or renegotiate the law-science nexus. See E Magnusson and B Selinger, "Forensic Science in Court" (1988) 12 *Criminal Law Journal* 86 at 86: "The public perception of forensic science has been dominated by the continuing saga of notorious cases, both here and abroad".

72 Odgers and Richardson note 5 *supra* at 117, 118, 129.

73 Huber note 3 *supra* at 111.

74 G Edmond and D Mercer, "Public Understanding of Science: Democracy and the Jury", paper delivered to the Science, Technology and Policy Program, University of Wollongong, 11 November 1996.

75 Odgers and Richardson note 5 *supra* at 111 See also I Freckelton, "Expert Evidence and the Role of the Jury" (1994) 12 *Australian Bar Journal* 73.

76 Odgers and Richardson note 5 *supra* at 111.

77 (1960) 103 CLR 486 at 491.

articulate the source of expertise or the 'nature' of the category "science".[78] The first proposition is a *non sequitur.* However, the interpretation which Odgers and Richardson place on the passage is instructive - they lament the absence of articulated principles and definitions characterising the proper nature of science.

Odgers and Richardson explain that in the absence of a clear definition of science, and before the *Evidence Act* 1995 (Cth), Australian courts developed a number of 'practical' rules for determining the admissibility of expert opinion evidence. Included among these rules are the following (paraphrased): the evidence must derive from a 'field of expertise';[79] the witness must be an expert;[80] the opinion must be relevant; the opinion must not be a matter of common knowledge; the opinion must not be in respect of an 'ultimate issue';[81] there must be disclosure of the facts (assumed) from which the opinion is based;[82] and, evidence must be admitted to prove the assumed facts; and the probative value must not outweigh the prejudicial effect.[83] Odgers and Richardson accept the historical utility of these rules whilst emphasising their fallibility.

Notably, Odgers and Richardson have conflated the issue of the admissibility of novel putative scientific evidence with the issue of establishing the admissibility of scientific evidence where there is scientific disagreement and concern over localised technical competence/incompetence.[84] They emphasise their concern at miscarriages of justice, pointing to *R v Chamberlain*[85] as the exemplar. Yet *Chamberlain* was a case where scientific controversy did not emerge solely from novel or questionable scientific techniques. Similarly, despite their anxiety about the entry of syndrome evidence[86] into Australian courts,[87] Odgers and Richardson do not provide any examples of 'miscarriages' which have followed from these admissions.[88]

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78 Odgers and Richardson note 5 *supra* at 109.

79 *R v Gilmore* [1977] 2 NSWLR 935.

80 *Casley-Smith v FS Evans & Sons Pty Ltd* (1988) 49 SASR 314.

81 For difficulties in recent cases see Aronson and Hunter note 49 *supra.* See also: *Runjanjic v R* note 49 *supra,* per King CJ; *Murphy v R* (1989) 167 CLR 94, per Mason CJ, Toohey and Deane JJ. For commentary see: B Selinger, "Expert Evidence and the Ultimate Question" (1986) 10 *Criminal Law Journal* 246; Maskin note 4 *supra* at 1930. Maskin suggests that 'ultimate issue' concerns featured prominently in the backdrop to the *Daubert* decision. In Australia, the ultimate issue and common knowledge rules have been reformed.

82 *Perry v R* (1990) 49 A Crim R 243.

83 *Evidence Act* 1995 (Cth), s 135.

84 This theme will reappear in our critique of what we describe as Odgers and Richardson's legal distortion model.

85 *R v Chamberlain* (unreported, Supreme Court of the Northern Territory, Justice Muirhead, 29 October 1982); *Chamberlain v The Queen* (1983) 72 FLR 1; *Chamberlain v The Queen* (1983) 153 CLR 521; Royal Commission of Inquiry into the Chamberlain Conviction, *Report of the Commissioner, The Hon Mr Justice TR Morling* (1987); *Re Conviction of Chamberlain* (1988) 93 FLR 239.

86 Odgers and Richardson note 5 *supra* at 118; Freckelton note 4 *supra* at 32; Huber note 3 *supra.*

87 Odgers and Richardson note 5 *supra* at 123. Odgers and Richardson cite *Ingles v R* (unreported, Tasmanian Court of Criminal Appeal, Green CJ, Crawford and Zeeman JJ, 4 May 1993) and *R v Accused* [1989] 1 NZLR 714.

88 Whether there has been a rise or 'crisis' in the frequency of litigation and the admission of new types of evidence is far more contentious than Odgers and Richardson reveal, especially in the US. Consider the following: M Galanter, "Reading the Landscape of Disputes: What We Know and Don't Know (and

Odgers and Richardson believe that Australian courts, in contrast to the allegedly liberal pre-*Daubert* US position,[89] are tending to adopt a more restricted[90] basis for the subject matter admitted as expert opinion. However they show concern with the flexibility of existing common law rules and fear that too much is contingent upon broad judicial discretions. Specifically, they point to a failure by the High Court to provide a clear definition of a 'field of expertise'. This raises Odgers and Richardson's concern with the apparent rise in novel kinds of scientific evidence. Odgers and Richardson are especially concerned with the "burgeoning area of syndrome development"[91] as well as the aetiology of toxic torts as represented in US, and to a more limited extent, Australian experience. Their concern is with:

> ...the new syndromes that have been promoted in the courts in the past few decades - battered child syndrome, child sex abuse syndrome, battered woman syndrome and rape trauma syndrome to name but a few. *Frye* helped to bring some order to what appeared to be the chaotic situation of new syndromes developing almost every month.[92]

The types of 'knowledge' which concern Odgers and Richardson are predominantly "the so-called 'soft' or social and behavioural sciences" especially psychology and psychiatry.[93]

According to Odgers and Richardson, there are dangers of miscarriage of justice if novel science, not conforming to the proper method of scientific practice, is admitted to court. Odgers and Richardson have located the "proper method" in the philosophical prescription known as falsification as endorsed by the US Supreme Court in *Daubert*.[94] We have shown that the current Australian and earlier US exclusionary approaches were structurally similar in that they placed the constraints of acceptability upon the professional communities, albeit judicially managed. Recourse to falsification, as espoused in *Daubert*, represents a major conceptual and practical shift to examining scientific methodology.

Odgers and Richardson's support of the majority judgment in *Daubert* reflects a concern that the existing, ostensibly external, assessment of scientific expertise

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Think We Know) About Our Allegedly Contentious and Litigious Society" (1983) 31 *UCLA Law Review* 4; MJ Saks, "Do We Really Know Anything About the Behaviour of the Tort Litigation System - And Why Not?" (1992) 140 *University of Pennsylvania Law Review* 1147; T Eisenberg and JA Henderson, Jr, "Inside the Quiet Revolution in Products Liability" (1992) 39 *UCLA Law Review* 731.

89 Hutchinson and Ashby note 4 *supra* at 1880.

90 Odgers and Richardson note 5 supra at 123, but compare at 111.

91 *Ibid* at 119. Compare P Gianelli, "'Junk Science': The Criminal Cases" (1993) 84 *Journal of Criminal Law & Criminology* 105 at 113: "...'syndrome' evidence, has flooded the courts".

92 Odgers and Richardson note 5 *supra* at 118.

93 *Ibid*. See also B Black, "A Unified Theory of Scientific Evidence" (1988) 56 *Fordham Law Review* 595 at 629, 647, 651-4. Black shares Odgers and Richardson's concern about forensic psychology, psychiatry and syndrome evidence requiring "a rudimentary understanding of the philosophical foundations of science" to be found "unscientific".

94 Odgers and Richardson are by no means the first Australian commentators to endorse or acknowledge falsification as the appropriate technique for science. See also Bourke note 19 *supra* at 188, 191; B Selinger, "Science in the Witness Box" (1984) 9 *Legal Service Bulletin* 108 at 109. The concept has also received powerful endorsement in the US. Consider Loevinger, "Science as Evidence" (1995) 35 *Jurimetrics Journal* 153 at 169.

is inadequate. Such an assessment allows the introduction of forms of knowledge which do not meet their *a priori* requirements for what constitutes valid science. To satisfy this political objective of giving the judiciary power to classify and/or exclude certain forms of knowledge, an objective shared with many protagonists in the US regulatory and legal cultures, Odgers and Richardson emphasise the need for judicial education about scientific method to provide a corrective influence to existing legal institutions.[95]

The flexible interpretation of *Frye* has led Odgers and Richardson to be critical of external admissibility criteria for scientific evidence.[96] A further point in relation to the flexibility of *Frye* has been made by Huber, who has noted the phenomenon of 'pseudo-sciences' organising themselves professionally in order to resemble orthodox 'scientific' fields and thus being capable of fulfilling its 'general acceptance' (or in Australia the 'field of expertise') requirement.[97] It is noted that falsification will not provide a solution for demarcating between such 'pseudo-sciences' and 'orthodox' science, as discussed below at Part V. Furthermore, it is possible to speculate that falsification is capable of being a double-edged sword that in some contexts could be wielded against establishment science.[98]

In their attempt to incorporate the principle underlying the majority position in *Daubert* into Australian legal discourse, Odgers and Richardson make several references to 'support' from cognate fields such as HPS/SSK. Odgers and Richardson state:

> Instead, they [judges] must be able to discern *good from bad science*, which in turn means that judges must come to some understanding of the history, philosophy and sociology of science, and of proper ways of doing science.[99]

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95 Odgers and Richardson note 5 *supra* at 121-2. Consider E Gejuoy, "Science and Technology Resources for the Courts" (1995) 17 *Technology in Society* 1; M Rosenberg, "Science in the Courthouse" (1994) 16 *Technology in Society* 1.

96 Odgers and Richardson note 5 *supra* at 118.

97 Huber note 3 *supra.*

98 Jasanoff (1990) note 71 *supra*; *Id* (1993) note 46 *supra* at 78: "The adversarial structure of litigation is particularly conducive to deconstruction of scientific facts, since it provides parties both the incentive (winning the lawsuit) and the formal means (cross-examination) for bringing out the contingencies in their opponent's arguments". See also S Jasanoff and D Nelkin, "Science, Technology, and the Limits of Judicial Competence" (1981) 214 *Science* 1211 at 1212; S Yearley, *The Green Case: A Sociology of Environmental Issues, Arguments and Politics*, Harper Collins Academic (1991) p 142; JS Oteri, MG Weinberg and MS Pinales, "Cross Examination of Chemists in Drugs Cases" in B Barnes and D Edge (eds) *Science in Context: Readings in the Sociology of Science*, (1982) p 250; S Yearley, "Bog Standards: Science and Conservation at a Public Inquiry" (1989) 19 *Social Studies of Science* 421 at 432, 437.

99 Odgers and Richardson note 5 *supra* at 116 (italics added). Other commentators make similarly naive claims. For example Miller, Rein and Bailey note 4 *supra* at 258: "preliminary determinations on admissibility of expert scientific testimony do require a grounding in the sociology of science and the scientific method..."; Black note 93 *supra* at 658-59. Ironically a large portion of contemporary scholarship in the sociology of science rejects the existence of any singular or overarching scientific method capable of distinguishing between 'good' and 'bad' science. Whilst we would agree that lawyers and judges could benefit from a grounding in HPS/SSK, others have questioned this assertion in relation to the work of Jasanoff. Consider L Loevinger, "On Logic and Sociology" (1992) 32 *Jurimetrics Journal* 527 at 534; D Kaye, "On Standards and Sociology" (1992) 32 *Jurimetrics Journal* 535 at 541, 546.

They also cite Miller, Rein and Bailer's suggestion that: "at a minimum, judges will have to become more conversant with the 'sociology of science'".[100] Surprisingly, there are very few categories such as 'good' and 'bad' in the relevant literature.[101]

Odgers and Richardson buttress their contentions with the assurance that a number of eminent scientific organisations, such as the US National Academy of Sciences and the American Association for the Advancement of Science, advocate a particular philosophy or description of a putatively singular transhistorical and efficacious scientific methodology, namely falsification.

Whilst we agree that contemporary perspectives drawn from HPS/SSK could provide insights capable of facilitating useful legal reform, it is our contention that Odgers and Richardson demonstrate little familiarity with these fields or their ongoing development. In one example of their remoteness, Odgers and Richardson cite, with seeming approval, the very contentious claim by Underwager and Wakefield that Popper's falsifiability is in keeping with contemporary understanding of the nature of science. Underwager and Wakefield describe *Daubert*, ironically in Kuhnian jargon, as "a revolutionary paradigm shift that replaces naive logical positivism with the contemporary understanding of the nature of science".[102]

In addition, Odgers and Richardson's unfamiliarity with the field is exemplified by their impoverished representation of falsification as well as their conflation of various mutually exclusive models of scientific methodology.[103] Not only have they poorly characterised falsification, the subject of their paean, but they cite only one other article, by Sheila Jasanoff, from an SSK perspective, and no critical HPS perspectives on falsificationist methodology.[104] Ironically the following claims featured prominently in Jasanoff's paper:

> Investigations into the social structure and operation of science have revealed a picture of scientific knowledge that is distant from the logically coherent but highly abstract accounts constructed by philosophers of science.[105]

And,

> *Frye*, and to a lesser extent *Daubert*, are based on a positivist image of science that does not stand up to sociological, historical, or philosophical, scrutiny.[106]

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100 Odgers and Richardson note 5 *supra* at 121.

