

## Remembering the Spread of Western Science

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In the fifty years since publication of George Basalla's 'The Spread of Western Science', historians of science have wavered between securely locating knowledge production in specific settings and trying to explain how scientific concepts and practices travel and come to appear universally applicable. As science has come to seem ever more 'situated' and fragmented, we struggle to explain its obvious mobility and reproducibility. No single analytic framework seems plausibly to explain the globalization of science.

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### Introduction: How Science Travels

When George Basalla published his article, 'The Spread of Western Science', in *Science* in May 1967 it made scarcely a ripple, let alone a splash.<sup>1</sup> It appeared at the start of the northern summer, and American readers likely were preoccupied with the escalation of the war in Vietnam or fights over desegregation in Mississippi or the opening of Expo 67 in Montreal or the call up of reservists in Israel. A recent PhD graduate from the Department of the History of Science at Harvard, Basalla in his article used a formal stadial model to describe the introduction of 'modern science' into non-European countries—thereby conveniently recapitulating Walt W. Rostow's explanation of stages leading to a 'take off' into economic growth or modernization.<sup>2</sup> In the 1960s, Basalla was not alone in his concern to explain how science spread or diffused or took up residence far from its presumed site of origin in Europe. Harvard historians I. Bernard Cohen and Donald H. Fleming were ruminating on the development of science in settler societies of the new world; while Derek de Solla Price at Yale and John M. Ziman at Bristol, among others, were trying to make sense of the growth of the worldwide scientific community.<sup>3</sup> But most historians of science regarded such inquiries as secondary matters, as distractions from their main task of elucidating processes of scientific discovery and justification in Europe. Rarely could they be jolted into looking elsewhere. Not until the 1980s did citation of Basalla's article accelerate. By then it mostly served as a handy straw man for the rising generation of postcolonial critics and radical historians of science who sneered at the simple diffusionist pieties of the Cold War. After a few decades, once an empty suit was no longer needed to set the scene, the article could be discarded in English-language studies—though it still seems to be accorded respect in translation, and in outmoded realms of policy discourse that favour evolutionary schemes of development.

I was first exposed to Basalla's model around 1987, some twenty years after its publication, in classes in the Department of History and Philosophy of Science at the University of Melbourne. Vaguely, I recall my teacher Roderick W. Home chiding Basalla for the simplicity and sparseness of his scheme. Home suggested that it obviously represented the rather distant view from the North Atlantic littoral. His colleague Jan Sapp, ever iconoclastic, was more vehement in his criticism, muttering about Cold-War mentalities and the flogging of dead horses. My unpublished essay on the

spread of science to Australia, which I recently was spurred to read again after thirty years, proved surprisingly generous to Basalla—perhaps naively so. The greater virtue and sophistication of Roy M. MacLeod's alternative explanation for the growth of science in colonial societies had been inculcated in me, but I hesitated to substitute it for the appealing theoretic economy of the three-stage model.<sup>4</sup> I believe that I did not, obtusely, even regard the interpretations as mutually contradictory: the academic figure of the straw man might yet have been alien to my unworldly intelligence. The debate, although at the time enigmatic and amorphous in my perception, fascinated me, and it prompted a career-long interest in how science travels.<sup>5</sup>

It is unusual for a scholarly article to become a shibboleth, and even more rare for it to be used to slay imaginary Ephraimites some fifty years since first uttered. But, in fact, reaction to 'The Spread of Western Science' has turned out to be remarkably stimulating and generative, helping to shape—somewhat paradoxically, if not perversely—critical inquiry into the 'globalisation' of science. At the very least, 'The Spread of Western Science' fits that important category, the good 'bad essay', the sort of flawed essay that makes us think differently or more deeply. In its fifty-first year, the article therefore seems to warrant further scrutiny, and not a little retrospective charity.<sup>6</sup>

### The Unity of Science in the Cold War

'How did modern science diffuse from Western Europe and find its place in the rest of the world?', asked Basalla in 1967.<sup>7</sup> The attractiveness of his explanatory scheme derived from its simplicity, universality and teleological drive. The young historian of science and technology detected three overlapping phases or stages in the development of science around the world. Societies in the first phase were essentially unscientific in the modern European sense, though they might foster 'ancient, indigenous scientific thought', and constitute resources or data mines for European savants.<sup>8</sup> The scientific revolution already had transformed Europe, elevating it as a centre of calculation, a superior place where the rest of the world might be made legible and intelligible. There, 'the physical universe was to be understood and subdued ... through a direct, active confrontation of natural phenomena'.<sup>9</sup> There, accumulation, classification and appraisal might readily occur. In the second phase, this special scientific sensibility dispersed and took root in suitable European colonial

settings. Although estimable in places, such peripheral science often remained 'dependant' on European intellectual and institutional sustenance. Sometimes nationalism and further development might promote the growth of an independent scientific tradition—as it did in the United States, Canada, Australia and Japan—propelling these countries into the third, and most mature, phase. Basalla noted that 'creating and supporting scientific institutions and fostering attitudes conducive to the rapid growth of science' were arduous tasks, and not all places, sadly, could reach the highest stage of modern scientific development.<sup>10</sup>

Following the fashion of the 1950s and early 1960s, Basalla emphasised the creation of scientific communities, not the fabrication of scientific knowledge. As an assiduous reader of the sociological works of Robert K. Merton and Joseph Ben-David, he assumed that he was modelling the spreading of scientists over the earth's surface rather than situating scientific practices and knowledge making.<sup>11</sup> Admittedly, he wrote 'we cannot ignore the peculiar environment in which members of a national group of scientists are trained and carry out their research'.<sup>12</sup> Nor would he give it too much weight. 'If that setting does not decisively mold the conceptual growth of science, it can at least affect the number and types of individuals who are free to participate in the internal development of science.' Then again, he wondered if 'the effect is more profound'.<sup>13</sup> Instead of anticipating the social construction of scientific knowledge, he chose to remain at best equivocal, or even mute, on matters epistemological.

Basalla was careful to suggest his scheme was simply a 'heuristic device useful in facilitating a discussion of a neglected topic in the history of science'. Moreover, nothing in the phases of his model was 'cosmically or metaphysically necessary'. He hoped that others might make a 'systematic investigation of the diffusion of Western science throughout the world'. Such a large-scale inquiry would include a 'comparative appraisal of the development of science in different national, cultural, and social settings and would mark the beginning of truly comparative studies in the history and sociology of science'.<sup>14</sup> Basalla therefore sought to provide the analytic framework and stimulus for a global research project. His scientific manifesto paralleled Rostow's ambitious and influential anti-communist economic manifesto. A few years previously, the economic historian had described five stages of development and modernization: traditional society; the preconditions for take off; take off; the drive to maturity; and high mass consumption.<sup>15</sup> It is perhaps paradoxical that Basalla would follow so closely this deterministic Weberian trajectory while attempting to shield scientific *knowledge* from any such 'external' pressures.

