

Race by Another Name: Vernacular Race Science, Caste and the Making of Serosocial Identities in India, c. 1918-60.

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A serendipitous encounter during the Great War left a brilliant Polish-Jewish scientist and his wife stranded at a Greek outpost with a small contingent of British and French Imperial troops. This chance encounter led to the birth not only of a new branch of science, i.e. sero-anthropology, but also a novel theory about the origin of the blood group 'B' in India. In the following decades, this theory evolved and metamorphosed within British India through transnational scientific conversations as well as its resonances with South Asian identity politics. As the meanings of the isohaemagglutinin B morphed, the transnational meanings of race were repeatedly tripped up. In due course Indian sero-anthropology produced a range of serosocial identities located as much in blood sera as in embedded socialities. After 1960, however, these serosocial identities were gradually overcome by purely sanguinary identities whose truth was located exclusively in the blood devoid of any sociality.

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Blood, they say, will out. Yet it has remained, at least historiographically speaking, one of the best-kept secrets in the history of Indian science. Even as histories of colonial and postcolonial science and medicine have appeared thick and fast in the recent years, picking up speed from its hesitant beginnings some two and a half decades ago, historians have hardly seen blood. Yet, from the beginning of the twentieth century, blood gained an unprecedented new visibility and importance in medicine and science. One can notice this importance in the sudden efflorescence of the poetics of blood. Bram Stoker's *Dracula* appeared in 1897. Its 1922 filmic adaptation as *Nosferatu* made the blood sucking count something of a global phenomenon.¹ In India rumours of vampirism were already rife from the 1890s.² Much of this appetite for vampire lore was stoked no doubt by the large-scale blood transfusions attempted during the Great War.³ On the other hand, courts from the middle of the first decade of the twentieth century increasingly began to see police depend upon bloodstains and their chemical analysis for the conviction of criminals and murderers.⁴ From the second decade of the twentieth century, it became known that blood tests could reveal or clarify doubtful paternity.⁵ This was also the time

¹ Davison, 'Introduction', pp. 19-40.

² Arnold, *Colonizing the Body*, pp. 218-26.

³ Newquist, *The Book of Blood*, pp. 55-62.

⁴ Sutherland, *Blood-Stains*.

⁵ Anon., 'Hereditary Blood Qualities', p. 3.

that pathological blood-work became an increasingly important component of medical diagnosis through the gradual adoption of the Germ Theory.⁶ In a somewhat ironic circularity, the microscopic visualisation of blood in turn soon yielded further causes to study it. A number of new illnesses were identified and defined as being some form of deficiency in the blood or the other. These were the various types of anaemias.⁷ Over and above everything else, family relations were still designated as 'blood relations' and the phrase carried legal import.⁸ While many scientists sought to develop new languages of heredity inscribed upon an updated conceptualization of blood.⁹ All things considered, the last decade of the nineteenth century and the first three decades or so of the twentieth, were a rather 'bloody' time.

Much of the expansion in the operational and poetic fields of blood was enabled by Karl Landsteiner's discovery in 1901 of the major blood groups.¹⁰ Though the crucial Rhesus factor was not discovered till 1937—seven years after Landsteiner had won the Nobel Prize for his discovery of the blood groups—it allowed a new understanding of human immunity to emerge. It also finally facilitated the development of blood transfusion as a reliable and frequently life-saving technology that in turn revolutionised many areas of medicine ranging from surgery to childbirth. But blood groups also had another consequence. As superficially invisible, but yet inheritable, characteristics, they gave a wholly new twist to the conceptualisation of human bodily difference.