101 Compare the legal literature where these categories in the form of 'good', 'bad', 'junk' and 'pseudo' are ubiquitous. See Odgers and Richardson note 5 *supra* at 116, as cited in the text above, and at 129. See also Roisman note 4 *supra* at 1945; S Tipple, "Forensic Science: The New Trial by Ordeal" (1986) 24 *Law Society Journal* 44 at 51: "reached a verdict relying on nonsense rather than good science".

102 Odgers and Richardson note 5 *supra* at 122.

103 This is discussed later, but is also a very common occurrence in the legal literature. See Freckelton note 4 *supra* at 29; Bernstein note 4 *supra* at 2144. Bernstein cites a comment from US asbestos litigation when he notes that "[t]he decision in *Daubert* kills *Frye* and then resurrects its Ghost".

104 S Jasanoff, "Beyond Epistemology: Relativism and Engagement in the Politics of Science" (1996) 26 *Social Studies of Science* 393 at 408. Jasanoff notes the interpretative flexibility with which her own work has been embraced in the *Daubert* debate. Contrast G Edmond & D Mercer, "Beyond Accommodation, Engagement and the Politics of Law and Science: Putting Jasanoff to 'Work'", paper delivered to the History and Philosophy of Science Research Program, University of Wollongong, 13 September 1996.

105 S Jasanoff, "What Judges Should Know About the Sociology of Science" (1993) 77 *Judicature* 77 at 77.

We would contend that ignoring established fields of expertise fundamentally distorts the utility which approaches like that postulated in *Daubert* will provide - a position long recognised in other intellectual domains.[107]

It follows that many of the implications raised by more recent philosophical and ethnographic studies in HPS/SSK mean that simple judicial assessment of scientific method, and the rigorous application of the *Daubert* judgment, will be far more complicated and problematic than Odgers and Richardson suggest.[108] Simply educating judges along the lines proposed by Odgers and Richardson will not assist judicial comprehension of the processes of science and will prove unsatisfactory as a mechanism for excluding 'inadequate' scientific knowledge in the manner in which Odgers and Richardson propose.[109] Odgers and Richardson state that:

> It is by no means certain that judges have had sufficient training to understand the principles involved in implementing the falsifiability criterion.[110]

But this presupposes that falsifiability is itself a workable criterion, a presupposition which we intend to examine. The following sections will provide elaboration upon the insufficiency of falsification as a criterion for distinguishing between 'good' and 'junk' science.

Odgers and Richardson note that the phrasing of s 79 of the *Evidence Act* 1995 (Cth) is different to Rule 702 of the Federal Rules of Evidence, as the Australian provision makes no mention of scientific knowledge. They interpret the Australian legislation to allow entry to "specialised knowledge" provided it is "reliable". They afford "reliability" vague parameters, especially in reference to what they would describe as unfalsifiable and therefore non-scientific knowledge. However, following from their faith in the principle of falsifiability, they suggest reform of the Australian legislation to distinguish the entry requirement for scientific expertise:

> ...it would be preferable if the Australian legislation were amended to include specific criteria for determining the admissibility of scientific evidence.[111]

Where this leaves non-scientific expertise and how useful such a distinction would be is left uncanvassed. There is no explanation of how such a taxonomy would assist their position for excluding certain forms of knowledge because it would presumably still be permitted under the broader 'specialised knowledge' requirement. Such a distinction might ease the entry requirement for more 'questionable' knowledge claims as long as they are accompanied by the

106 *Ibid* at 81. We do not agree with Jasanoff on this point because *Frye* makes no attempt to assess the internal workings of science whereas *Daubert* does.

107 Odgers and Richardson are not alone in this approach. Consider the bizarre qualification conceded by Black, Ayala and Saffran-Brinks note 4 *supra* at 753 n 260: "We make no claim to philosophical rigour, or to resolving the positivist versus relativist and other debates about the nature of science. Instead, our discussion aims to present a picture of science in accordance with the way most scientists actually practice their profession."

108 Jonakait note 4 *supra* at 2104, 2109. In contrast see Hutchinson and Ashby note 4 *supra* at 1877, 1887.

109 Before *Daubert*, Bourke advocated the importance of judicial education in terms similar to those of Odgers and Richardson. See Bourke note 19 *supra* at 192-3.

110 Odgers and Richardson note 5 *supra* at 122.

111 *Ibid* at 129.

appropriate judicial warning. Perhaps this is what Odgers and Richardson would desire. Yet it remains unclear whether the jury, or judge, would respond to such a proposal and judicial hierarchisation of evidence in the manner they might hope. These are criticisms which might be applied if the criterion of falsifiability were workable. However as we will now argue, such a proposition is extremely unlikely.

V. SCIENTIFIC METHOD AND ODGERS AND RICHARDSON'S NAIVE CONCEPTUALISING OF THE LAW-SCIENCE RELATIONSHIP

A. Background

Odgers and Richardson, following the *amicus curiae* brief submitted by Black[112] and Ayala,[113] outline their diagnosis for the occurrence of problems in the maintenance of 'good science' in legal contexts according to the precepts of *Frye*. A large part of the problem, according to Odgers and Richardson, flows from the opportunities for "scientists to give testimony based mainly on their personal biases".[114] This opportunity for distortion[115] is created because the judiciary have failed to develop "clear and consistent" guidelines for evaluating scientific evidence and have failed to hold "experts to the same standards scientists themselves use in evaluating each others' work".[116] Importantly, argue Odgers and Richardson, there should be an "appreciation of how science works through the formulation and testing of hypotheses...[and] the institutional mechanisms science has developed for sharing and evaluating results".[117] These themes, particularly the importance of falsifiability (the testing of hypotheses) as a demarcation criterion between 'good', 'bad' and 'junk' science are developed further at a later point by Odgers and Richardson. Before exploring the implications of Odgers and Richardson's image of falsification more specifically,

112 Chair of the American Bar Association's Standing Committee on Scientific Evidence.

113 President of the American Association for the Advancement of Science and Member of the National Academy of Science.

114 *Ibid* at 117, citing F Ayala and B Black, "Science and the Courts" (1993) 81 *American Scientist* 230.

115 This has presumably been derived from Huber note 3 *supra*. Huber correctly identifies the influence on the construction of scientific claims in a number of contemporary areas of law by lawyers hoping to maximise compensation. This critique operates by juxtaposing these areas of science against an ideal image of science unaffected by social factors. In doing so, Huber conflates social influence, always present in science, for some type of extraordinary intrinsic legal-epistemological distortion. The following authors refer to some type of Court distortion theory: Magnusson and Selinger note 75 *supra* at 90; D Blazevic, "When Science and the Law Go Head to Head" (1986) 11 *Litigation News* 3 at 23-4; Clark note 21 *supra* at 69; Zatz note 13 *supra* at 14; P Huber, "A Comment on *Toward Incentive-Based Procedure: Three Approaches for Regulating Scientific Evidence* by E. Donald Elliot" (1989) 69 *Boston University Law Review* 513.

116 Odgers and Richardson note 5 *supra* at 117

117 *Ibid.* Eminent US commentators make a similar point in their recent commentaries on *Daubert*. See B Black, F Ayala and C Saffran-Brinks, "Science and the Law in the Wake of Daubert: A New Search for Scientific Knowledge" (1994) 72 *Texas Law Review* 715; L Loevinger, "Science as Evidence" (1995) 35 *Jurimetrics Journal* 168 at 169.

and its role in solving the law-science problem, it is useful to first address Odgers and Richardson's confidence that a solution to problems of defining science in legal contexts can be arrived at by exposing scientists in courts to the same standards that scientists themselves use in evaluating each other's work.

Odgers and Richardson's confidence in this solution relies on the idea that there exists in science, outside legal contexts, a scientific method and special institutional mechanisms sufficiently clearly defined and operational so that the legal system can be reshaped to facilitate their effective transfer and accommodation. To Odgers and Richardson, the reshaping of admissibility requirements for expert evidence along *Daubert* lines represents an important step in facilitating the removal of distortion that normally occurs when science enters legal contexts. The adequacy of *Daubert* and the whole of Odgers and Richardson's schema becomes problematic when a more nuanced image of the institutional mechanisms of science and scientific method are considered. The following discussion will suggest that Odgers and Richardson are seeking to reshape legal contexts to be receptive to an image of science which bears little resemblance to the reality of scientific work and the construction of scientific knowledge. Following debate stimulated by Kuhn and Feyerabend,[118] numerous studies in HPS/SSK have challenged notions that there is any kind of clearly defined operational universal scientific method or standard of conduct and institutional practice unique to science, which guarantee its privileged epistemological status. We can begin challenging Odgers and Richardson's unrealistic image of science by addressing the latter point.

B. Problems With Ideal Standards (Norms) and Institutional Imperatives of Science

There have been a number of attempts to formulate, in abstract terms, the ideal standards of conduct (norms) and institutional imperatives of science. Merton has provided the most influential academic formulation of these principles. He characterised them under the four following headings: communalism; universalism; disinterestedness; and organised scepticism.[119] SSK scholar, Mulkay, has formulated these in less specialised terms as follows:

> The norms of science are seen as prescribing that scientists should be detached, uncommitted, impersonal, self-critical and open-minded in their attempts to gather and interpret objective evidence about the natural world. It is also assumed that considerable conformity to these norms is maintained; and the institutionalisation of these norms is seen as accounting for that rapid accumulation of reliable knowledge which has been the unique achievement of the modern scientific community.[120]

118 P Feyerabend, *Against Method*, Verso (1993); TS Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press (1962). Ironically, Popper's critiques of logical positivism helped enhance the reception of the critiques of scientific method of Feyerabend and Kuhn.

119 RK Merton, *The Sociology of Science*, University of Chicago Press (1973) (originally formulated in 1942).

120 M Mulkay, *Science and the Sociology of Knowledge*, Allen & Unwin (1979) p 64.

Whilst being commonly cited,[121] the idea of norms has been questioned as a description of actual scientific behaviour. At an empirical level, numerous studies of the actual conduct of scientists have failed to identify adherence to such norms as being a meaningful defining feature of doing scientific work. One of the most widely quoted studies in this context has been the work of Mitroff. Whilst he set out to rescue a weak, highly modified version of norms, his case study is typically taken as showing the implausibility of the whole schema.[122] Mitroff found that the ideal norms of scientific conduct were equally represented by counter norms. For instance, whilst scientists sometimes advertised the value of emotional neutrality they also claimed the need for the occasional strong, even unreasonable commitment, to scientific ideas. This was something of significant psychological importance given the disappointments, frustrations and intellectually taxing nature of scientific work.[123]

Further, Mitroff noted that ideals of universalism and judging claims on impersonal grounds were also matched by scientists considering it perfectly normal to assess knowledge claims on personal criteria such as the experience, status, reliability and skills of a researcher.[124] It has been argued that such 'gate-keeping' processes are to be expected as pragmatic responses to the vast quantity of modern scientific research output and the pressures of maintaining research momentum in the typically competitive environment of modern scientific research.[125]

Other examples of the pragmatic flexibility in the application of the 'norms' of science identified by Mitroff included the balancing of ideals of the open communication of the scientific results with the use of secrecy. Secrecy was often justified by scientists as an important device to avoid the disruption of priority disputes and to be able to check results without jeopardising priority whilst avoiding criticism of preliminary results which could dampen their

121 See for example: Committee on the Conduct of Science, National Academy of Sciences, *On Being a Scientist*, 1989, pp 1-21; ML Smith, "On Being an Authentic Scientist, (1992) 14 *IRB* at 1-4; NW Storer, *The Social System of Science*, Hollt, Reinhart & Winston (1966).

122 I Mitroff, *The Subjective Side of Science*, Elsevier (1974). Much of our discussion of Mitroff has been drawn from Mulkay. See note 126 *supra*; *Id*, "Norms and Ideology in Science" (1976) 15 *Social Sciences Information* 637; *Id*, "Interpretation and the Use of Rules: The Case of the Norms of Science" in T Gieryn (ed) *Science and Social Structure (A Fetschrift for RK Merton)*, Transactions of the New York Academy of Sciences, Series 2, Vol 39 (1980); B Barnes and R Dolby, "The Scientific Ethos: A Deviant Viewpoint", (1970-71) 11-12 *Archives Europeennes de Sociologie* 3.

123 This theme of the importance of commitment in science is also apparent in scientific biography. For instance, the case of Kepler as outlined in Arthur Koestler, *The Sleepwalkers: A History of Man's Changing Vision of the Universe*, Hutchinson (1959). This point has also been noted by R Albury in *The Politics of Objectivity*, Deakin University Press (1983) and scientific autobiographies such as JD Watson, *The Double Helix: A Personal Account of the Discovery of the Structure of DNA*, Weidenfeld and Nicolson (1968).

124 Note that these are characteristics which are often seen to be criteria used by a scientifically illiterate jury to come to decisions based on scientific evidence and are usually employed to criticise the institution or current organisation of the jury. See Bernstein note 4 *supra* at 2146; Klein note 4 *supra* at 2223. Compare B Wynne, *Rationality and Ritual: The Windscale Inquiry and Nuclear Decisions in Britain*, BSHS Monograph (1982) p 133.