To be sure, cold-war modernization theory was not the sole influence on Basalla's thinking. The frequent recourse to the process of 'diffusion' also brings to mind the older, pre-war diffusionist anthropology of Grafton Elliot Smith, W. J. Perry, W. H. R. Rivers and others. They had discerned civilisation spreading from ancient Egypt and Greece, not from Western Europe, but they displayed the same critical engagement with the travel of ideas and artefacts, contact with other cultures, and patterns of reception and transformation.<sup>16</sup> As Elliot Smith insisted:

The diffusion of culture is not a mere mechanical process such as the simple exchange of mechanical objects. It is a vital process involving the unpredictable behaviour of human beings who are the

transmitters and those who are receivers of the borrowed and inevitably modified elements of culture.<sup>17</sup>

But enthusiasm for such cultural diffusion had been short lived, dwindling by the 1940s to a few scattered theories of culture contact. It is doubly ironic that the narrowly bounded structural-functional studies that displaced these pre-war 'global' projects in social anthropology should eventually shape the situated studies of scientific knowledge production that crowded out Basalla's own belated, and equally unfashionable, diffusionist project.

Basalla's time at Harvard helped prompt his interest in the spread of science. In thrall of Benjamin Franklin, Cohen was speculating in the Department of the History of Science on how the United States had turned into a major scientific power. Why, he asked, was it 'so late in the history of America that there developed a full-scale scientific tradition worthy of standing up to the best of the Old World?' Why, in other words, was Franklin alone? He felt the situation in the nineteenth century dire enough to be relegated to the impoverished province of the 'social historian', not the historian of ideas.<sup>18</sup> Indeed, not until the twentieth century did the United States 'become a source of scientific ideas for Europe and not merely a source of data for European ideas'. In 1959, Cohen called for further historical research that 'encompasses an analysis of the stages by means of which scientific ideas are diffused, applied, and accepted or rejected'.<sup>19</sup> (Note his use of the terms 'stages' and 'diffused'.) Meanwhile, it appears some of Cohen's ideas had diffused effectively across Harvard Yard. In the Department of History, Fleming also was endeavouring to understand how Americans, Canadians and Australians had come 'to figure among the scientifically productive peoples of the twentieth century'.<sup>20</sup> He believed that the 'colonial posture' of peripheral scientists and their embedding in a 'universal network of communications' had enabled the prolonged 'absentee landlordism' of the European scientific community. He claimed that 'Puritan intellectuality' and wealth gave the advantage to the United States, while other white settler colonies still were struggling to conjure up the spirit of research.<sup>21</sup> Basalla would acknowledge that Cohen's essay 'provided the inspiration' for his model; and he thanked Fleming for 'sharpening' his analysis, even as he insisted they disagreed 'on fundamental points'.<sup>22</sup> What these may have been remains ambiguous, though one suspects some ordinary long-forgotten internecine dispute.

In a sense, then, Basalla's attempt to explain the spread of Western science represented the lingering international concerns of Harvard's history of science programme. As early as 1924, the discipline's founder, George Sarton, perched uneasily at the university, had asserted that the 'unity of knowledge and the unity of mankind are correlative notions'.<sup>23</sup> While most historians were 'dominated by occidental prejudices ... one must needs recognize that human civilization is not exclusively occidental, not by any means'. A dedicated cosmopolitan, Sarton believed that 'the systematic exploration and description of the universe can be accomplished only by an international collaboration involving many thousands of investigators'.<sup>24</sup> Of course, by 'international' Sarton at the time meant principally European nations and British settler colonial societies, such as the USA and Australia, not mere outposts of empire—not then, not yet, at any rate. All the same, Sarton's beliefs could be extrapolated into a monogenetic argument for the unity of science and the unity

of humanity, rather than a polygenetic romanticism extolling multiple sciences and multiple races. As he put it:

The true internationalism toward which the unity of knowledge and the unity of mankind are steadily driving us, will constitute an immense progress. This progress will be largely due to the development of positive knowledge and scientific methods.<sup>25</sup>

As science diffused around the world, we therefore would see a new era in human progress, or scientific modernity. Cohen and Fleming, apparently imbued with the same internationalism and ‘new humanism’, passed these tendencies on to Basalla.

Looking back, one might also discern the figure of Alexis de Tocqueville briefly haunting discussions at Harvard in this period. Of course, Tocqueville was concerned more with the spread of the ‘principle of democracy’ to the new world, and its subsequent diffusion across the continent, than with the dispersal of science.<sup>26</sup> Thus, democracy ‘has there been able to spread in perfect freedom, and peaceably to determine the character of the laws by influencing the manners of the country’. More specifically, ‘the civilization of New England has been like a beacon lit upon a hill, [which] after it has diffused its warmth immediately around it, also tinges the distant horizon with its glow’.<sup>27</sup> Of scientific diffusion he was less sanguine. The typical American, he reflected, ‘values science not as an enjoyment, but as means, and is only anxious to seize its useful applications’.<sup>28</sup> Tocqueville occasionally pondered how the true European scientific spirit might come to be ‘implanted’ as deeply as the principle of democracy in the new world, but he remained, unlike later Harvard interlocutors, puzzled and pessimistic about such matters.

### Imperial Assemblages and Postcolonial Critique

Most historians of science in the 1960s remained committed to recording the heroic achievements of European scientists and their development of the ‘scientific method’, but the Harvard crew were not alone in beating against the current. While working in Singapore in the 1940s, Derek de Solla Price, then an English physicist, had become fascinated by the exponential growth of science around the world. In the 1960s, as a historian of science at Yale, he concocted an elaborate quantitative analysis to determine ‘the nature of the total world network of scientific papers’ and to document the existence of what he called the ‘research front’, which regrettably he left geographically rather vague.<sup>29</sup> Similarly, the philosopher of science Stephen Toulmin, who taught briefly at the University of Melbourne in the 1950s, hoped ‘sociometrics’ would somehow unite the ‘externalist’ social study of science with ‘internalist’ accounts of ‘the ontogeny or morphogenesis of science in isolation from its ecological environment’. Thus, Toulmin attempted to discover the ‘selection criteria’ for scientific concepts and to establish the evolutionary dynamics of scientific development around the world.<sup>30</sup> A theoretical physicist brought up in New Zealand, John Ziman also sought to explain ‘the growth and spread of basic science, from its original nuclei in the industrial countries of western Europe, into all corners of the Earth’. ‘We hold it almost as self-evident’, he wrote in 1968, ‘that currents of knowledge, skill, attitudes and techniques should diffuse the culture of scientific research throughout the world.’<sup>31</sup> But how does this happen? Ziman attributed the dispersal of science to the implanting of research and training institutions that transmitted tacit knowledge of scientific investigation

and cultivated a particular inquiring sensibility. ‘The spread of science throughout the world has not been the haphazard distribution of windblown seeds’, he wrote, ‘but by runners and tendrils reaching out from original institutions and establishing themselves in new soil.’ Once established, new national traditions of scientific research might display ‘charming variations of style’, but the intellectual content must be the same wherever science, and the free trade in knowledge, flourished.<sup>32</sup> Evidently, the imperialism of free trade still found refuge in theories of the globalisation of science.<sup>33</sup>

No wonder that historians of imperial science—especially those located in settler colonial societies such as Australia—came early in the 1980s to question such pieties of the diffusion of western science. Thus, Roy MacLeod, trained at Harvard and Cambridge, and based at the University of Sydney, suggested that the crucial problem was not how science might have propelled itself around the globe, but rather, ‘how did the pursuit of natural knowledge become a part of statecraft?’ He criticised Basalla for proposing ‘an evolutionary, almost deterministic pattern of cultural expansion’. The three-stage model was far too linear and homogeneous, ignoring colonial political and economic influences, and obscuring lingering enforced dependencies and inequities.<sup>34</sup> As MacLeod later elaborated, Basalla’s model assumes that:

Science is everywhere ‘value-neutral’, and so understates the significance of intellectual hegemonies; it neglects the cultural significance of traditional knowledge, and avoids mention of critical, if at times ‘unscientific’, traditions of dissent. In positing a linear sequence of events, it is apparently blind to the cultural, historical and economic context of the process of diffusion itself.