Modern racial classifications are generally dated from the eighteenth century and most attempts until the dawn of the twentieth century had built upon superficially visible bodily differences.¹¹ Thus, in India, the classic anthropometric work of colonial officers such as Sir HH Risley in the nineteenth century had dwelt on features such as shape of the head, shape of the nose, height, skin-tone and so forth.¹² There was no way of speaking of somatic difference unless it could be co-related to some form of superficially visible distinction. With the discovery of blood groups, all this changed. It became suddenly possible to imagine bodily difference that was inheritable, but yet invisible to the naked eye. Additional grounds for greater objective reliability of blood groups as the bases of racial classification also derived from the fact that being invisible they were not affected by deliberate, cultural choices humans made in the choice of breeding partners.¹³ Moreover, whereas physical measurements had long proved notoriously slippery, being prone to errors through observer bias, instrument error or simply from sampling errors, blood groups undoubtedly seemed to be a much firmer ground upon which to build solid, scientific arguments. It is perhaps not co-incidental that both modern eugenics and statistics are children of the same ancestors, viz. Sir Francis Galton and Karl Pearson. One of the first contributions of classical statistics was a 'theory of error'. By accepting human observational error as an inescapable component of all scientific observations, it

⁶ See, Gradmann, *Laboratory Disease*.

⁷ Wailoo, *Drawing Blood*.

⁸ See for instance, the case of Jagannath Prasad Gupta Vs. Runjit Singh in *The Indian Law Reports (Calcutta Series – New Series)*, pp. 237-46. Or, see the case of Bahal Singh anr. Vs. Mubarik-Un-Nissa anr. in *The Indian Law Reports (Allahabad Series)*, pp. 77-81.

⁹ See for instance, Eastbrook & Davenport, *The Nam Family*.

¹⁰ Anon., 'Karl Landsteiner—Biographical'.

¹¹ Barkan, 'Race and the Social Sciences'.

¹² For Risley, see Bates, 'Race, Caste and Tribe in Central India'.

¹³ Gannett & Griesemer, 'The ABO Blood Groups', p. 125.

sought to rectify this by creating mathematical operations that would accommodate and correct for the error.¹⁴ In India, the earliest application of this statistical principle was by PC Mahalanobis to the anthropometric measurements of HH Risley amongst others.¹⁵ But an alternate way to minimise error, naturally, was to reduce as far as possible the role of human intermediation or observations. The simple chemical reaction that revealed the blood group of an individual, by being a purely technical operation greatly reduced the scope of human error by reducing in turn the scope of human judgement, than the anthropometric methods used thus far.¹⁶

These advantages of serology, i.e. the study of blood sera, over anthropometry for organising human bodily difference did not become obvious till after the Great War. It was in the inter-war period that blood groups emerged as the pre-eminent basis for conceptualising races. In India, this serological discussion about races came to intersect, overlap and dominate discussions about the nature, origin and mutual relationship of different castes.

In this article, it is these overlapping conversations between race and caste, using serological techniques and data, that I wish to interrogate. My particular focus will be on the shifting metonymies of the blood group B. At different times, during the period under review, the blood group B came to stand in—that is, very literally became a metonym—for a variety of national, caste and regional identities. Throughout the period a vague image of race, understood as a purely biological and transnationally relevant category, haunted these metonymies of blood group B. I will argue that two inter-related consequences followed through this. First, the sociological and political identities of ‘nation’, ‘caste’ and ‘region’ were effectively re-calibrated as ‘serosocial identities’, i.e. identities that were located at the intersection between blood and sociality. Second, the interpolation of ‘race’ with the socio-political categories of ‘nation’, ‘caste’ and ‘region’, produced a new and distinctive discursive and disciplinary object of study in the Indian works on sero-anthropology. This new object was what I will call ‘serosociality’, namely, a hybrid object of knowledge born out of the braiding of serology and sociology. I will end by arguing that after 1960 both serosocial identities and the serosociality were overwhelmed by an emergent set of ‘sanguinary identities’ that were seen to be located exclusively in the blood and that the study of sociality was redundant to their apprehension.