125 Albury note 123 *supra*.

enthusiasm for further research.[126] Mitroff's empirical work suggests that norms and institutional imperatives of science do not constitute a set of rules or a consistent way of demarcating good from bad science. Rather they can be seen, to quote Mulkay, as part of:

> ...a complex moral language which appears to focus on certain recurrent themes or issues; for instance on procedures of communication, the place of rationality, the importance of impartiality and commitment, and so on [a complex moral language where norms rather than providing a solution become a]...vocabulary or repertoire which scientists can use flexibly to categorise professional actions differently in various social contexts.[127]

C. Problems With Identifying a Universal Scientific Method

The notion that there is a simple identifiable universal scientific method, used in some kind of standard way by scientists in practical contexts to distinguish science from non-science is similarly difficult to support on any kind of empirical basis. Although this point is discussed further below at Part VIB, there are some issues worth noting immediately.

One of the first points to consider is the immense diversity of activities which can be placed beneath the umbrella of modern science. Given such diversity, various branches of scientific knowledge rely, to varying degrees, upon observational practices, experimental tests and mathematical proofs. At anything other than the most ideal, unrealistic and abstract level, empirical studies suggest that it is better to talk of "scientific methods/heuristics guides" (in the plural), which might apply to various branches of the "sciences" (again plural). For instance, in some areas of contemporary science such as in some branches of industrial chemistry, test situations can be established where there is a strong linkage of theory, practice and phenomena. In contrast, other areas of science rely upon situations intrinsically difficult to test. In such situations there may be reliance on statistical methods, new sensitive measuring devices and phenomena not easily modelled in laboratory situations. The latter would be true in many areas of atmospheric physics, ecology, and epidemiology.[128] From this brief sample, it is obvious that notions such as standards of proof, the role of models, acceptable error rates and the role of observation, often noted as ingredients of scientific method, will vary from one branch of science to the next.

A further point is that there has now been more than 25 years of studies in HPS/SSK following from Thomas Kuhn,[129] which have articulated how the judgments made by scientists are not determined by any kind of over-arching

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126 Mulkay note 120 *supra* at 67, 70-1.

127 *Ibid* at 71.

128 This problem has been essayed at some length by Yearley note 98 *supra*.

129 Kuhn note 118 *supra*. It is important to note that whilst Kuhn's work has been extremely significant in re-shaping the way many issues in HPS/SSK are addressed, a number of his more specific ideas such as paradigm and incommensurability have been significantly modified. Kuhn represents an important starting point to a tradition rather than being the provider of all the tools used by practitioners of HPS/SSK. See B Barnes, *TS Kuhn and Social Sciences*, MacMillan (1982).

scientific method.[130] A significant theme in these studies has been the important role of the socialisation of scientists to work in a 'paradigm' or research tradition. Within such contexts, rather than working according to 'the scientific method', scientists make judgments according to standards of measurement, ways of reporting and evaluating results, ideal problem solutions and particular types of experimental practices. Whilst some of these practices are specified in ideal terms in textbooks, they are more often the components of craft or tacit knowledge over which there will be a negotiated consensus at a given time and place during settled periods of science. This 'consensus' is not fixed, and being built on tacit knowledge does not constitute any kind of simple, reducible algorithm against which 'good' or 'junk' science can be evaluated. Judgments as to what constitutes 'good' versus 'junk' science are sometimes made but such social judgments are open to dispute and negotiation and are affected by things like the status of relevant scientists, their research backgrounds, and both their narrow career interests and responses to broader social pressures.[131]

The flexibility and tacit nature of definitions of the adequacy of science within a particular paradigm or research tradition dictates against simple notions of a single, identifiable scientific method.[132] If we further consider, as noted earlier, that there are numerous research traditions and paradigms making up the rich diversity of modern 'science', all with their own various socially negotiated standards of adequacy for what constitutes science, the notion that there exists an operationally viable version of the scientific method useful across the diversity of contexts in which science and law are brought together looks decidedly fragile.[133]

Whilst judgments about what counts as science in practice have been revealed to be tacit and flexible, this does not mean that in some contexts scientists do not use the term 'scientific method'. However, it is used as part of a flexible rhetoric of justification. Two important areas where this occurs have been in scientific controversies and in boundary disputes[134] between 'fringe' and

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130 Examples of case studies include: J Schuster and R Yeo (eds), *Politics and Rhetoric of Scientific Method: Historical Studies*, Kluwer Academic (1986); H Collins and T Pinch, *The Golem: What Everyone Should Know About Science*, Cambridge University Press (1993).

131 T Gieryn, "Boundaries of Science" in S Jasanoff, G Markle, J Petersen, T Pinch (eds), *Handbook of Science and Technology Studies* (1995) pp 403-4; N Gilbert and M Mulkay, *Opening Pandora's Box*, Cambridge University Press (1984). See also H Collins and T Pinch, "Construction of the Para-Normal: Nothing Unscientific is Happening" in R Wallis (ed), *On the Margins of Science: The Social Construction of Rejected Knowledge*, Sociological Review Monograph No 27, University of Keele (1979) p 237; B Wynne, "CG Barkla and the J-Phenomenon: A Case Study of the Treatment of Deviance in Physics" (1976) 6 *Social Studies of Science* 307.

132 Note 3 *supra* at 2795. Many commentators use the singular scientific method or methodology, some employ methodologies and others oscillate between the two with no explanation. Consider: A Roisman, "Conflict Resolution in the Courts: The Role of Science" (1994) 15 *Cardozo Law Review* 1945 at 1945; Maskin note 4 *supra*; B Koukoutchos, "Solomon Meets Galileo (And Isn't Quite Sure What to do With Him)" (1994) 15 *Cardozo Law Review* 2237 at 2243.

133 B Barnes note 129 *supra*; H Collins, *Changing Order: Replication and Induction in Scientific Practice*, Sage (1985); J Ravetz, *Scientific Knowledge and its Social Problems*, Clarendon Press (1971).

134 T Gieryn, "Boundary-Work and the Demarcation of Science From Non-Science" (1983) 48 *American Sociological Review* 781; *Id* note 131 *supra*.

'orthodox' science.[135] In the context of scientific controversies, the social negotiation and resolution of interpretive flexibility involved in constructing science normally taken for granted becomes more visible. As one leading SSK scholar, Collins, has put it, the virtue of investigating controversies can be likened to the metaphor of looking at the construction of ships in bottles:

> ...it is only by examining scientific controversies while they are in progress that the mechanism by which ships (scientific findings) get in to bottles (validity) can be understood. If this process is not seen in operation it may be thought that ships were always in the bottles, and that all scientists did was find them ready assembled, as it were.[136]

A number of patterns of rhetoric using ideals of the scientific method have been observed operating in scientific controversies. Most important has been the various ways scientists have used 'flexible evaluative repertoires', that is, the use of flexible vocabularies for describing their own work relative to their opponents according to different social contexts and various social interests.[137] For example, in evaluation of the rhetoric used by scientists in a controversy in bio-chemistry, Mulkay observed a consistent pattern of a dual conception of what constituted a scientific 'fact':

> This strategy, adopted by both authors, seems to be related to the dual conception of scientific fact which has appeared in every letter so far. The interpretative conception of 'fact' is used in criticising one's opponent. The interpretative basis of the latter's view is made visible and emphasised as the author formulates the inconsistencies, uncertainties and mistakes perpetrated by his opponent. It is always possible for the author to find such errors because the opponent's claims are inevitably assessed in relation to the authors' different conception of the facts and their scientific meaning.
>
> In contrast, when formulating his own views, each author minimises the interpretative work apparently involved. As a result, each author's position comes to appear in the text of each separate letter as indistinguishable from the observable realities of the bio-chemical world.[138]

In basic terms, a tendency has been observed for scientists, in the setting of controversy, to deploy rhetoric to suggest how their scientific findings are isomorphic to nature, constituted by the application of 'appropriate' scientific practices (the so-called 'constitutive forum'). In contrast, rival scientific work can be explained as the by-product of social contingencies (the so-called 'contingent forum').[139]

The use of scientific method as part of a flexible evaluative repertoire has also been observed to follow similar patterns in debates between 'orthodox' and

135 It is important to remember that these labels are flexible, dynamic and open to negotiation.

136 HM Collins, "Son of Seven Sexes: the Social Destruction of a Physical Phenomenon" (1981) 11 *Social Studies of Science* 45.

137 M Mulkay and N Gilbert, "Accounting for Error: How Scientists Construct Their Social World When They Account for Correct and Incorrect Belief" (1982) 6 *Sociology* 165; *Id*, "Warranting Scientific Belief" (1982) 12 *Social Studies of Science* 383.

138 M Mulkay, *The World and the Word*, George, Allen & Unwin (1985) p 43.

139 See discussion in Gilbert and Mulkay note 137 *supra* at 55-58. See also Collins and Pinch note 131 *supra* at 237-270, especially 239-240; Mulkay note 120 *supra* at 83.

'fringe' sciences.[140] In a study of para-psychology, and its relationship to orthodox science, it was noted that the manner by which para-psychology 'scientific claims' were rejected by orthodox science did not follow the pattern of the imposition of some kind of clear demarcation criteria.[141] Whilst para-psychologists placed considerable effort into framing their claims via experimental methods, ideas of proof and mimicking communication processes and the institutional trappings of orthodox science (for example, peer review), their claims were still largely rejected by orthodox scientists on a variety of grounds spanning the contingent and constitutive forums.[142] For example, some scientists rejected the claims of para-psychologists as uninteresting examples of empty correlations unworthy of further research. In other contexts, empirical work of para-psychologists was rejected on the basis that the theory underlying the work was unconvincing. Finally, even when some results of experimental work were accepted as conforming to existing standards in probability theory, it was argued that such results should be inadmissible. Given the wider validity of probability theory in recognised 'scientific contexts', the positive results should be interpreted as representing the by-product of experimental error, fraud, or self-deception.[143] Ideas of scientific method in demarcating orthodox science from 'fringe non-science' appear to be strongly influenced when the objects of discourse are seen as intuitively plausible and acceptable, rather than by mechanical appeals to doctrines of scientific method. Even then, note that in some contexts, what is taken as the 'relevant' field of science can also be open to challenge.[144]

Finally, evidence that scientists rarely reflect on scientific method in philosophical terms in their day to day work mitigates against the idea of there being a universal method providing a sufficient way of defining science within the scientific community. Barnes, for instance, has noted surveys showing a lack of formal philosophical literacy amongst working scientists.[145] Mulkay and Gilbert have noted the inconsistent meanings attached to the philosophies of science in those instances when scientists advertised the importance of scientific

140 We are not attempting to make judgment about what should or should not be regarded as legitimate science, rather these are social actors' categories.

141 It appears that Odgers and Richardson, have eliminated syndrome evidence in a similar manner to the orthodox rejection of other allegedly 'pseudo-sciences'.

142 RG Dolby, "Reflections on Deviant Science", in Wallis (ed) note 131 *supra* at 9, 19; Collins and Pinch note 137 *supra*. Whilst still largely rejected, there was some evidence that taking on institutional trappings of science did provide some kind of enhancement of the scientific status. Note that legal commentators are generally dismissive of what they describe as 'junk' and 'pseudo sciences' without clearly *demonstrating* faults in their underlying methodologies. For example D Faigman, E Porter and M Saks, "Check Your Crystal Ball at the Courthouse Door, Please: Exploring the Past, Understanding the Present and Worrying About the Future of Scientific Evidence" (1994) 15 *Cardozo Law Review* 1799 at 1801; Koukoutchos note 132 *supra* at 2244.

143 Mulkay note 120 *supra* at 84-85.

144 T Pinch and H Collins, *Frames of Meaning: The Social Construction of Extraordinary Science*, Routledge and Kegan Paul (1982).

145 B Barnes, *About Science*, Basil Blackwell (1986). This does not discount the contribution to the philosophy of science by eminent scientific figures such as Hiesenberg and Mach. Consider D Oldroyd, *The Arch of Knowledge*, University of New South Wales Press (1986).

method.[146] As Yearley ironically notes, classes on Popper and Lakatos and how to apply the scientific method are much more common in economics, sociology and psychology than chemistry.[147]

Overall, the above discussion shows that norms and scientific method, rather than being rules or explicit guides to demarcate good science from bad science, are better identified as part of the professional rhetoric of science, operating as a vocabulary used to describe the ideal workings of science in simplified terms. This rhetoric is most often deployed in contexts where the symbolic authority of science more generally is at stake, for instance in scientific controversies, educational contexts, legal contexts, and science 'popularisations'.[148] The closer the empirical focus on the actual workings of science has been, and the more current and uncertain the area of science examined, the more difficult it has become to identify simple ideal models of methods and norms. This point undermines Odgers and Richardson's legal distortion model because its policy agenda implicitly requires norms and scientific method to be specific and sufficiently well formulated to operate in legal contexts. Legal contexts may impose pressures on the work of scientists but not in the sense of distorting some kind of unrealistic epistemological ideals of scientific method or norms in the manner suggested by Odgers and Richardson.