There was, MacLeod repined, no ‘geopolitical dimension’ to Basalla’s argument.<sup>35</sup> And yet, this omission had been so flagrant as to draw attention, inadvertently, to relevant processes of colonialism and decolonisation. The author’s dereliction made cynosures of local content and context. Thus, MacLeod emphasised how British metropolitan and imperial sciences ‘reflect and mediate the changing perceptions of vested interests, both in England and the colonies’. In response to Basalla, it had become necessary to situate colonial science precisely in its local political, economic and cultural settings, to render it multi-centred. Moreover, MacLeod now offered ‘an impressionistic taxonomy’ of British imperial science, more complex and nuanced than Basalla’s, describing metropolitan, colonial, federative, and commonwealth or dominion modalities. This historically specific scheme demonstrated how ‘science became a convenient metaphor of empire itself, or more exactly, what the empire might become’.<sup>36</sup>

MacLeod first presented his critique of facile speculations on ‘diffusion’ of western science at a conference on ‘scientific colonialism’, held in Melbourne in 1981. One of the symposium’s organisers, Rod Home, recalls that he thought ‘it might be a useful way of getting beyond the very limited literature on the subject of science in a colonial context’.<sup>37</sup> As the editors of the proceedings pointed out, most of the participants had agreed that Basalla’s model was inadequate. ‘It simply did not capture the richness and complexity of the diffusion of Western scientific culture’, wrote Nathan Reinhold and Marc Rothenberg. Instead, what emerged—as ‘a heuristic solution’—from the Melbourne meeting was ‘to regard science as polycentric’.<sup>38</sup> At the conference, David Wade Chambers, the Harvard-trained professor of science studies at Deakin University, endorsed these criticisms in his study of the development of Mexican

science before 1867. In Mexico, ‘the three phases of the Basalla model are seen to be so much intermingled as to be of little value in analysing scientific growth’. Chambers regarded the scheme as a ‘strictly linear analysis of extremely varied and complex cultural scientific variables’, its supposed universalism actually spurious and pretentious.<sup>39</sup> Later, with Richard Gillespie, he expatiated on histories of colonial and national science. Chambers and Gillespie argued that ‘modern science is better understood, both metaphorically and actually, as a polycentric communications network’. They condemned Basalla’s ‘unrelenting Eurocentrism’, proposing instead the historical comparison of local sciences.<sup>40</sup> The framework, they wrote:

Should be symmetrical and interactive across the great divides—center/periphery, local/global, national/colonial, and traditional/modern. It should be nonlinear, nonstaged, and nonprescriptive, but it should specify a set of parameters that allow systematic comparison of the great array of independent and interdependent local histories of the production, application, and diffusion of natural knowledge. It should be dynamic and flexible and should identify vectors of communication, exchange and control.<sup>41</sup>

It was, as they put it, ‘a tall order’. Moreover, Chambers and Gillespie understood that in emphasizing the disunity of science they risked ‘sinking into a vast sea of nativist ethnohistories’.<sup>42</sup> To stay afloat, then, they leapt on colleague David Turnbull’s notion of technoscientific assemblages, by which he had meant the ‘amalgam of places, bodies, voices, skills, practices, technical devices, theories, social strategies and collective work that together constitute technoscientific knowledge/practices’.<sup>43</sup> Turnbull argued that ‘all knowledge traditions, including Western technoscience, can be compared as forms of local knowledge so that their differential power effects can be compared but without privileging any of them epistemologically’.<sup>44</sup> Chambers and Gillespie expanded the concept of such vital collectives of knowledge practices, referring to ‘conglomerate vectors of assemblage’ to denote additional dynamism.<sup>45</sup>

But not everyone at the Melbourne meeting was dissatisfied with the coloniser’s model of the world.<sup>46</sup> Lewis Pyenson, for example, went on to complete his pioneering study of the ‘exact’ sciences—by which he meant physics and astronomy—in the Dutch East Indies (later Indonesia), extolling the apparently frictionless spread of western science, and concluding that local conditions had exerted no influence on alien ways of knowing. European civilization thus flowed uncontaminated over the messy, disorderly archipelago.<sup>47</sup> All Pyenson cared for, wrote Paolo Palladino and Michael Worboys, was ‘the work of scientific missionaries exporting metropolitan civilization to the colonial periphery.’ As harbingers of a richly contextual history of colonial science, Palladino and Worboys insisted that ‘western methods and knowledge were not accepted passively, but were adapted and selectively absorbed in relation to existing traditions of natural knowledge and religion and other factors.’<sup>48</sup> Pyenson protested ‘only sharp-toothed unkindness would associate my conclusions with the denial of any people’s authentic history.’<sup>49</sup> But clearly such authentic history, in Pyenson’s view, could not re-channel, let alone taint, the imperial diffusion or laminar flow of exact sciences.

Unsparringly, the tide of historiography through the 1990s was turning against Basalla and Pyenson and other diffusionists. Perhaps the end of the Cold War and the institutionalising of decolonisation

had sucked dry the source of any imperial *amour propre*. Increasingly, we were exposed to densely realised studies of the contact zones of mobile knowledge practices, often invoking an anthropological sensibility and placed explicitly in postcolonial frames.<sup>50</sup> Sandra Harding, for example, sought to use cross-cultural studies of knowledge traditions to achieve further epistemological pluralism. For Harding, postcolonial accounts provided ‘resources for more accurate and comprehensive scientific and technological thought’. ‘We can employ the category of the postcolonial strategically’, she wrote, ‘as a kind of instrument or method of detecting phenomena that otherwise are occluded’.<sup>51</sup> Influenced by feminist standpoint theory, Harding emphasised the importance of local knowledge and called for more dynamic and inclusive global histories. But her main goal was the strengthening of modern scientific objectivity, achieving better modernity through remedying ‘dysfunctional universality claims’.<sup>52</sup> Similarly, Helen Verran, with the Yolngu people of Arnhem Land, Australia, studied the interaction of local knowledge practices, one ‘traditional’, the other ‘scientific’, and described ‘the politics waged over ontic/epistemic commitments’. Her intention was not just to exploit the splits and contradictions of western rationality: she wanted a community that ‘accepts that it shares imaginaries and articulates those imaginaries as part of recognizing the myriad hybrid assemblages with which we constitute our worlds’.<sup>53</sup>