D. Reconceptualising the Law-Science Relationship

As well as problems with naive conceptualisation of norms and method, Odgers and Richardson's case for legal distortion can be undermined for a number of additional reasons. We can begin by pointing out that 'disagreements' in science do not require explanations such as Odgers and Richardson's, which rely on theories of some kind of intrinsic 'epistemological distortion' of science by the entry of social factors into its normally 'epistemologically pristine' domain. Rather than exemplify instances of 'epistemological distortion', it has been noted that scientific disagreements occur for a variety of reasons. These can range from 'internal' pressures within sub/cultures of scientists when there are shifts in the intellectual orientation of scientific 'research traditions' and 'paradigms' resulting from competition

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146 M Mulkay and N Gilbert, "Putting Philosophy to Work: Karl Popper's Influence on Scientific Practice" (1981) 11 *Philosophy of the Social Sciences* 389. Consider for example writings by P Medawar, *Advice to a Young Scientist*, Harper and Row (1979) or H Bondi, "The Philosopher for Science" (1992) 352 *Nature* 363.

147 S Yearley, "Understanding Science From the Perspective of the Sociology of Scientific Knowledge, An Overview" (1994) 3 *Public Understanding of Science* 245 at 249. For similar observations see A Chalmers, *What is This Thing Called Science?*, Queensland University Press, (1982) p xiv. The popularity of Popper with law students at the London School of Economics was given added support through a discussion with Dr A Taylor. It is ironic then that commentators have suggested that "judges must achieve at least a basic level of literacy" when this so-called "literacy" in its philosophical guise is not part of conventional scientific training. P Miller, B Rein and E Bailey, "*Daubert* and the Need for Scientific Literacy" (1994) 77 *Judicature* 254 at 254.

148 It is worth noting that a relatively small number of elite scientists dominate public discourse on science and possess a disproportionate influence over packaging the images of science presented to the public: D Nelkin, *Selling Science*, WH Freeman and Co (1987); S Blume, *Towards a Political Sociology of Science*, Free Press (1974). Ayala and Black's entry into the *Daubert* debate exemplifies this tendency.

between scientists for funding and social authority;[149] to the need to respond to social demands to provide authoritative explanations for natural phenomena, particularly in areas where there is a lack of settled knowledge or well defined boundaries of relevant expertise.[150] From this perspective, scientific disagreement should be analysed as part of the diversity of processes of science and not as something to be explained away as epistemologically atypical special cases.

Following this framework we should also avoid 'explaining away' as an *a priori* epistemological problem the presence of scientific disagreement in legal contexts. In some instances a legal setting may be drawing on a pre-existing scientific disagreement, yet in others there may be special features of the legal setting itself which is contributing to the disagreement in question. Scientific disagreements in legal settings should be empirically investigated with consideration of: the particulars of the scientific knowledge claims in question; the specific features of the legal setting in question; and the specific way science and law have been brought together. This does not mean that it is impossible to make generalisations about the nature of science and law and their relationship, but that such generalisations should not be built on the basis of *a priori* epistemological categories of science and law.

Proceeding from a more contextual view of the law-science relationship, such as that outlined above, writers such as Smith, Wynne, Jasanoff, and Yearley have, in a sense, turned Odgers and Richardson's image of legal distortion on its head. Rather than scientists exploiting the supposed epistemological latitude provided by legal contexts to distort scientific questions, the situation is often one where scientists are constrained by the lack of epistemological latitude in legal contexts. Scientists often find that their knowledge is prone to deconstruction by lawyers exposing scientific knowledge claims to formal standards and models quite different from the tacit judgments involved in the practice of the particular branch of science in question.

The chasm between ideal images of science and the messy realities of scientific practice is a particularly fertile source for the legal deconstruction of science. This is especially so in adversarial settings where lawyers exert greater control over the spatial and temporal contexts in which knowledge claims are produced and evaluated. Scientists then experience difficulties in legitimating their knowledge claims in the face of formalised scepticism. A number of case studies support these propositions.[151]

A widely quoted example can be found in the work of Oteri, Weinberg and Pinales.[152] In their study, the authors provide a guide for cross examination of chemists in drug cases. They indicate the specific way an expert's authority can be thrown into doubt. They note that the lawyer may challenge whether or not

149 Albury note 123 *supra*; Mulkay note 120 *supra*.

150 Yearley note 98 *supra*; Gieryn note 130 *supra*.

151 Oteri, Weiburg and Pinales note 98 *supra*; Yearley *ibid* at 140-143; B Wynne note 131 supra at 34-5; Wynne note 1 *supra* Ch 7; Jasanoff note 98 *supra*; R Smith, "The Trials of Forensic Science" (1988) 4 *Science As Culture* 71.

152 *Ibid*.

the qualifications of the chemist neatly match the practical issue at stake. For instance, has the chemist personally tested the specific substance at hand, or do they rely on hearsay from other researchers?[153] Other strategies include highlighting the variations between the methods used in various drug tests. For example, some tests may be performed which have a strong empirical background but an absence of deeper theoretical knowledge about the underlying processes involved. Such tests may be widely accepted by convention even though they rely on numerous assumptions. In other contexts, it can be brought to court's attention that the tests being used might not be the most accurate, but have been chosen because they are easier or quicker or cheaper to perform.

Scientific experts pitted against one another in a legally mediated environment, where knowledge claims are frequently exposed to intense scepticism, are hardly in a position to acknowledge the more informal processes involved in the construction of scientific knowledge. Ironically, maintaining their authority in such settings is one of the very things leaving their scientific work open to deconstruction. Wynne develops these themes to critique the various ways artificial images of rationality foreclose a proper understanding of the law-science relationship. He posits that one of the reasons there is a disinclination to openly acknowledge the craft and tacit based knowledge of science is that the legal system boosts its own social authority by nurturing a self-image of legal practice similar to the ideal image of science.[154] The ideal self images of legal thought and practice emphasise the possibility of legal systems to transcend the political and personal to ensure the optimal rational outcomes in conflict resolution, via the objective discovery of facts and impersonal application of rules.[155]

This image has notable similarities to that of science, being defined according to its possession of behavioural 'norms' and the application of the scientific method. Recognition that legal forms of knowledge and assessment, like science, rely on various tacit and contingent judgments would weaken legal claims for social authority. It is not surprising that, in contexts where law and science are brought together, there is little recognition of the contingent nature of

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153 Aronson and Hunter note 47 *supra* at 971 state: "working with hearsay is one of the hallmarks of most areas of expertise". See also Wynne note 131 *supra* at 132.

154 This image has been open to criticism from the time of the American Realists and openly explored in the writings by more contemporary critiques such as those of critical legal studies. A sample includes: D Kennedy, "Are Lawyers Really Necessary?" (1987) 14 *Barrister* 11; M Tushnet, "Is There a Marxist Theory of Law?" (1983) 26 *Nomos* 171; AC Hutchinson and PJ Monahan, "Law, Politics, and the Critical Legal Scholars: The Unfolding Drama of American Legal Thought" (1984) 36 *Stanford Law Review* 199; D Fraser, "Truth and Hierarchy: Will the Circle be Broken?" (1984) *Buffalo Law Review* 729.

155 This legalistic or formalistic approach to hermeneutics has dominated Australian jurisprudence since 1920 when Isaacs J gave his famous judgment in *Amalgamated Society of Engineers v Adelaide Steamship Co* (1920) 28 CLR 129. A shift can be ascertained in the writing of recent High Court appointee Justice Michael Kirby. See M Kirby, "Courts and Policy: The Exciting Australian Scene" (1993) *Commonwealth Law Bulletin* 1794; *Id*, "In Defence of Mabo" [1994] 1 James Cook University Law Review 51. In the policy arena consider: S Prasser, "Public Inquiries in Australia: An Overview" (1985) xLiv/1 *Australian Journal of Public Administration* 1; OECD Report, *Technology on Trial* (1979).

the application of rules and the divergence between images of rationality and the actual processes involved in the construction of knowledge.

Such unremitting scepticism is not the only way in which the legal context can be seen as providing a restrictive framework for scientists. For example, scientists are often called upon to answer problems that do not neatly mesh with any pre-defined body of scientific expertise.[156] They have to work to unfamiliar time constraints,[157] and are forced to accept that their knowledge claims will be 'reconstituted' into legally tractable terms, both during proceedings and in the record of a court or similar legal entity.

On the latter point, it is worth noting that scientists have not always been willing to accept the re-working of their knowledge into legally tractable terms. A good example of this has surrounded the unwillingness of scientists to participate in, or accept the authority of, so-called science courts. Science courts were originally proposed in 1976 by Arthur Kantrowitz in a White House Task Force on anticipated advances in science and technology. The courts were originally designed to work through three phases of problem solving: first, the identification of significant questions of science and technology associated with the controversial public policy issue in question, leaving ethical/political questions for subsequent consideration; second, the establishment of an adversarial proceeding to be presided over by scientists/judges where scientific experts would testify and scientific advocates would cross examine them; and finally, the issue of a decision by the judges on the scientific facts pertaining to a disputed technical question.[158] A leading proponent of science courts, Mazur, attempted to set up a science court/adversarial proceeding in the wake of the New York Public Service Powerline Inquiry.[159]

Mazur contacted two leading proponents of linkages between health problems and electric and magnetic fields ("EMF"), Marino and Becker, and a number of experts backing the power utilities who proposed that there was no health danger. He asked the various proponents to put forward their key claims as clearly as possible. Marino and Becker agreed to this. Acting in the capacity as referee, Mazur recommended ways in which he felt Becker and Marino could modify their claims so as to free them from ambiguous, unfalsifiable assertions, and 'value judgments'. This was eventually done to Mazur's satisfaction and should have constituted a pre-requisite for embarking on an adversarial

156 For example, the need to ascertain whether a dingo had bitten material in the *Chamberlain* case. See note 85 *supra*.

157 D McBarnet, *Conviction: Law, the State and the Construction of Justice*, MacMillan (1983) Chs 3, 5; J Nyhart and M Carrow (eds), *Law and Science in Collaboration: Resolving Regulatory Issues of Science and Technology*, Lexington Books (1983).

158 B Caspar and P Wellstone, "Science Court on Trial in Minnesota" in B Barnes and D Edge (eds) *Science in Context* (1982) p 282. See also A Kantrowitz, "Democracy and Technology" in C Starr and C Ritterbush (eds), *Science, Technology and the Human Prospect* (1980) p 199; R Masters and A Kantrowitz, "Scientific Adversary Procedures: The SDI Experiments at Dartmouth" in M Kraft and N Vig (eds), *Technology and Politics* (1988) p 278.

159 A Mazur, A Marino and R Becker, "Separating Factual Disputes from Value Disputes in Controversies Over Technology" (1979) 1 *Technology in Society* 229; A Mazur, *The Dynamics of Technical Controversy*, Communication Press (1981) pp 34-42.

proceeding. Nevertheless, this was not to be; the experts acting on behalf of the power utilities ultimately dismissed the project and refused to co-operate with Mazur arguing any response gave their opponents' claims false credibility.[160]

Following from work in the sociology of science, the conclusion can be drawn that any analysis of the law-science relationship must be cautious in using models which imply that some kind of epistemological essence of science is being distorted by the legal process. We should be aware of the challenges scientists face when confronted with settings where lawyers dominate interpretive control over the processes involved in the construction of knowledge. Nevertheless, we should keep in mind that part of the tension in the law-science relationship flows from the fact that both areas of activity share parallel justificatory rhetorics and it is structurally difficult for each body of practice/discourse to acknowledge the more localised features relevant to the framing and negotiation of both scientific and legal knowledge. In sociological terms, much of this tension in the law-science relationship involves the process described by Gieryn as "boundary work". Boundary work describes the processes involved when subcultures attempt to establish claims about the scope, extent and application of their expertise and preferred professional image to outsiders and in so doing attempt to provide barriers between their subcultures and competitors.[161]

A further complexity in developing a plausible legal distortion model for understanding the law-science relationship in the manner of Odgers and Richardson is the significant number of areas where science and law have been brought together in hybrid forms.[162] Such areas include forensic science, patent law, environmental regulation and insanity laws. With increasing demands on governments to formulate authoritative public policy,[163] certain areas of science and law have evolved together in close relationships. Smith and Wynne describe this trend as follows:

> At the same time as science's role in legal processes expands legal procedural models also begin to enter science. This is because science's own rather informal

160 Mazur *ibid* at 41-42. In a similar vein, science court proposals have received criticism for assuming that the use of court-like procedures would be able to separate scientific facts from social preconceptions. One problem is that for a scientist to gain sufficient scientific authority to pronounce in an authoritative way on a matter of scientific controversy, such a scientist is normally already a participant in the controversy in question. Selecting 'scientist-judges' or 'experts' who possess scientific authority but are not simultaneously embroiled in the proceedings is difficult. Further, selection of scientists-judges without prior involvement may well lead to inconclusive, non-authoritative conclusions. See DW Mercer, *The NIEMR/EMF Controversy: The Social Construction of Scientific Knowledge and Science Policy in the 'Gibbs' Powerline Inquiry 1990/91*, (unpublished PhD thesis, 1993), at 295-313, 361-9. Alternatively a 'decisive' but not scientifically authoritative conclusion may appear to be a technocratic imposition. See DW Mercer, "Understanding Scientific and Technical Controversy", University of Wollongong, Department of Science and Technologies Studies Occasional Paper (1996) pp 22-7.