At the end of the twentieth century, the critique of sovereign claims and hegemonic presumptions in knowledge making—challenges to master narratives—disseminated widely, often under the rubric of postcolonial studies. In new histories of science, some epistemological space could be found for independent concept building beyond the North Atlantic littoral. The flow of knowledge and practice from Europe, and into it, began to seem more turbulent, no longer laminar; and matters of local terrain and inescapable friction came to roughen the surface. Problems of translation, mediation, transformation—as well as indifference and resistance—seemed ever more pressing, displacing older hydraulic models and metaphors. Around 2000, Gabrielle Hecht and I organised some workshops on what we called ‘postcolonial technoscience’, resulting in a special issue of *Social Studies of Science*. ‘We hope that a closer engagement of science studies with postcolonial studies’, I wrote in the introduction, ‘will allow us to question technoscience differently, find more heterogeneous sources, and reveal more fully the patterns of local transactions that give rise to global, or universalist, claims.’ The monitory example of Basalla even then was in my mind. In contrast, we were inquiring into ‘the localness of technoscientific networks, the situated production of ‘globality’, the transnational processes of displacement and reconfiguration, the fragmentation and hybridity of technoscience’.<sup>54</sup> A few years later, Vincanne Adams and I were asserting that postcolonial analysis offers a ‘flexible and contingent framework for understanding contact zones of all sorts, for tracking unequal and messy translations and transactions that take place between different cultures and social positions, including between different laboratories and disciplines even within in Western Europe and North America’.<sup>55</sup> We felt strongly that ‘global’ science should be critically refigured as polycentric or multi-sited knowledge making enterprises. ‘The task of understanding planet-wide interconnections’, as Anna Loewenhaupt Tsing put it, ‘requires locating and specifying globalist projects and dreams, with their contradictory as well as charismatic logics and their messy as well as effective encounters and translations’.<sup>56</sup>

It is possible, then, to trace an ontological turn in contemporary social studies of science, a shift toward recognising other thought worlds, a multiplication that gradually served to re-orient histories of science away from what Jacques Derrida called the ‘white mythology’.<sup>57</sup> In 2015, I found myself advocating ‘Asia as method’ in science studies as a means to distinguish East Asia as a concatenation of domestic sites for scientific inquiry, and not just spaces for data extraction or places to which European concepts diffused.<sup>58</sup> I was postulating an Asia that is good to think with, and think from, in histories of science, rather than a fixed, hegemonic geographical region or essential civilizational entity. Additionally, I was hoping to connect histories of science with emerging critical area studies, which were posing, in Pheng Cheah’s words, the ‘methodological problem of how to think about matters comparatively, without dogmatically privileging the North Atlantic as the main point of theoretical reference, or taking it for granted as a world-historical *telos*’.<sup>59</sup> The assembling of such a deliberately untidy cognitive platform on which one might compose different and heterodox histories of science seemed to me inherently a postcolonial project, a method of decolonising our narratives, our accounts of scientific modernity.<sup>60</sup> Even as Bruno Latour showed us that ‘we’ (whether in Europe or elsewhere) have never been truly ‘modern’, postcolonial critics thus were desecrating multitudes of plural modernities: alternative modernities, new modernities, Indigenous modernities—and so it went on and on.<sup>61</sup> Perhaps we have never been truly scientific—but at the same time we have never, it appears, had more ways of being, and places to be, quasi-scientific.

### Circulating through the Networks

‘If facts depend so much on... local features, how do they work elsewhere?’ Simon Schaffer asked insistently in 1992, as science was becoming so exhaustively ‘situated’.<sup>62</sup> ‘The more local and specific knowledge becomes’, wrote James A. Secord, ‘the harder it is to see how it travels’.<sup>63</sup> Or as Steven Shapin observed, ‘we need to understand not only how knowledge is made in specific places but also how transactions occur between places’.<sup>64</sup> Having placed the view from nowhere, how do we get from there to somewhere else? Even as we multiply the disunities of science, should we not be looking for how knowledge is harmonised or connected or translated? According to philosopher of science David J. Stump, contextualisation does not imply delegitimation, but it does mean ‘we need to provide an account of how knowledge and skills are disseminated out of a specific context despite their being local’.<sup>65</sup>

From the 1980s, Latour, Michel Callon, John Law, and many others were fashioning actor-network theory (ANT) to account for the travels and new dwellings of science and technology—for the worldliness of formal or technical knowledge and practice, for the laboriously constructed ‘unity’ of science. ‘The model of extended translation’, Callon wrote, ‘does not oppose local and global, nor does it negate agency and passive behaviors. Rather, it describes the dynamics of networks of different lengths, degrees of irreversibility, diversity, and interconnectedness’.<sup>66</sup> In the beginning, ANT explained how a series of translations across a network could keep science and technology invariant in different settings. For example, how do we make the laws of physics apply equally well in Paris and Gabon? The extension and transformation of networks might stabilise facts like this, producing ‘immutable mobiles’. The more

articulations that develop with human and non-human actors, the more stable and robust the object becomes. Accordingly, society, nature, and geography are the outcomes, rather than the causes, of these mobilisations, translations and enrolments. ‘Facts’, according to Latour, are ‘circulating entities. They are like a fluid flowing through a complex network’.<sup>67</sup>

Despite their ‘complexity’, such networks often have followed uncritically older colonial contours. Unlike those French intellectuals who seek to repress or sublimate their own imperial emplotments, Latour started auspiciously, advising us to look to the colonies, not the ‘home country’, in order to ‘follow [the] transformation of a society by a ‘science’’. In *The Pasteurization of France*, he argued that in the colonial tropics ‘we can imagine best what a pasteurised medicine and society are’.<sup>68</sup> But then he chose to cast colonial relations in simple forms of dominance and submission, thereby exhibiting the expansion of sovereign networks of French science. Later, in *Pandora’s Hope*, Latour took a field trip to the Amazon to bring order to ‘the jungle of scientific practice’.<sup>69</sup> In deepest Amazonia, scientists developed a laboratory in the depopulated jungle, remaining in conversation with their European colleagues in order to stabilise scientific facts on the margins of civilisation. More colonial *amour propre* than postcolonial analysis, Latour’s story managed to omit local agents and context, thus turning the network into a sort of iron cage through which no native can break. Thus, the ‘local’ seemed quite abstract, strangely depopulated, and depleted of historical and social content. John Law has observed that some ANT ‘tends to ignore the hierarchies of distribution, it is excessively strategic and it colonises ...the Other’.<sup>70</sup> Shapin criticised the ‘militaristic and imperialistic language that so characterize Latour’s work’.<sup>71</sup> Recently, however, Latour has stressed instead the need for science to become *embedded* in a connected series of laboratories, which provide the ‘life supports’ or ‘plausible ecosystems’ for objectivity. But, as he laments, he can no longer find ‘space for making sense of the billions of migrations that define the ‘global’ but in effect not-so-global world’.<sup>72</sup>

Kapil Raj, another Paris-based scholar, has expressed his disdain for ‘the dominant vision of colonial science as a hegemonic European enterprise whose universalisation can be conceived of in purely diffusionist terms’.<sup>73</sup> As an alternative, he proposes (or rather, supposes) a ‘circulatory’ model for the spread of western science. He seeks to illuminate the ‘co-production of the local and the global’ by ‘following the conduits and heterogeneous networks of exchange through which transfers of knowledge passed, by locating the spaces of circulation between South Asia and Europe in which they acquired meaning, and finally by focussing on the appropriation and grounding of these knowledges in specific localities within these spaces of circulation ....’<sup>74</sup> There certainly is a lot going on here: ‘conduits’, ‘heterogeneous networks’, ‘exchange’, ‘transfers’, ‘spaces of circulation’, ‘appropriation’, and ‘grounding’. Promisingly, Raj insists he will turn attention ‘to transformations in knowledge practices, and in the men who embodied them, as they circulated, negotiated, and reconfigured their skills in the contact zone’.<sup>75</sup> But as that closing term—‘contact zone’—suggests, ‘circulation’ becomes, in effect, a dead letter, a dispatch left undelivered. Raj adduces several interesting case studies in the exchange and negotiation of ‘scientific’ ideas and practices in South Asia, but circulation can only be inferred from the ‘intercultural encounter’, from the production of *situated* knowledges. Thus, he gives us fascinating examples ‘of the construction

of scientific knowledge in the contact zone itself'.<sup>76</sup> (Surprisingly, in a book abounding with such postcolonial insight, the author is prone to bungled diatribes against 'postcolonial' histories.) For Raj, 'circulation occurs within bounded spaces', within the 'intercultural encounter'.<sup>77</sup> Fa-ti Fan has criticised circulation models for tending 'to suggest that people, information, and material objects flowed smoothly along networks and channels .... The image of circulation', he wrote, 'tends to impose too much unity, uniformity, and directionality on what was complex, multidirectional and messy.'<sup>78</sup> As Anna Tsing put it, circulation models are too often 'closed to attention to struggles over the terrain of circulation and the privileging of certain kinds of people as players.'<sup>79</sup> However, in rendering 'circulation' inoperative or at least profoundly enigmatic, Raj deftly sidesteps such reproval.

### Conclusion: the Harmonisation of Heterogeneity

Fifty years ago, Basalla set out to explain the scientific unification of the globe, but changing epistemological premises, especially inclinations toward heterogeneous local framings of science, caused his model to become incongruous and defunct. In the new intellectual environment, his evolutionary theory of the spread of western science had to be decommissioned. It was no longer obvious what would even count as 'science'—or 'western' for that matter. But as we do better at reconstituting and situating scientific ideas and practices, at producing multiple sciences, we struggle to decipher their migrations, adaptations and translations. We have a rich vocabulary to describe the local construction of the knowledge we credit as scientific—but we often are lost for words in accounting for how such assemblages, or parts thereof, manage to travel.

As we decolonise knowledge and disperse scientific agency, we also discard, necessarily, old imperial models of diffusion and dissemination. How might we find substitutes, without recuperating customary sovereignties? Gestures toward networks, circulation, flow or diffusion are unconvincing, particularly when imperial baggage restricts their amplitude. We tend to get little more than thin descriptions of conventional hydraulics—more metaphor than method.<sup>80</sup> But other approaches are available. In narrating the history of research into a disease in the highlands of New Guinea, I rather fancifully tried to refigure the transaction of related scientific objects as a sort of modern *kula* ring, an extended exchange system, in order to show how things, persons, and practices co-constituted one another and were mobilised around the world, using a locally generated theory of how this might happen.<sup>81</sup> However, if there is any virtue in this, it is unlikely to prove broadly applicable. Alternatively, one might contemplate extending borderlands history to link a multiplicity of scientific encounters, viewing concept work more generally as edge effect. 'As a study of entanglements', Pekka Hämäläinen and Samuel Truett write, 'borderlands history is well situated to work on a variety of scales to circumvent the traditional blind spots of imperial and national histories.'<sup>82</sup> Further possibilities come to mind. We might describe, for example, the passages of science using methods derived from historical and anthropological studies of diaspora and migration, taking up a kind of 'thick transregionalism'. As Engseng Ho argues, this reorientation would allow historians 'to discover veins of data that speak to connections with other regions, data that were not seen or were ignored earlier simply because we did not understand the mobile ... processes that generated them in the first place'.<sup>83</sup>

I could go on, but my point is we should be open to a multitude of explanations for the family resemblance of science around the world. We have come to recognise the postcolonial plurality of knowledge making—why, then, should we demand a unique model for its transaction? Fifty years ago, Basalla could confidently propose a single, cohesive model for the spread of what appeared to be unitary science, but now, it seems to me, the most we can hope for is the harmonisation of heterogeneity, or the calibration of disunity.

### Conflict of interest

The author declares no conflict of interest.

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### Endnotes

1. George Basalla, 'The Spread of Western Science', *Science*, 156 (1967), 611–622. Roy MacLeod tells me (personal communication, 16 February 2018) that many scholars in Cambridge, MA, and Cambridge, England, did read it soon after publication, and eventually it prompted MacLeod to offer a graduate course in the subject. Basalla had been MacLeod's tutor when he was a Harvard undergraduate.
2. W. W. Rostow, 'The Stages of Economic Growth', *Economic History Review*, 12 (1959), 1–16, and *The Stages of Economic Growth: a Non-Communist Manifesto* (New York: Cambridge University Press, 1960). Rostow was based down Mass Ave at MIT when Basalla was a graduate student. Basalla recalled he conceived the model in 1964, his last year of graduate school: see his 'The Spread of Western Science Revisited', in *Mundialización de la ciencia y cultural nacional*, eds Antonio Lafuente, Alberto Elena, and María Luisa Ortega (Madrid: Doce Calles, 1993), pp. 599–603.
3. I. Bernard Cohen, 'The New World as a Source of Science for Europe', *Actes du IXe Congrès international d'histoire des sciences* (Barcelona: Hermann, 1959), pp. 95–130. Donald H. Fleming, 'Science in Australia, Canada, and the United States: Some Comparative Remarks', *Proceedings of the 10th International Congress of the History of Science, Ithaca, NY, 1962* (Paris: Hermann, 1964), pp. 179–196. Derek de Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963), and 'Networks of Scientific Papers', *Science*, 149 (1965), 510–515. J. M. Ziman, 'Some Problems of the Growth and Spread of Science into Developing Countries', *Proceedings of the Royal Society A*, 311 (1969), 349–369.
4. Roy M. MacLeod, 'On Visiting the "Moving Metropolis": Reflections on the Architecture of Imperial Science', *Historical Records of Australian Science*, 5 (1982), 1–16. See also R. W. Home and Sally Gregory Kohlstadt, eds *International Science and National Scientific Identity: Australia between Britain and America* (Dordrecht: Kluwer, 1991)—the essays in this volume were derived from a conference on the topic held in Melbourne in 1988.
5. Warwick Anderson, 'Postcolonial Technoscience', *Social Studies of Science*, 32 (2002), 643–658, 'From Subjugated Knowledge to Conjugated Subjects: Science and Globalisation, or Postcolonial Studies of Science?' *Postcolonial Studies*, 12 (2009), 389–400, and 'Postcolonial Science Studies,' in *International Encyclopedia of the Social and*