161 Gieryn note 134 *supra*; Jasanoff note 44 *supra*.

162 R Smith, "Forensic Pathology, Scientific Expertise, and the Criminal Law" in Smith and Wynne (eds) note 1 *supra* at 56; C Arup, "Introduction" (1992) 10 *Law in Context* 5; A Cambrioso, P Keating and M Mackenzie, "Scientific Practice in the Courtroom: The Construction of Socio-Technical Identities in a Biochemical Patent Dispute" (1990) 37 *Social Problems* 275; Jasanoff note 1 *supra* at 1-24, 50-2.

163 Y Ezrahi, "The Authority of Science in Politics" in A Thackray and E Mendelsohn (eds), *Science and Values: Patterns of Tradition and Change* (1974) p 215.

procedural mechanisms are found inadequate for reaching authoritative truths in contentious policy issues.[164]

This integration of science and law often operates more deeply than merely the specific settings of given legal proceedings. In fact, the very constitution of certain types of scientific knowledge can be shown to be shaped by the demands of legal/quasi-legal settings.[165] Smith and Wynne note that this integration appears at its most obvious when we consider fields of knowledge such as forensic pathology:

> ...it is not only the court room interaction that socially shapes knowledges: the institutional integration of a particular expert profession into the legal process already achieves this. Indeed, for forensic science and pathology, the legal process itself has created their particular type of professional interaction and expert knowledge. The social integration of forensic expertise with the law is such that forensic experts have learnt to reconcile themselves to the regular adversarial scepticism of legal processes, whilst maintaining the normal consensual discourses of scientific expertise whereas other disciplines may manage this by defining the courtroom interaction as 'unscientific', this is not so easily available to forensic experts, because the courtroom is their ultimate professional arena.[166]

Whilst some critics might argue that such pressures for hybrids to develop is part of the source of problems in a law-science relationship,[167] such viewpoints rely on a naive image of the insulation of science from society and ignore that it has been quite common for scientific disciplines to develop out of the context of particular social, economic or technical needs.

Many of the weaknesses in Odgers and Richardson's legal distortion model, as discussed above, are common to their recommendation that the scientific method doctrine of falsification developed by Popper should play a significant role in overcoming the so-called legal distortion of science.

VI. FALSIFICATIONISM AS A SOLUTION TO THE LAW-SCIENCE 'PROBLEM'

A. Background

Whilst it would be difficult to overestimate the importance of Popper as one of the key figures in HPS/SSK, it would appear to be easy for people outside of HPS/SSK to underestimate the degree of intellectual criticism levelled at Popper's doctrine of falsificationism. There has been considerable debate highlighting falsificationism's internal inconsistencies, its poor fit with the history of science, and the difficulties in imagining how it could be applied in practical contexts. These debates have been complicated because Popper continuously modified the finer points of the doctrine in response to his critics.

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164 Smith and Wynne note 1 *supra* at 2; Jasanoff note 105 *supra* at 397. Jasanoff employs the term "co-production" to capture the intricacies involved with knowledge construction and compatible situated social orders.

165 *Ibid*; Jasanoff note 1 *supra*; Mercer note 160 *supra*.

166 Smith and Wynne note 1 *supra* at 15.

167 Huber note 3 *supra*.

This problem is accentuated as many of his critics and supporters have also operated with a variety of interpretations of the doctrine. The attractive and simplistic formulations of Popper's doctrine encountered outside HPS/SSK rarely acknowledge its more complex mutations or convey that in its own intellectual context it has been widely rejected, having failed to fulfil its promises.

The central promises of Popper's doctrine of falsification were that: it provided a solution to the problem of induction; that it explained the progress of science; and that it constituted a demarcation criterion to separate science from non-science. It is this last feature which has no doubt accounted for its attractiveness to those concerned with extracting so called 'junk' from 'real' science in the courtroom.

How did Popper believe falsification solved these problems? The problem of induction, sometimes known as Hume's problem,[168] surrounded the paradox that no number of positive inductive observations can provide a certain inductive generalisation in the way that deductive mathematical knowledge can. Whilst we might develop confidence in our knowledge gained by inductive experience we can never claim certainty.

A second problem of induction that pre-occupied Popper, one often overlooked in 'pop' reconstructions of his work, was his recognition that the psychological processes involved in observation made it an unreliable source for generating new scientific theories:

> Popper's work on falsifiability is now often appraised in terms of its registering of the vital importance of the empirical test. But in an intellectual climate dominated by empiricism - that of the English speaking world of the 1950s and 60s - Popper's classic arguments were seen as drawing attention to theory construction and undermining a naive belief in the simple accumulation of data, from which theory would eventually arise, like steam out of a kettle, this anti-empiricist impetus in Popper's work was evident in the writings of those influenced by him, above all, Paul Feyerabend.[169]

Contrary to the logical positivist philosophers against whom he addressed his early work, Popper contended that observations are shaped by our prior expectations and secure their meaning by their links with other observations and existing theory.[170] This point was linked to his desire to account for the progressive nature of scientific change. For Popper, if science was based on induction/observation it would not be able to guarantee certainty (because of Hume's problem) and it would not have progressed, as the psychology and theory loading of observation would have led scientists merely to confirm their existing theories. The problem for Popper was providing an explanation for the certainty and progress of science that did not rely on induction/observation as its starting point.

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168 D Hume, *An Inquiry Concerning Human Understanding*, Collier (1962). See also Allen note 4 *supra*.

169 See comment by R Blackburn, "Symposium - Karl Popper, (1902-1994), Learning From Negative Instances" (1995) 70 *Radical Philosophy* 8.

170 KR Popper, *The Logic of Scientific Discovery*, Harper Torch Books (1959).

In its simplest, most commonly quoted form[171] (also its most austere, tractable but problematic form) falsification worked in the following way. The building block of science is the critical test. The source of scientific hypotheses is irrelevant as long as scientists are able to frame such hypotheses in terms that leave them logically open to being shown to be false. These hypotheses (or conjectures) should also be framed in the most specific testable terms possible. The bolder and more open to test it is, the better the conjecture. A further demand on the discipline of the scientist was to adopt the appropriate critical attitude and avoid developing emotional commitment to theories. Even one single falsifying observation should be sufficient for the rejection of a well established theory. As far as possible, devices like *ad hoc* hypotheses or temporary suspension of criticism, even toward part of a theory, were forbidden. Out of this critical process of trial and error certain testable theories survive attempts at falsification. These become the most certain knowledge of that particular time - but because science is an open ended enterprise even these theories, as robust as they might seem, are open to future testing and future rejection.

By focusing scientific energy on testing ahead of generating positive confirming observations, Popper argued that falsificationism avoided Hume's problem. This was the case because, whilst Hume was correct in noting that no number of positive observations could lead to a generalisation being held as certain, falsification was built on the logical proposition that only one falsifying instance could lead to a generalisation being held as certainly wrong. The logical economy of this asymmetry was supposedly exploited by science operating according to falsificationism. Falsification, by its emphasis on testing was also seen to address the traps of scientific stagnation implied in models of scientific method which emphasised that science started with observation and built up generalisations inductively.

Apart from these putative philosophical 'strengths', Popper also argued that falsification offered a way to demarcate science from 'pseudo science'. This theme overlapped with the more 'political' Popper. Popper was concerned by Marxism, certain branches of psychology, Freudian and Adlerian in particular, as well as the social sciences more generally. He argued that such branches of knowledge might inappropriately be able to claim the status of science if the demarcation criterion of scientific method merely relied on inductivism - the confirmation of theories by building up empirical observations. In fact, this was one of the key problems of the bodies of knowledge he wished to reject, namely that they framed hypotheses so that they explained all possible states of affairs.

In his influential introductory guide to the philosophy of science, Chalmers provides an example of the way Adlerian 'theorising' could be deemed unscientific according to the falsificationist schema. We can begin by noting that a fundamental tenet of Adlerian theory is that feelings of inferiority provide the prime motivation for human actions. This tenet can be verified by imagining

171 *Id*, "The Demarcation Between Science and Metaphysics", in *Conjectures and Refutations: The Growth of Scientific Knowledge*, Routledge and Kegan Paul (1963) pp 253-92.

the motivations and actions of a man watching a nearby child fall into a treacherous river:

> The man will either leap into the river in an attempt to save the child or he will not. If he does leap in, the Adlerian responds by indicating how this supports his theory. The man obviously needed to overcome his feelings of inferiority by demonstrating that he was brave enough to leap into the river, despite the danger. If the man does not leap in, the Adlerian can again claim support for his theory. The man was overcoming his feelings by demonstrating that he had the strength of will to remain on the bank, unperturbed, while the child drowned.[172]

It is possible to suggest that Adlerian theory is confirmed by any imaginable form of human action. For Popper, this lack of falsifiability makes it a 'closed', 'static' knowledge system. It cannot 'progress' or provide any explanation beyond what is already known, and cannot be regarded as a true science.

B. Falsification: Not a Reliable View of Scientific Practice

Falsification has been promoted as a model of scientific method in the retrospective mythical re-creations of the work of famous scientists,[173] in some areas of science policy[174] and in popular contexts, where it has been used as an important element in the rhetorical defence of orthodox science against perceived threats by fringe science.[175] However, falsificationism has been criticised on a number of overlapping philosophical, historical and practical grounds.

(i) Philosophical Problems with Falsificationism

Two related philosophical challenges to falsificationism have been: first, that it has not evaded the problems of the complex and uncertain nature of observation; and second, that it adopts an extremely naive view of real world test situations. The former point can be explained in the following terms: a theory cannot be conclusively falsified because attempts at falsification rely on observations which are, as Popper noted himself, fallible and open to revision, and thus the problems with induction remain. Chalmers describes the situation succinctly in the following terms:

> But it is precisely the fact that observation statements are fallible and their acceptance only tentative and open to revision, that undermines the falsificationist position. Theories cannot be conclusively falsified because the observation statements that form the basis for the falsification may themselves prove to be false in the light of later developments. Knowledge available at the time of Copernicus did not permit a legitimate criticism of the observation that the apparent sizes of Mars and Venus remain roughly constant, so that Copernican theory, taken literally, could be deemed falsified by that observation. One hundred years later, the falsification could be revoked because of new developments in optics.[176]

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172 Chalmers note 147 *supra* at 38.

173 B Magee, *Popper*, Woburn Press (1974) "Introduction".

174 D Collingridge and C Reeve, *Science Speaks to Power: The Role of Experts in Policy-Making*, Pinter (1986).

175 Wallis note 131 *supra*.

176 Chalmers note 147 *supra* at 60.

Real world test situations present similar difficulties. Whilst it may appear that a theory is being disproved by a negative test result or failed prediction, it is always possible that it is actually part of the test situation itself which might be the source of problems. Chalmers provides the hypothetical example of attempting to test an astronomical theory by observing the position of a planet through a telescope:

> The theory must predict the orientation of the telescope necessary for a sighting of the planet at some specified time. The premises from which the prediction is derived will include the interconnected statements that constitute the theory under test, initial conditions such as the previous positions of the planet and the sun; auxiliary assumptions such as those enabling corrections to be made for the refraction of light from the planet in the earth's atmosphere, and so on. Now if the prediction that follows from this maze of premises turns out to be false...then all the logic of the situation permits us to conclude is that at least one of the premises must be false. It does not enable us to identify the faulty premise. It may be the theory under test that is at fault, but alternatively it may be an auxiliary assumption or some part of the description of the initial conditions that is responsible for the incorrect prediction. A theory cannot be conclusively falsified because the possibility that some part of the complex test situation other than the theory under test is responsible for an erroneous prediction cannot be ruled out.[177]

A good example showing the complex status of experimental tests settling scientific issues can be taken from the contemporary debate surrounding the health and safety of EMF.[178] The parties to the dispute are highly scientifically polarised and, what is for one side of the debate a convincing experimental test enhancing the plausibility of links between EMFs and ill health is, to the other side of the debate, an unconvincing experimental error or artefact.

One experiment in question involves the movement of calcium ions across the cell membrane of brain cells in tissue cultures exposed to weak EMFs. This effect has been observed to occur with exposure to field levels putatively too weak to cause heating or some direct energy effect on the cells. Further, the effect appears to be 'information related' (responsiveness seems to be dependent on wave form and frequency rather than strength of the field).[179] This experiment is seen to be important because, if accepted, it challenges the scientific rationale on which most EMF health and safety regulations have been framed. The rationale has been that the only relevant biological effects of EMF are those linked to direct energy transfer to living things, such as electrocution or

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177 *Ibid* at 61; I Lakatos, "Falsification and the Methodology of Scientific Research Programmes" in I Lakatos and A Musgrave (eds), *Criticism and the Growth of Knowledge* (1970) p 130; Albury note 123 *supra* at 12-15; Oldroyd note 145 *supra* at 302-4; J Schuster, *An Introduction to the History and Social Studies of Science*, University of Wollongong (1995) pp 110-15; Mulkay note 120 *supra*.

178 Mercer note 160 *supra*; L Dalton, *Radiation Exposures*, Scribe Publications (1991); US Office of Technology Assessment Report, *Biological Effects of Power Frequency, Electric and Magnetic Fields*, May 1989; US Department of Energy, *Questions and Answers about EMF: Electric and Magnetic Fields Associated With the Use of Electric Power*, 1995.