- Behavioral Sciences*, 2nd edn, ed. James D. Wright (Oxford: Elsevier, 2015), pp. 652–657.
6. See also Dhruv Raina, 'From West to Non-West? Basalla's Three-Stage Model Revisited', *Science as Culture*, 8 (1999), 497–516, and William K. Storey, *Scientific Aspects of European Expansion* (Aldershot: Variorum, 1996).
  7. Basalla, already cited (n. 1), p. 611.
  8. Basalla ignores the sciences of China, India and Middle East.
  9. Basalla, already cited (n. 1), p. 612.
  10. Basalla, already cited (n. 1), p. 617.
  11. Robert K. Merton, *The Sociology of Science*, eds B. Barber and W. Hirsh (New York: Free Press, 1962); and Joseph Ben-David, 'Scientific Growth: a Sociological View', *Minerva*, 2 (1964), 455–476. Merton also frequently referred to the 'diffusion' of ideas: see especially his 'The sociology of knowledge', *Isis*, 27 (1937), 493–503.
  12. Basalla, already cited (n. 1), p. 620.
  13. Basalla, already cited (n. 1), p. 620.
  14. Basalla, already cited (n. 1), p. 620.
  15. Rostow, already cited (n. 2). Surprisingly, Basalla refrained from using the term 'take off'. See also communications theorist Everett M. Rogers, *Diffusion of Innovation* (New York: Free Press, 1962), and historian of technology D. R. Headrick, *The Tools of Empire: Technology and European Imperialism in the Nineteenth Century* (New York: Oxford University Press, 1981).
  16. Ross L. Jones and Warwick Anderson, 'Wandering Anatomists and Itinerant Anthropologists: the Antipodean Sciences of Race in Interwar Britain', *British Journal of the History of Science*, 48 (2015), 1–16.
  17. G. Elliot Smith, *The Diffusion of Culture* (London: Watts and Co., 1933), p. 10. See also Gabriel Tarde, *The Laws of Imitation*, trans. Elsie Clews Parson (New York: Holt, 1903 [1890]), and A. L. Kroeber, 'Diffusion', in *The Encyclopedia of the Social Sciences*, eds Edwin R. A. Seligman and Alvin Johnson (New York: Macmillan, 1937), 2: 137–142. It could be said that all of these are just fancy ways of talking about the 'civilising project': see Norbert Elias, *Civilizing Process*, trans. Edmund Jephcott (New York: Urizen Books, 1978 [1939]).
  18. Cohen, already cited (n. 3), pp. 95, 96.
  19. Cohen, already cited (n. 3), pp. 121, 96.
  20. Fleming, already cited (n. 3), p. 179.
  21. Fleming, already cited (n. 3), p. 182. See also Donald Fleming, 'American Science and the World Scientific Community', *Journal of World History*, 8 (1964), 666–678.
  22. Basalla, already cited (n. 1), p. 621. Also in this period, Louis Hartz, a professor of government at Harvard, published *The Founding of New Societies: Studies in the History of the United States, Latin America, South Africa, Canada, and Australia* (New York: Harcourt, Brace, and World, 1964).
  23. George Sarton, 'The New Humanism', *Isis*, 6 (1924), 9–42, p. 24.
  24. As above, p. 25.
  25. As above, p. 26.
  26. In the cold war, the two phenomena—democracy and science—were frequently amalgamated.
  27. Alexis de Tocqueville, *Democracy in America*, trans. Henry Reeve, ed. Francis Bowen, 2 vols. (Cambridge: Sever and Francis, 1862 [1848]), vol. 1, pp. 7, 37.
  28. Tocqueville, as above, pp. 508–509.
  29. Price, already cited (n. 3), pp. 510, 513. See also Derek de Solla Price, *Science Since Babylon* (New Haven: Yale University Press, 1961), Price, 1963, already cited (n. 3), and 'Nations can Publish or Perish', *Science and Technology*, 70 (1967), 70–84.
  30. Stephen Toulmin, 'The Evolutionary Development of Natural Science', *American Scientist*, 55 (1967), 456–471, p. 458. On externalism, see Roy M. MacLeod, 'Changing Perspectives in the Social History of Science', in *Science, Technology and Society: A Cross-Disciplinary Perspective*, eds Ina Spiegel-Rösing and Derek de Solla Price (London: Sage, 1977), 149–195.
  31. Ziman, already cited (n. 3), p. 349. The paper was delivered the previous year as the Rutherford Memorial Lecture. See also John M. Ziman, *Public Knowledge: an Essay Concerning the Social Dimension of Science* (Cambridge: Cambridge University Press, 1968), and 'The International Scientific Community: Ideas Move around inside People', *Minerva*, 15 (1977), 83–93. The founding of *Minerva* in 1962, under the editorship of Edward Shils, funded by the Congress for Cultural Freedom, and dedicated in part to the study of science and development in places like India and Africa, also is significant. See Roy M. MacLeod, 'Consensus, Civility and Community: the Origins of *Minerva* and the Vision of Edward Shils', *Minerva*, 53 (2016): 255–292.
  32. Ziman, already cited (n. 3), pp. 360, 362.
  33. A reference to John Gallagher and Ronald Robinson, 'The Imperialism of Free Trade', *Economic History Review*, 6 (1953), 1–15.
  34. MacLeod, already cited (n. 5), p. 1.
  35. Roy M. MacLeod, 'The Contradictions of Progress: Reflections on the History of Science and the Discourse of Development', *Prometheus*, 10 (1992), 260–284, p. 271. See also the introduction to his *Archibald Liversidge, FRS: Imperial Science Under the Southern Cross* (Sydney: Sydney University Press, 2000).
  36. MacLeod, already cited (n. 5), pp. 14, 7, 14. See also Ian Inkster, 'Scientific Enterprise and the Colonial "Model": Observations on the Australian Experience in Historical Context', *Social Studies of Science*, 15 (1985), 677–704.
  37. Email to author, 3 December 2017.
  38. Nathan Reingold and Marc Rothenberg, 'Introduction', in *Scientific Colonialism: a Cross-Cultural Comparison*, eds Reingold and Rothenberg (Washington DC: Smithsonian Institution Press, 1987), pp. vii–xiii, pp. xi, xii.
  39. David Wade Chambers, 'Period and Process in Colonial and National Science', in Reingold and Rothenberg, eds, *Scientific Colonialism*, pp. 297–321, p. 312. Ironically, Chambers' 1969 Harvard PhD dissertation was entitled: 'Two Stages of the Development of Science in Mexico'.
  40. David Wade Chambers and Richard Gillespie, 'Locality in the History of Science: Colonial Science, Technoscience, and Indigenous Knowledge', *Osiris*, 15 (2000), 221–240, pp. 223, 224. See also Chambers, 'Locality and Science: Myths of Centre and Periphery', in Lafuente, Elena, and Ortega, eds, *Mundialización de la ciencia*, pp. 605–618.
  41. Chambers and Gillespie, already cited (n. 41), pp. 226–227.
  42. Chambers and Gillespie, already cited (n. 41), pp. 227, 229.
  43. David Turnbull, 'Local Knowledge and Comparative Scientific Traditions', *Knowledge and Policy*, 6 (1993–4), 29–54, p. 34. See also his 'Cartography and Science in Early Modern Europe: Mapping the Construction of Knowledge Spaces' *Imago Mundi*, 48 (1996), 7–14, and 'Reframing Science and Other Local Knowledge Traditions', *Futures*, 29 (1997), 551–562, and Helen Watson-Verran and Turnbull, 'Science and Other Indigenous Knowledge Systems', in *The Handbook of Science and Technology Studies*, eds Sheila Jasanoff, Gerald E Markle, James C. Petersen and Trevor Pinch (Thousand Oaks CA: Sage, 1995), pp. 115–139. Both Turnbull and Verran also worked in the science studies programme at Deakin University. Turnbull derived his use of assemblage from Gilles Deleuze and Félix Guattari, *A Thousand Plateaus: Capitalism and Schizophrenia*, trans. Brian Massumi (Minneapolis: University of Minnesota Press, 1987 [1980]). For later usage, see Stephen J. Collier and Aihwa Ong, 'Global Assemblages, Anthropological Problems', in *Global Assemblages: Technology, Politics, and Ethics as Anthropological Problems*, eds Ong and Collier (Oxford: Blackwell, 2005), pp. 3–21.
  44. David Turnbull, *Masons, Tricksters and Cartographers: Comparative Studies in the Sociology of Scientific and Indigenous Knowledge* (Amsterdam: Harwood Academic, 2000), p. 6.

45. Chambers and Gillespie, already cited (n. 41), p. 231.
46. J. M. Blaut, *The Colonizer's Model of the World: Geographical Diffusionism in Eurocentric History* (New York: Guilford Press, 1993).
47. Lewis Pyenson, *Empire of Reason: Exact Sciences in Indonesia, 1840–1940* (Leiden: Brill, 1989). See also his *Civilizing Mission: Exact Sciences and French Overseas Expansion, 1830–1940* (Baltimore: Johns Hopkins University Press, 1993), which makes a similar argument for French Indochina. Pyenson was happy to admit that 'weak' sciences might adapt to local conditions and be employed in statecraft—but not the 'exact' sciences. For other examples of the recrudescence of Basalla's model in the early 1990s, see Thomas Schott, 'The World Scientific Community: Globality and Globalization', *Minerva*, 29 (1990), 440–462, 'World Science: Globalization of Institutions and Participation', *Science, Technology and Human Values*, 18 (1993), 196–208, and 'Collaboration in the Invention of Technology: Globalization, Regions, and Centers', *Social Science Research*, 23 (1994), 23–56, and Edward Shils, 'Reflections on Tradition, Center and Periphery, and the Universal Validity of Science: the Significance of the Life of S. Ramanujan', *Minerva*, 29 (1991), 393–419.
48. Paolo Palladino and Michael Worboys, 'Science and Imperialism', *Isis*, 84 (1993), 91–102, pp. 98, 99.
49. Lewis Pyenson, 'Cultural Imperialism and the Exact Sciences Revisited', *Isis*, 84 (1993), 103–108, p. 106.
50. The popular term 'contact zone' comes from Mary Louise Pratt, *Imperial Eyes: Travel Writing and Transculturation* (London: Routledge, 1992). As Pratt notes: 'By using the term "contact", I aim to foreground the interactive, improvisational dimensions of colonial encounters so easily ignored or suppressed by diffusionist accounts of conquest and domination' (p. 6).
51. Sandra Harding, *Is Science Multicultural? Postcolonialisms, Feminisms, and Epistemologies* (Bloomington: Indiana University Press, 1998), pp. 8, 16. See also her 'Is science multicultural? Challenges, resources, opportunities, uncertainties', *Configurations*, 2 (1994), 301–330, and *Sciences from Below: Feminisms, Postcolonialities, and Modernities* (Durham NC: Duke University Press, 2008).
52. Harding, 1998, already cited (n. 52), p. 33. See also Donna Haraway, 'Situated Knowledges: the Science Question in Feminism and the Privilege of Partial Perspective', *Feminist Studies*, 14 (1988), 575–599.
53. Helen Verran, 'Re-Imagining Land Ownership in Australia', *Postcolonial Studies*, 1 (1998), 237–254, p. 238. See also her 'The Telling Challenge: Environmental Scientists and Aboriginal Land Owners Seeking to Cooperate in Resource Management through Firing', *Social Studies of Science*, 32 (2002), pp. 729–762.
54. Anderson, already cited (n. 6), pp. 643, 651.
55. Warwick Anderson and Vincanne Adams, 'Pramoedya's Chickens: Postcolonial Studies of Technoscience', in *The Handbook of Science and Technology Studies*, edn 3, eds Edward J. Hackett, Olga Amsterdamska, Michael Lynch, and Judy Wajcman (Cambridge MA: MIT Press, 2007), pp. 181–207, p. 184. For 'trading zones', see Peter Galison, 'Computer Simulations and the Trading Zone', in *The Disunity of Science: Boundaries, Contexts, and Power*, eds Peter Galison and David J. Stump (Stanford: Stanford University Press, 1996), pp. 118–157. See also David N. Livingstone, *Putting Science in its Place: Geographies of Scientific Knowledge* (Chicago: University of Chicago Press, 2004), and Suman Seth, 'Putting Knowledge in its Place: Science, Colonialism, and the Postcolonial', *Postcolonial Studies*, 12 (2009), 373–388.
56. Anna Loewenhaupt Tsing, 'The Global Situation', in *The Anthropology of Globalization: a Reader*, eds Jonathan Xavier Inda and Renato Rosaldo (Oxford: Blackwell, 2002), pp. 453–486, p. 456. See also her *Friction: an Ethnography of Global Connection* (Princeton: Princeton University Press, 2005).
57. Jacques Derrida, 'White Mythology: Metaphor in the Text of Philosophy', trans. F. C. T. Moore, *New Literary History*, 6 (1974), 5–74.
58. Warwick Anderson, 'Asia as Method in Science and Technology Studies', *East Asian Science, Technology and Society Journal*, 6 (2012), 445–451, and 'Postcolonial Specters of STS', *East Asian Science, Technology and Society Journal*, 11 (2017), 229–233. See also Fa-ti Fan, 'Modernity, Region, and Technoscience: One Small Cheer for Asia as Method', *Cultural Sociology*, 10 (2016), 352–368. For South Asia, see Ashis Nandy, *Alternative Sciences: Creativity and Authenticity in Two Indian Scientists* (Delhi: Oxford University Press, 1995), and Shiv Visvanathan, *Carnival for Science: Essays on Science, Technology and Development* (New York: Oxford University Press, 1997).
59. Pheng Cheah, 'Grounds of Comparison', *Diacritics*, 29 (1999), 3–18, p. 17.
60. Among the inspirations was Chen Kuan-hsing, *Asia as Method: toward Deimperialization* (Durham NC: Duke University Press, 2010). The project is related to other cognitive reorientations toward the 'global south', such as: Ranajit Guha and Gayatri Chakravorty Spivak, eds., *Selected Subaltern Studies* (New York: Oxford University Press, 1988); Fernando Coronil, 'Beyond Occidentalism: toward Non-Imperial Geohistorical Categories', *Cultural Anthropology*, 11 (1996), 51–87; Linda Tuhiwai Smith, *Decolonising Methodologies: Research and Indigenous Peoples* (London: Zed Books, 1999); Dipesh Chakrabarty, *Provincializing Europe: Postcolonial Thought and Indigenist Difference* (Princeton University Press, 2000); Walter D. Mignolo, *Local Histories/Global Designs: Coloniality, Subaltern Knowledges, and Border Thinking* (Princeton: Princeton University Press, 2004); Raewyn Connell, *Southern Theory: the Global Dynamics of Knowledge in Social Science* (Sydney: Allen and Unwin, 2007); and Jean Comaroff and John L. Comaroff, *Theory from the South: or, How Euro-America is Evolving Toward Africa* (New York: Routledge, 2015).
61. Bruno Latour, *We Have Never Been Modern*, trans. Catherine Porter (Cambridge, MA: Harvard University Press, 1993). See Arjun Appadurai, 'Global Ethnoscapes: Notes and Queries for a Transnational Anthropology', in *Recapturing Anthropology: Working in the Present*, ed. Richard G. Fox (Santa Fe: School of American Research Press, 1991), pp. 191–210; Marilyn Strathern, 'The New Modernities', in *Property, Substance and Effect: Anthropological Essays on Persons and Things* (London and New Brunswick, NJ: Athlone Press, 1999), pp. 117–135; Marshall Sahlins, 'What is Anthropological Enlightenment? Some Lessons of the Twentieth Century', *Annual Review of Anthropology*, 28 (1999), 1–23; and Dipesh Chakrabarty, 'The Muddle of Modernity', *American Historical Review*, 116 (2011), 663–675.
62. Simon Schaffer, 'Late Victorian Metrology and its Instrumentation: a Manufactory of Ohms', in *Invisible Connections: Instruments, Institutions and Science*, eds Robert Bud and Susan E. Cozzens (Bellingham, WA: SPIE Optical Engineering Press, 1992), pp. 23–56, p. 23.
63. James A. Secord, 'Knowledge in Transit', *Isis*, 95 (2004), 654–672, p. 660.
64. Steven Shapin, 'Placing the View from Nowhere: Historical and Sociological Problems in the Location of Science', *Transactions of the Institute of British Geographers*, 23 (1998), 5–12, pp. 6–7.
65. David J. Stump, 'Afterword: New Directions in the Philosophy of Science Studies', in Galison and Stump, eds, *The Disunity of Science*, pp. 443–450, p. 448. See also Marc Berg and Annemarie Mol, eds, *Differences in Medicine: Unraveling Practices, Techniques, and Bodies* (Durham NC: Duke University Press, 1998).
66. Michel Callon, 'Four Models for the Dynamics of Science', in Jasanoff, Markle, Petersen, and Pinch, eds, *The Handbook of Science and Technology Studies*, pp. 29–63, p. 60. See also John Law and John Hasard, eds, *Actor-Network Theory and After* (Oxford: Blackwell, 1999).
67. Bruno Latour, 'A Well-Articulated Primatology: Reflections of a Fellow Traveler', in *Primate Encounters: Models of Science, Gender, and Society*, eds Shirley C. Strum and Linda M. Fedigan (Chicago: University of Chicago Press, 2000), 358–381, p. 365.