179 WR Adey, "The Energy Around Us" in *The Sciences*, New York Academy of Science, (1986) pp 53-8; *Id*, "Biological Effects of Radio Frequency" in J Lin (ed), *Interaction of Electromagnetic Waves with Biological Systems* (1988); Mercer *ibid* at 61-68.

heating - effects occurring at levels of exposure well beyond those implicated in the calcium movement experiment.[180]

Supporters of this scientific rationale have rejected the relevance of the calcium movement studies on numerous grounds. Some have argued that the effect defies the laws of physics and, in the absence of a plausible alternative physical explanation, the effect must be the result of some yet to be identified failure in experimental control (magnite contamination of tissue cultures being one suspect).[181] Alternatively, the argument has been put forward that researchers identifying the effect have been guilty of self deception, something seen as possible given the highly emotionally charged nature of the EMF debate and the subtle measurements involved. Yet others have argued that the effect may exist but that it is impossible to extrapolate from the laboratory to the 'real' world. They argue that the experiment provides no evidence for the movement of 'biologically relevant calcium'.[182]

Proponents of the calcium movement experiment have argued that opposition (critique of their experimental work) has been heavily politically motivated. The huge potential costs of stricter EMF regulation has meant that critics have found it easy to get their work funded and published. They have also argued that the narrow specialisation of bio-physics means that many of their critics simply do not have the appropriate training to understand the sophisticated bioelectromagnetic processes involved.[183] All these claims and counter claims have been set against debates about the status of various researchers.

The example of the EMF debate indicates how irrelevant Popper's abstract theorising is to living scientific debates. The calcium movement experiment can in no simple way decide the issues at stake. The complex test situation is prone to deconstruction and various constructions of its significance, according to webs of subsidiary hypotheses and the competing theoretical vantage points of the antagonists. What is more, a Popperian gloss could be used by either side to give their claims authority. For the proponents of the calcium movement experiment, traditional 'energy' viewpoints of EMF have been falsified and a vista of a new branch of science opened. For the more dismissive opponents, it could be argued that the hypothesis, that calcium movement was indicative of alternative mechanisms of EMF biological interactions, was not framed in sufficiently clear physical terms (according to their understanding of the laws of physics) to be properly falsifiable and was therefore unscientific.

180 L Dalton, "EMR Exposures: Setting the Standards" (1991) 19 *Habitat* 8-9; I MacMillan, *Electromagnetic Fields: Electric Power and Public Health*, Collingwood Community Health Centre (1987); DW Mercer, "Limits to Exposure", Letters, *New Scientist*, (May 7, 1992).

181 W Bennett, *Health and Low Frequency Electromagnetic Fields*, Yale University (1994) pp 12-16, 136-9.

182 Electricity Commission of NSW, *Submission to Inquiry into Community Needs: High-Voltage Transmission Line Development* (Gibbs Inquiry) August 1990.

183 RW Adey, Transcript of Radio Interview on Radio National Science Program (May 6 1987), see also Adey cited in P Brodeur, *Currents of Death*, Simon & Schuster (1989) pp 99, 216-17.

(ii) Problems with Falsification from the History of Science

Apart from these philosophical problems, the credibility of falsification is also strongly challenged if we examine the history of science. Studies in the history of science suggest falsificationism is too strict a demarcation criterion. Such a criterion would have denied the development and acceptance of many major scientific theories in their own times. A common feature of novel approaches in science has been the selective temporary suspension of concern with contradictions and 'falsifying' instances. A notable example is Copernicanism.

In the case of the Copernicanism, a number of pieces of observational evidence of the time could have been classed as strong falsifications in a Popperian sense. As well as problems with the observed sizes of the planets noted above, there were numerous 'physical problems' attached to the Copernican idea of a moving earth. Opponents of Copernicanism pointed out that logically you could draw and test the prediction from Copernicus' work that if the earth were spinning and an object were dropped from a tower, the earth would spin whilst the object was falling and the object would therefore reach the ground a considerable distance to the west of the point from where it was dropped (the earth spins to the east).[184] In a strict Popperian framework forbidding the temporary suspension of doubt, or the use of *ad hoc* hypotheses to save a theory, this false prediction should have marked the rejection of Copernicanism. Rather than reject Copernicanism, for many, the issue of explaining terrestrial motion within a Copernican context became an important stimulus to the development of new theories such as Gilbert's belief in celestial magnetism[185] and Galileo's ideas of the earth as an inertial system which were proposed over half a century later. Ultimately, Newton reconceptualised the issues in his theory of universal gravitation. Shelving a falsification until theory 'catches up' is strictly anti-Popperian but captures the patterns of growth of many areas of knowledge in the history of science.

Apart from the numerous cases where the falsification of a theory has been fruitfully ignored, there are also many instances where scientifically accepted propositions would not pass a strict test of being falsifiable. An instructive debate in the context of the latter has surrounded the potential falsifiability of Darwin's proposition that the causal motor for the theory of natural selection was the 'survival of the fittest'. A number of philosophers have noted that the concept 'as it has most commonly been interpreted' (that is, as a scientific law rather than a probabilistic generalisation) is unfalsifiable.[186] Olroyd provides an example in the following terms:

> It is usually objected that the Darwinian theory is infalsifiable, because there is no criterion of the fitness of the organisms other than their survival. The group of organisms that survives *must,* by definition have been the fittest. Putting it another way, it is claimed that the expression the survival of the fittest is tautological...So

184 Kuhn note 118 *supra*; Schuster note 177 *supra* at 110-111; Chalmers note 147 *supra*; Koestler note 123 *supra*.

185 G Edmond, "An Attraction for Copernicanism: Reclaiming Gilbert's *De magnete* for the New Historiography of Science", unpublished (1992 Honours Thesis, University of Wollongong).

186 CH Waddington, *The Nature of Life*, Unwin Books (1963) p 79.

> the question comes back to the problem of whether there are, or can be, criteria for the fittest other than that of survival. Can we set up any kind of experimental situation whereby we may test whether the fittest do in fact survive? For if this cannot be done, it would seem that this part of the theory is unfalsifiable, and according to the well-known criterion of Sir Karl Popper this would mean that the principle is not a component of a *bona fide* scientific theory. In Popper's language, the theory of evolution would be pseudo-scientific.[187]

Considerable debate has ensued concerning whether it is possible to reconstruct survival of the fittest into falsifiable terms, what sort of status the proposition should have if it cannot be reconstructed, and, most importantly for our purposes, given the 'scientific utility' of the proposition, what sort of status should the concept of falsification have.[188]

Considering these brief examples as just a small representative sample from the history of science, it is clear that falsificationism is a philosophy of method that simply ignores important dynamics involved in the development of scientific knowledge. Many historians and philosophers of science have long noted that every particular area of scientific knowledge has its own deeply embedded basic sets of assumptions that are not open to falsification. Such baseline 'metaphysics' involve implicit theories of causality, standards of proof, laws, models and so on.[189] Laudan, in a critique of the elevation of the status of falsification as a central defining feature of science in the *McLean v Arkansas Board of Education*[190] creationism case (discussed further below at Part (iii)), describes these limitations of falsification succinctly:

> ...historical and sociological researchers on science strongly suggest that the scientists of any epoch likewise [like creation science] regard some of their beliefs as so fundamental as not to be open to repudiation or negotiation. Would Newton, for instance be tentative about the claim that there were forces in the world? Are quantum mechanicians willing to give up the uncertainty relation? Are physicists willing to specify circumstances under which they would give up energy conservation? Numerous historians and philosophers of science (for example, Kuhn, Mitroff, Feyerabend, and Lakatos) have documented the existence of a

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187 Oldroyd note 145 *supra* at 125.

188 KK Lee, "Popper's Falsifiability and Darwin's Natural Selection" (1969) 44 *Philosophy* 291. Popper held a variety of positions throughout his career in relation to the scientific status of natural selection. See KR Popper, "Natural Selection and the Emergence of Mind" (1978) 32 *Dialectica* 339.

189 These concepts pre-date the well known post-Kuhnian debate about paradigms and have a long lineage in the history and philosophy of science. See RG Collingwood, *The Idea of History*, Clarendon Press (1946); *Id*, The *Idea of Nature*, Clarendon Press (1946); EA Burtt, *The Metaphysical Foundation of Modern Physical Science: A Historical and Critical Essay*, Routledge & Kegan Paul (1932); EW Strong, *Procedures and Metaphysics: A Study in the Philosophy of Mathematical Physical Science in the Sixteenth and Seventeenth Centuries*, University Microfilms International (1937); A Koyré, *Metaphysics and Measurement: Essays in Scientific Revolution*, Chapman & Hall (1968). Similar notions have been developed by Michel Foucault, *Archaeology of Knowledge*, Tavistock (1972).

190 529 F Supp 1255 (1982). See also for example: M Mulkay, "Applied Philosophy and Philosopher's Practice" (1981) 6 *Science, Technology, & Human Values* 7; L Laudan, "Commentary: Science at the Bar - Causes For Concern" (1982) 7 *Science, Technology, & Human Values* 16; M Ruse, "Response to Commentary: *Pro Judice*" (1982) 7 *Science, Technology, & Human Values* 19; M La Follete, "Creationism, Science, and the Law" (1982) 7 *Science, Technology, & Human Values* 9; B Gross, "Commentary: Philosophers at the Bar - Some Reasons for Restraint" (1983) 8 *Science, Technology & Human Values* 30; M Shermer, "Science Defended, Science Defined: The Louisiana Creation Case" (1991) 16 *Science, Technology & Human Values* 517.

certain degree of dogmatism about core commitments in scientific research and have argued that such dogmatism plays a constructive role in promoting the aims of science.[191]

These critiques accept that various forms of 'falsification' and 'testing' might play some role in the development of many areas of knowledge. However, they suggest that such processes are informal, open to social negotiation and interpretation, and subject to such considerations as: prior agreement on what is actually being tested, agreement about the nature of a test situation, and agreement on the outcome of a test situation.

(iii) Pragmatic Difficulties in Putting Falsification to Work

A further limit on the utility of falsification as a demarcation criterion between science and 'pseudo-science' surrounds the pragmatic difficulties involved in testing all the bodies of knowledge aspiring towards being granted the status of 'science'. Modern science exists in a budgetary and political context where it is simply not possible to imagine every hypothesis/conjecture being subject to test. Albury has noted, without necessarily supporting the substance of their claims, that popular examples of 'pseudo scientists', Lysenko and Velikovsky, were both able to generate potentially falsifiable hypotheses. In both cases there was an extreme reluctance to test hypotheses of these putative sciences. Apart from their respective deviations from accepted theory, Lysenkoism was rejected in the West because of its overt political link to Stalinism, and Velikovsky because of the boldness of his claims and the huge costs involved in setting up potential tests for them.[192]

Drawing from this work, it is obvious that the Popperian image of science involving the testing of the boldest possible conjectures is utopian. At times, what is classed as orthodox science will be based on untestable and untested theoretical presuppositions and what is relinquished to the status of 'pseudo-science' will rely on potentially falsifiable hypotheses. It is interesting to consider the way a 'post *Daubert* court' might grapple with the question of the admissibility of hypothetically testable, but practically difficult to test and theoretically 'fringe' bodies of knowledge such as claims of a latter day Lysenko or Velikovsky or Galileo. A related question surrounds the flexibility with which the ascription of falsifiability can be attached to knowledge claims, and its potential to be a 'double edged sword'[193] in terms of the kinds of knowledge to which it is able to grant or deny the ascription of 'science'. An informative debate in relation to this topic surrounded the use of falsification in legal proceedings as a criterion to prevent Creationism being ascribed the status of a *bona fide* science.

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191 Laudan *ibid* at 163. A similar point is made by Oldroyd note 145 *supra* at 315.

192 Albury note 123 *supra*; R Lewontin and R Levins, "The Problem of Lysenkoism", in H Rose and S Rose (eds), *The Radicalisation of Science* (1976).

193 The more subtle philosophical issues involved in applying general rules to specific areas of human conduct are discussed in more detail in the work of Mulkay and Gilbert. See for example note 146 *supra*.

In *McLean*, District Judge William Overton was called on to adjudicate the constitutional validity of the *Balanced Treatment for Creation-Science and Evolution - Science Act,* Arkansas (1981). The case hinged on whether the statute violated the US First Amendment safeguard against 'establishment of religion'. One of the tests for satisfying the First Amendment was that the "principal or primary effect" of the statute "must be one that neither advances nor inhibits religion". Judge Overton argued that the statute failed this test because Creationism did not satisfy the "essential characteristics of science". This meant that the statute could not have the effect of advancing science and therefore: "the conclusion is inescapable that the only real effect of [the statute] is the advancement of religion".[194] For these essential characteristics of science Overton J drew from a submission by the philosopher Michael Ruse. The criteria endorsed stipulated that a science must possess the following five interlocking features:

> [1] It is guided by natural law; [2] It has to be explanatory by reference to natural law; [3] It has to be testable against the empirical world; [4] Its conclusions are tentative, ie, are not necessarily the final word; and [5] It is falsifiable. (Ruse and other science witnesses)[195]

Philosophers Laudan and Quinn voiced serious concerns about this definition of science and the need for philosophers of science to display caution when striking out into broader fields of discourse without acknowledging internal disagreements within their own ranks.

As far as the 'definition' was concerned, the viability of falsification was exposed to sustained attack. Laudan noted that it was incorrect for Overton J and Ruse to suggest that Creationism did not make a number of testable assertions about empirical matters of fact.[196] For instance, their suggestions about the recent origin of the earth, their claims that the geological features of the earth's surface are consistent with a huge Noachian deluge and that the human and animal fossil records are co-extensive could be reconstructed to be testable.

Further, Laudan challenged Overton J and Ruse's suggestion that Creationism is unscientific because of its refusal to modify its views in relation to new 'evidence'. Laudan pointed out that Creationists could claim to have changed their views over time in the light of new scientific evidence in certain contexts, for instance in relation to the amount of variability allowed in species change.[197] In addition, Laudan noted that whilst Creationism does contain a number of

194 Judge Overton quoted in WA Thomas "Commentary: Science v Creation Science" (1986) 11 *Science, Technology, and Human Values* 47-51.

195 *McLean* note 190 *supra* at 1267. Black note 93 *supra* at 684-5. Black contends that: "The decision in *McLean v Arkansas Board of Education* provides a particularly thoughtful and thorough analysis of how science works and how courts can distinguish it from efforts to clothe religious beliefs in scientific garb...the *McLean* court *easily focused* on the scientific deficiencies of creationism and rejected it." [italics added]

196 Laudan note 190 *supra* at 16.

197 *Ibid* at 17.

untestable assumptions in relation to Biblical authority, the presence of untestable assumptions is a feature shared with many branches of science.[198]

By noting the potential falsifiability of a number of Creation science's central claims, Laudan should not be confused as a supporter. Rather, he is concerned that by arguing the fundamental inadmissibility of Creation science on the criterion of falsifiability, the opportunity to expose the poor fit of Creation science with the existing evidence is lost:

> Rather than take on the Creationists obliquely and in wholesale fashion by suggesting that what they are doing is "unscientific" *tout court* (which is doubly silly because few authors can even agree on what makes an activity scientific), we should confront their claims directly and in piecemeal fashion by asking what evidence and arguments can be marshalled for and against each of them.[199]

Ironically, Laudan fears that by focusing on falsification it could actually be easier for Creationism to claim the status of 'valid' science.

Finally, whilst Laudan concedes that Creationists may well display a more overt dogmatism about their core assumptions than many branches of establishment science, he argues that to focus on this is to conflate *ad hominem* considerations with theories of scientific method: "...the *ad hominem* charge of dogmatism against Creationism egregiously confuses doctrines with proponents of those doctrines".[200] In the following section this tendency for falsification to be used as a normative guide (that is a way of judging the conduct and attitude of scientists) rather than as a logical methodological rule will be explored in detail.

Quinn comments on the *McLean* proceedings at a broader level, largely supporting Laudan's commentary. Quinn makes additional observations about the inconsistent use of falsification by a number of leading scientific witnesses. He notes that Gould claims that: 'scientific creationism' is a self contradictory nonsense phrase precisely because it cannot be falsified" and presents "no testable alternative". But then immediately follows, in the next sentence, by contradicting himself asserting: "the individual claims [of Creationism] are easy enough to refute with a bit of research." Quinn comments:

> ...this glaring inconsistency is the tip off to the fact that talk about testability and falsifiability functions as verbal abuse and not as a serious argument in Gould's anti-creationist polemics.[201]

The problem highlighted by Quinn in Gould's use of falsifiability is clearly displayed in Odgers and Richardson's work.

An important point for Odgers and Richardson is that falsifiability provides a key resource for denying certain types of psychological and psychiatric evidence the status of scientific evidence. They are particularly concerned with the status of syndrome evidence.[202] Drawing support from Underwager and Wakefield it is noted that "many of the syndromes are based on Freudian theory" and that

198 See Part VI(B)(ii). Consider also T Pinch, *Confronting Nature: The Sociology of Solar-Neutrino Detection*, D Reidel Publishing (1986).

199 Laudan note 190 *supra* at 18.

200 *Ibid* at 17.

201 P Quinn, "The Philosopher of Science as Expert Witness", in J Cushing, C Delaney and G Gutting (eds), *Science and Reality: Recent Work in the Philosophy of Science* (1984) pp 32, 43-4.

202 Richardson, Ginsburg, Gatowski and Dobbin note 5 *supra*.

"American psychiatry is largely Freudian".[203] We are then told that Popper demonstrated, "at some length", the unfalsifiability of Freudian theory. Odgers and Richardson conclude that psychiatric testimony based on such principles might now be inadmissible as scientific evidence. Odgers and Richardson continue, as did Gould, using falsification as an inconsistent rhetorical device. They quote Underwager and Wakefield for the forceful proposition that one of the central features of Freudian theory, the concept of repression, has been falsified:

> Faced with the massive weight of over sixty years of research that falsifies the concept of repression, a reasonable judge must rule that testimony based on the concept is unscientific.[204]

Odgers and Richardson clearly do not fully understand the concept of falsification. They commence using Freudian thought as one the "best examples of unfalsifiable theories" and then proceed to argue that "there is considerable evidence falsifying the Freudian concept of repression".[205]

Further evidence of Odgers and Richardson's failure to understand falsification, and one shared in the *Daubert* judgment itself and by a variety of commentators, is the tendency to treat falsification as an interchangeable part of a matrix of criteria for establishing the methodological validity of science.[206] It will be remembered that whilst the *Daubert* judgment emphasises the centrality of falsifiability it simultaneously places it alongside subsidiary criteria including "known and potential error rates", "peer review" and "general acceptance".[207] It should be remembered that the stricter version of Popper's falsification was derived in opposition to these forms of traditional empiricism and sociological considerations and has maintained its claim to superior status according to its ability to transcend them. The rhetorical clarity and strength of strict falsification is obviously seductive, but it is equally obvious that as soon as practical considerations concerning the admissibility of scientific evidence are encountered, the vaguer, weaker subsidiary criteria are required for meaningful discussion.

The flexible and inconsistent ways that Odgers and Richardson hope to apply falsification raises serious doubts about the transferability/portability of falsification, as well as their competence in fulfilling their self appointed role to educate the judiciary about science.[208] In this context we concur with Laudan's observations on the use of falsification in *McLean*:

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203 Odgers and Richardson note 5 *supra* at 119. No evidence for this proposition is provided.

204 R Underwager and H Wakefield, "New Paradigm in Evidence Law" (1993) 10 *Issues in Child Abuse* at 164-165 cited in Odgers and Richardson note 5 *supra* at 121.

205 Odgers and Richardson note 5 *supra* at 119.

206 See for example: Bourke note 94 *supra* at 191; Magnusson and Selinger note 71 *supra* at 92; Hutchinson and Ashby note 4 *supra* at 1885.

207 Odgers and Richardson note 5 *supra* at 115-116.

208 Elsewhere Odgers and Richardson state that: "an important component of judges' scientific literacy is their understanding of the principle of falsifiability" and "*Daubert's* criteria for admission of scientific evidence are probably not well understood or appreciated, requiring a considerable effort to educate lawyers and judges". Odgers and Richardson note 5 *supra* at 12, 16.

> It simply will not do for the defenders of science to invoke philosophy of science when its suits them (eg, their much loved principle of falsifiability comes directly from the philosopher Karl Popper) and to dismiss it as "arcane" and "remote" when it does not. However noble the motivation, bad philosophy makes for bad law.[209]

(iv) Falsification: An Inconsistent Doctrine

Apart from the philosophical, historical and practical difficulties surrounding falsificationism there are, as hinted at in our earlier discussion, a number of problems associated with its internal consistency. The problem, captured by Ravetz, was that Popper:

> ...was, of course, totally unclear on whether the search for refutations was a matter of logic, practice or motivation, and on whether it was characteristic of all actual science or only the best.[210]

On deeper examination, it is not clear how falsification should work in practice. A number of commentators have noted that in the wake of criticism, in particular, following from the work of Kuhn, Feyerabend and Popper's eminent student Lakatos,[211] Popper made a number of concessions concerning how strictly the criterion of falsification could be applied to practical scientific contexts. In the weaker versions of falsification which remained, Popper left considerable room for falsification to be interpreted merely as an ethical guide, a set of prescriptions which scientists should attempt to follow if they were to encourage scientific progress, rather than as a strict logical principle, fundamental to guaranteeing the unique epistemological status of science.[212] This moralistic, normative dimension to Popper's work conformed to his Cold War inspired aspirations to celebrate the superiority of 'Liberal Democracy' or the so called 'Open Society' over its 'Fascist' and 'Communist' alternatives.[213] The conduct of scientists displayed by icons such as Einstein, and the critical open minded attitude they supposedly embodied with the method of falsificationism exemplified the ideals that 'Liberal Democracies' were built upon and the ideals that 'Fascist' and 'Communist' regimes suppressed.[214] Ravetz describes this dimension of Popper's work in the following terms:

> Popper can be seen as the last deep philosopher who espoused science as the embodiment of virtue. Indeed, his great philosophical insight, abandoning verification for falsification, can be seen as a heroic gesture of jettisoning Science as True in order to rescue Science as Good. The whole Einstein fable displays Popper as a moralist rather than an epistemologist, and Popper himself never undertook a historical case study of a refutation in science.[215]

209 Laudan note 190 *supra* at 19. It is worth noting that not all the perspectives on science developed in our discussion in this paper would follow Laudan's methodology of science. We concur not so much in his belief in what science is, but in his clear identification of the problems of applying falsification. For a brief overview of Laudan's work in the philosophy of science see P Riggs, *Whys and Ways of Science*, Melbourne University Press (1992) pp 174-184.

210 J Ravetz, "Learning from Negative Instances" (1995) 70 *Radical Philosophy* 5.

211 Lakatos and Musgrave note 177 *supra*.

212 Oldroyd note 145 *supra* at 308-315; Schuster note 177 *supra*; Mulkay and Gilbert note 146 *supra* at 398.

213 K Popper, *The Open Society and its Enemies*, Routledge and Kegan Paul (1945).

214 Albury note 123 *supra*.

215 Ravetz note 133 *supra* at 5-6.

At a more specific level, the 'weaker Popper' was unclear about the role of 'conventionalising' theories. That is, where there are contexts in which it should be admissible for scientists to use *ad hoc* hypotheses to avoid enforcing falsifying evidence against a theory. In these 'weaker' versions of Popper, answers to questions such as when to falsify or not to falsify, and what makes a good/bad *ad hoc* hypothesis,[216] are being provided by scientists *in situ* rather than by philosophers (or judges). This means that the logical purity and neat transferability/portability of falsification as a demarcation criterion is lost.

Recognising these inconsistencies in falsification would leave Odgers and Richardson, and others who champion it as a tool for judicial reform, with a dilemma: Which version of falsification should judges attempt to apply? One that is strict and full of philosophical pitfalls; or one that is weak but with fewer pitfalls? Further, if the weaker version is adopted, *Frye* type concerns are reintroduced because 'judicial gate keepers' would need to defer to the judgments of scientists to ascertain: the 'details' of which parts of a theory can be seen as 'conventionalised' and immune from falsification; which parts can be seen as falsifiable; and whether or not according to peer judgment a scientist had displayed the appropriate critical attitude. Another dilemma for Odgers and Richardson in this context surrounds the question that, if as they suggest, judges are to learn Popper, which Popper should they learn? Should they learn the simple programmatic Popper drawn from a 'lean' sample of his work, or should they be required to immerse themselves in the intellectual intricacies required to better appreciate Popper's work.[217] In doing the latter, judges would be required to consider the 'early' Popper and his brilliant challenge to traditional empiricism and logical positivism in *The Logic of Scientific Discovery*, the 'middle' Popper as the Cold War political apologist for the West, and the zenith and nadir of the 'later' Popper in the intellectual milieu of deconstruction of falsificationism.[218] Such problems give considerable credence to the concerns voiced by Rehnquist CJ and Stephens J in their dissenting judgment in *Daubert*.

(v) Popper as a Flexible Part of the Rhetoric of Science

Another dimension surrounding the applicability of falsification as a demarcation criterion flows from the flexible ways that such a method doctrine can be interpreted to operate in real world contexts. Similar to his critique of Merton's 'norms of science', Mulkay[219] (and Gilbert) have also investigated the way scientists themselves perceive and use Popper's idea of falsification in their work. A number of scientists in a research network were interviewed by

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216 Lakatos expanded on these concerns at length, and attempted to reformulate a sophisticated 'watered-down' variation of falsificationism. His own version of falsificationism has not received great popularity, being described by many critics as complex, ambiguous and ultimately more normative than logical. See Oldroyd note 145 *supra* at 327-33.

217 Mulkay and Gilbert note 146 *supra* at 391.

218 DC Stove, "Popper on Scientific Statements" (1977) 55 *Philosophy* 81. It is important to acknowledge that Stove's critique of Popper comes from a vantage point diametrically opposed to the traditions of Kuhn and Feyerabend.

219 See note 146 *supra*.

Mulkay. Whilst all members of the research network were familiar with the general idea of falsification:

> Almost without exception, scientists stressed that, although there was a great deal of *talk* about Popper, there was actually little conformity to his rules. It was generally agreed that Popper had exerted little influence on the conduct of research.[220]

The most common context where actions of scientists were judged against an image of Popperian standards was in scientific disagreements. Scientists claimed that their actions complied with Popper but that those of their opponents did not. Interestingly, the same scientists did not claim that Popper had actually guided their deliberate conduct, rather they were able to reconstruct a particular piece of scientific work according to what they believed Popper's rules implied. Mulkay further noted that in such disagreements, different scientists appeared to "derive totally different actions from the rules...[that they]...appeared to mean all things to all scientists"[221] and that whilst "Popper's rules operate with terms like "disprove", "falsification, and *ad hoc* modification", the specific meaning of these terms is *entirely given* for individual scientists on particular occasions by their technical judgments".[222] Like Merton's norms of science, Popperian methodology becomes part of the informal vocabulary scientists use to negotiate the meaning of each others' activities.