68. Bruno Latour, *The Pasteurization of France*, trans. Alan Sheridan and John Law (Cambridge MA: Harvard University Press, 1988), p. 140.
69. Bruno Latour, *Pandora's Hope: Essays on the Reality of Science Studies* (Cambridge MA: Harvard University Press, 1999), p. 47. See also his *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes: Open University Press, 1986).
70. John Law, 'After ANT: Complexity, Naming and Topology', in Law and Hasard, eds, *ANT and after*, pp. 1–14, p. 6.
71. Shapin, already cited (n. 65), p. 7. Later versions of ANT have occupied a more varied terrain than smoothly colonising network space. For example, Marianne de Laet and Anne-Marie Mol, 'The Zimbabwean Bush Pump: Mechanics of a Fluid Technology', *Social Studies of Science*, 30 (2000), 225–263. For an extended discussion of the imperial assumptions of much ANT, see Anderson, already cited (n. 6).
72. Bruno Latour, 'Spheres and Networks: Two Ways to Reinterpret Globalization', *Harvard Design Magazine*, 30 (2009), 138–144, pp. 140, 142, 143. See also Bruno Latour, 'Networks, Societies, Spheres—Reflections of an Actor-Network Theorist', *International J. Communication*, 5 (2011), 796–810.
73. Kapil Raj, *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650–1900* (Basingstoke: Palgrave Macmillan, 2007), p. 23. Nonetheless, he refers in the book to Basalla's 'epoch-making paper' (p. 3). See also his 'Beyond Post-colonialism ... and Postpositivism: Circulation and the Global History of Science', *Isis*, 104 (2013), 337–347, and 'Networks of Knowledge, or Spaces of Circulation? The Birth of British Cartography in Colonial South Asia in the Late Eighteenth Century', *Global Intellectual History*, (2017) <http://dx.doi.org/10.1080/23801883.2017.1332883> Raj was influenced by Sanjay Subrahmanyam: see Claude Markovits, Jacques Pouchepadass, and Sanjay Subrahmanyam, eds, *Society and Circulation: Mobile People and Itinerant Cultures in South Asia, 1750–1950* (Delhi: Permanent Black, 2003).
74. Raj, already cited (n. 74), p. 22.
75. Raj, already cited (n. 74), p. 18.
76. Raj, already cited (n. 74), p. 11.
77. Raj, already cited (n. 74), p. 228. Admittedly, Raj occasionally refers to the importance of long-distance commercial networks in spreading science. For more on 'circulation', see Lissa Roberts, 'Situating Science in Global History: Local Exchanges and Networks of Circulation', *Itinerario*, 33 (2009), 9–30.
78. Fa-ti Fan, 'The Global Turn in the History of Science', *East Asian Science, Technology and Society Journal*, 6 (2012), 249–258, p. 52.
79. Tsing, 'already cited (n. 57), p. 463.
80. Warwick Anderson, 'Waiting for Newton? From Hydraulic Societies to the Hydraulics of Globalization,' in *Force, Movement, Intensity: the Newtonian Imagination and the Humanities and Social Sciences*, eds Ghassan Hage and Emma Kowal (Melbourne: Melbourne University Press, 2011), 128–135.
81. Warwick Anderson, *The Collectors of Lost Souls: Turning Kuru Scientists into Whitemen* (Baltimore: Johns Hopkins University Press, 2008).
82. Pekka Hämäläinen and Samuel Truett, 'On Borderlands', *Journal of American History*, 98 (2011), 338–361, p. 358. Borderlands history, emphasising 'cultural transience, mobility, and intermingling' (p. 341), should be distinguished from frontier history, which reinscribes narratives of sovereignty. See also Warwick Anderson, 'Edge Effects in Science and Medicine', *Western Humanities Review*, 69 (2015), 373–384. The term 'concept work' is from Ann Laura Stoler, *Duress: Imperial Durabilities in Our Times* (Durham NC: Duke University Press, 2016).
83. Engseng Ho, 'Afterword: Mobile Law and Thick Transregionalism', *Law and History Review*, 32 (2014), 883–889, p. 889. Evidently, Ho is mobilising Clifford Geertz's notion of 'thick description' ('Thick Description: Toward an Interpretive Theory of Culture,' in his *The Interpretation of Cultures* [New York: Basic Books, 1973], pp. 3–30).