Mulkay's work also addresses some deeper difficulties associated with the rationale of the whole Popperian exercise, difficulties also applicable to much of the philosophy of science and particular styles of their application such as *Daubert*. Mulkay notes that the Popperian rationale concentrates on the end products of scientific practice, working backwards from completed bodies of knowledge with prior knowledge of the 'correct' outcome. With these factors in mind, the rules that ought to have been applied can supposedly be formulated *ex post facto*. To quote Mulkay:

> When we look closely at examples where Popperian rules are applied, retrospectively in the philosophical literature to generate correct scientific answers, we find that the analysts draw continually on their prior scientific knowledge to identify Popperian and non Popperian actions. Popperian rules appear to work in some philosophical analyses because analysts have at their disposal an interpretive procedure denied to the practicing researcher, namely, that of identifying rational, Popperian courses of action through their connection with the intellectual outcome known in advance. Without this prior knowledge of the right answer and without this interpretive procedure it would be impossible for them to specify which course of action participants should have chosen.[223]

Odgers and Richardson would appear to fall victim to the kind of *ex post facto* reasoning exposed by Mulkay. In their article, Odgers and Richardson draw on examples from the history of science to support falsification. Mendel's work is represented as an exemplar of the careful Popperian testing of ideas derived by experimentation, whereas Lysenko's genetics and the eighteenth century practice

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220 M Mulkay, "Applied Philosophy and Philosophers' Practice" (1981) 6 *Science, Technology, & Human Values* 13.

221 *Ibid.*

222 *Ibid.*

223 *Ibid* at 41.

of blood-letting as a cure for diseases (and the cause of the death of George Washington) are employed as examples of unfalsifiable, non-scientific hypotheses.[224] These judgments are buttressed by evaluating these bodies of knowledge and practice with the benefit of hindsight.[225]

Judging the validity of the knowledge claims in question becomes much more difficult if we temporarily cast aside our current knowledge. For instance, if we place Mendel into his own historical context it becomes much more complicated to interpret him as a scientist using falsification or as some type of 'obvious' founder of modern genetics.[226] There has, in fact, been historical work suggesting that Mendel or an assistant may well have manipulated their recorded results of hybridisation to fulfil Mendel's theoretical expectations.[227] Further, Mendel's work only proved to be 'valuable' after half a century of work in neighbouring areas of intellectual inquiry such as evolutionary and molecular biology. A more nuanced historical account would acknowledge that some of the theoretical areas of knowledge contributing to modern genetics may well have been, in Odgers and Richardson's terms, unfalsifiable in their own context (for example, survival of the fittest and the principle of natural selection). It is easy to see Mendel as an example of 'good' science if we have a prior conception of the influence of his work and no conception of his work in its own historical, scientific and social context.

Similarly, whilst Lysenkoism is an example of gross social shaping of science by the state,[228] one of its central notions, the inheritability of acquired characteristics, harks back to the scientific tradition of Lamarck and still features as a theme in some 'established' areas of contemporary genetics.[229] Furthermore, Odgers and Richardson's argument that Lysenkoism was unfalsifiable is largely based on retrospective knowledge of its lack of technical efficacy, something open to debate in its own context. Various forms of

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224 This practice bears close resemblance to the approach adopted by Huber note 3 *supra*. It is also interesting to note the moralistic element brought into play. Not only are the bad knowledge systems in question unfalsifiable, they are also morally reprehensible. Lysenko is mentioned only in scorn and blood-letting caused nothing less than the death of a US President! Whiggish historical examples feature regularly in the legal scientific expert evidence literature. See Black, Ayala and Saffran-Brinks note 4 *supra* at 766-73; Farrell note 4 *supra* at 2189.

225 H Butterfield, *The Whig Interpretation of History*, Bell (1931); AR Hall, "On Whiggism" (1983) 21 *History of Science* 45; EH Carr, *What is History?*, MacMillan (1986); H Kearney, *Origins of the Scientific Revolution*, Longman (1964); K Jenkins, *Rethinking History*, Routledge (1991).

226 For an interesting discussion of the re-shaping of Mendel's work in different contexts see B Barnes, D Bloor and J Henry, *Scientific Knowledge: A Sociological Analysis*, University of Chicago Press (1996) pp 95-102. For a similar discussion on the historiographical problems of retrospectively identifying supposed progenitors of modern ideas see G Edmond, "The Freedom of Histories: Reassessing Hugo Grotius on the Seas" (1995) 2 *Law/Text/Culture* 49.

227 Mulkay note 120 *supra* at 50-2; Oldroyd note 145 *supra* at 172.

228 It is our position that there is never some kind of complete insulation of science from society. The situation surrounding Lysenko was extreme because of the total overpowering of long-standing subcultures and research traditions in biology for short term State goals. See Ruse note 190 *supra*.

229 M Parascandola, "Philosophy in the Laboratory: The Debate over the Evidence for EJ Steele's Lamarckian Hypothesis" (1995) 26 *Studies in the History and Philosophy of Science* 469 at 482-4, especially 489. For a discussion of Popper's role in this debate see Oldroyd note 145 *supra* at 172.

previously accepted knowledge, hypothetically falsifiable, could also be challenged in their failure to contribute to 'technological development'.

Finally, to argue that blood-letting was clearly unfalsifiable denies that there was any theoretical context in which the notion operated. Many areas of modern medicine could be evaluated in the near future as unfalsifiable if they were to be removed from their theoretical contexts. Even in recent times there have been surgical practices which, if evaluated in the same manner as the isolated example of George Washington, could be seen as unfalsifiable. For instance, the death of a patient after heart by-pass surgery is not used as a Popperian falsification of the therapy. The politics, intricacies and uncertainties involved in testing medical therapies have been essayed at length by Richards.[230]

VII. CONCLUSION

Our discussion began by recognising perceptions of protracted difficulties associated with the admission of expert scientific evidence to legal settings. These difficulties extend far beyond courts and into regulatory culture.

Despite acknowledging the importance of such perceptions we have not explicitly endeavoured to offer any single solution to the problems facing law and science. One of the implications of the approach adopted in this paper is that there is no single resolution to 'the law-science problem' nor is there any single problem to start with. In our discussion we have emphasised, following a number of writers in HPS/SSK, that science cannot simply be defined by its possession of a unique transferable method or set of behavioural norms or institutional structures. We noted the importance to scientists of tacit knowledge and skilled judgments, and the diversity of norms and institutional constraints under which scientists work. In this context, attempts such as falsificationism, to define a set of transferable rules for what constitutes valid science will always face difficulties accounting for the diversity of ways such rules can be interpreted and applied in practical contexts.

We also deconstructed Popper's methodology of falsification in some detail, highlighting its inconsistencies, philosophical problems, historical implausibility and its difficulties in application to practical contexts. We believe that providing such a detailed critique was warranted considering the pedagogical aspirations of Odgers and Richardson who seem enthusiastic in promoting a particularly shallow and inaccurate reading of falsification.

The similarities between the use of the rhetoric of scientific method and the rhetoric used to insulate legal discourse from similar questions concerning the local construction of the application and meaning of rules were also noted. Such professional rhetorics deflect attention from examining the more local processes involved in scientific, legal, and scientific/legal, decision making. The importance of examining the more local processes involved in the construction

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230 E Richards, *Vitamin C and Cancer: Medicine or Politics?*, MacMillan (1991); M Mulkay, "Knowledge and Utility" (1979) 9 *Social Studies of Science* 69.

of scientific-legal knowledges, and not using *a priori* categories of science and law, was highlighted by consideration of the profusion of numerous sites where law and science have been brought together into complex hybrid forms with their own institutional and knowledge making character.

Considering the points noted above, a number of criticisms can be made in relation to the rationale embodied in *Daubert* and its ability to fulfil its implicit policy objectives. As far as its rationale is concerned, it would appear that the US Supreme Court, and *Daubert* apologists such as Odgers and Richardson, in specifying the simple essence of falsification as the main solution to the purported problem of the admissibility of scientific evidence, have provided an analysis of the issues that fails to delve beneath the veneer of the professional rhetorics of law and science. They have framed the problem in ideal terms of how to maintain scientific integrity in legal contexts, taking for granted law and science as reified categories. For them the issue becomes: how can science and law by policy intervention be returned to their ideal states? As such their analyses lack the degree of ethnographic detail that would be required to begin to constructively address the complex issues involved in the law-science relationship.

Writers such as Wynne and Jasanoff have argued that it is important for policy makers addressing science and the law to unpack the more specific processes involved in the construction of scientific and legal knowledges taking both categories as dynamic and contingent.[231] These approaches do not disqualify making policy generalisations about law-science, but imply that such generalisations should be reflective of the politics involved in the particular settings in which any policy recommendation is applied and avoid epistemologically oriented *a priori* images of science and law. The celebration of *Daubert* and the 'intrinsic' evaluation of an *a priori* image of science within legal contexts discourages the more nuanced reflective and 'thick' descriptive approaches advocated in such literatures, (something quite ironic considering the US Supreme Court claimed to have been guided by at least some of this literature in formulating the *Daubert* judgment).

Whilst *Daubert* employs reified images of science, scientific method (falsification) and law, this does not mean it will not have some impact on legal practices involving science. In the first instance, it does represent a significant shift in the rhetorical resources available to those involved in the "co-production" of scientific-legal knowledge.[232] An important issue to remember is that the actual effects of *Daubert* will depend upon how it comes to be interpreted and applied in specific contexts.

We could imagine that similar processes will be encountered as *Daubert* comes to be applied in a variety of contexts. As one US commentator has noted, the purported rigid application of some version of falsification could result in not only the elimination of certain types of syndrome and other allegedly non-

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231 B Wynne "SSK's Identity Parade: Signing-Up, Off-and-On" (1996) 26 *Social Studies of Science* 357; and Jasanoff note 105 *supra*.

232 Jasanoff *ibid*.

scientific evidence, but it could also restrict the access of many types of orthodox forensic practices which could not hope to meet falsifiable criteria. These would include techniques such as fingerprints, ballistics, bite-marks, and handwriting to name but a few.[233] Exemplifying the argument made above, the same commentator noted that there was already pressure for educational programs to be developed to assist the repackaging of forensic knowledge claims to conform with *Daubert's* falsificationist stipulations. In the short term, *Daubert* is likely to represent a resource to exclude scientific evidence 'perceived' to be controversial (for example toxic tort aetiology) by providing a mechanism for judges to justify a stricter criteria for valid science than was previously available through *Frye*. However, just as orthodox forensic scientists are beginning to reshape their knowledge claims to be more tractable in a post-*Daubert* legal environment, it could be expected that similar patterns will emerge in the packaging of toxic tort claims.

Another interesting question involves the degree to which there has been a shift in the power relationship between the judiciary and scientists engendered in a move from external to intrinsic assessment of scientific evidence. Under *Frye*, judges possessed power over scientists being able to decide whether knowledge satisfied the 'general acceptance' test. In response, scientists could mobilise 'outside of the court' to try and have certain knowledge claims accepted or rejected. The shift to intrinsic assessment superficially weakens the efficacy of such lobbying, but the flexibility and contradictions involved in falsification or attempting to impose any single scientific method criteria allows for the significant re-entry of similar lobbying efforts external to the courts as scientists renegotiate their knowledge claims under the rhetorical banner of falsification. In this sense, the vagaries of *Frye* have not been transcended by *Daubert*. Just as there is room for considerable variation in how judges interpret 'general acceptance' in response to lobbying from scientists, so too there will be variations in how judges interpret falsification in any practical context; even if falsification develops a 'stabilised' judicial meaning.

The influence which *Daubert* may exert upon the admission of expert opinion evidence in Australian courts remains unclear and is complicated by the enactment of the *Evidence Act* 1995 (Cth). Our discussion has highlighted that it is extremely unlikely that the application of *Daubert* in Australia, as recommended by Odgers and Richardson, would lead to any significant improvement to the quality of the science admitted to Australian courts. In fact, it could well be the case that the application of *Daubert* would create new difficulties unanticipated by Odgers and Richardson.

Whilst the effects of *Daubert* are likely to be complex and in the longer term are unlikely to solve the perceived problems of *Frye*, an interesting philosophical irony of *Daubert* is worth commenting upon. *Daubert* has claimed to transcend *Frye's* 'general acceptance' test by selectively drawing from the fields of HPS/SSK and enabling the identification of a particular philosophical view of

233 J Siegel, "*Daubert* and its Consequences", presented to the Australian and New Zealand Forensic Science Society, 25 May 1996.

science which emphasises falsification. In these fields, falsification has been largely discredited and there is no simple consensus on there being one dominant view of scientific method, or even whether it is relevant to seek a simple model of it to start with. Some questions remain unanswered. Did the US Supreme Court's judgment rely on a 'general acceptance' test poorly applied to the history, philosophy and sociology of science? If not, on what grounds can the Court and its supporters seek legitimacy for *Daubert*?