Sciences of Race

Blood may have been invisible to the historian, but neither race nor the efforts to scientifically study bodily difference have been absent from the historian’s table. Peter Robb’s edited volume, *The Concept of Race in South Asia*, remains perhaps the best-known foundational work on the issue. The essays in the volume largely set the tone and outlined the key themes in the study of race in the Raj. Robb defined the concept of race as one which ‘included any essentialising of groups of people which held them to display inherent, heritable, persistent or predictive characteristics, and which thus had a biological or quasi-biological basis’. Most of the essays in the volume examined cultural

¹⁴ See Porter, ‘Statistics and Statistical Method’.

¹⁵ Mahalanobis, ‘Analysis of Race Mixture in Bengal’.

¹⁶ Gannett & Griesemer, ‘The ABO Blood Groups’, p. 125.

deployments of 'race', rather than the scientific deployments of the concept. But the insights developed in these essays are useful to the study of race-science as well. Many of the authors such as John Rogers, John Brockington and Dagmar Hellman-Rajanayagam, noted the lack of an exact fit between colonial notions of 'race' and sub-continental categories of difference such as caste. Others like Indira Sengupta-Chowdhury, Christophe Jaffrelot and Javed Majeed, described the complexities arising from attempts to nationalise the notion of 'race'. Susan Bayly, in one of the two essays that directly touched upon the science of race, noted the diversity of views maintained by colonial ethnographers. Crispin Bates, most cogently for us, charted the history of anthropometry.¹⁷

The issue of race and particularly its relationship to caste have also frequently come up in discussions of the social and cultural impact of colonialism. Bernard Cohn, Thomas Metcalf and Nicholas Dirks in the course of their very different discussions have described colonial ethnology and its racialised imagination.¹⁸ Similarly, Christopher Pinney's work on the visual culture of colonial ethnology has illuminated yet another dimension of this nexus between colonial ethnology and a racialised imagination.¹⁹ This nexus has also been made explicit in several poignant studies on 'criminal tribes'.²⁰

Despite the cogent insights developed by this rich and diverse extant historiography, one area has remained particularly wanting in historical scrutiny. This is the role of *science as such*—viz. science understood in its practical, technical and materially operational modalities, rather than merely as 'discourse'—in constituting, perpetuating and shaping the racialised imagination of Indian society. Even in works such as Bates' pioneering essay on anthropometry, it is the discursive elements of the racial theories that are attended to while the practical, technical and material modalities of the science of anthropometry are largely ignored. This neglect is even more obvious once we step into the twentieth century and the technical and scientific aspects of science become more arcane. Practically no historiographic attempt has been made to engage the science of race in its practised (rather than discursive) form in either the nineteenth or the twentieth centuries in South Asia.

In fact, there has been very little work on race in twentieth-century South Asia. Much of the interest generated by volumes such as Robb's has remained confined to the nineteenth century or been projected further back to the eighteenth century.²¹ Few have tried to bring the discussion forward into the twentieth century. Two exceptions to this general neglect are Sarah Hodges' discussion of middle-class eugenicists and eugenic societies and Sekhar Bandyopadhyay's investigation of PC Ray's efforts to use anthropometry and eugenics against the caste system.²² Though neither Hodges nor Bandyopadhyay engage the material and practical aspects of the science that informs the actions of their respective protagonists, they highlight two important changes in the twentieth century. Hodges does much to point towards the transnational linkages of the

¹⁷ See Robb ed., *The Concept of Race in South Asia*.

¹⁸ Cohn, 'The Census, Social Structure and Objectification in South Asia'; Metcalf, *Ideologies of the Raj*, particularly chapters 3 & 4; Dirks, *Castes of Mind: Colonialism and the Making of Modern India*.

¹⁹ Pinney, *Photography & Anthropology*.

²⁰ See for instance, Nigam, 'Disciplining and Policing the 'Criminals by Birth''.

²¹ See for instance, Mehta, *Liberalism and Empire*. Mantena, *Alibis of Empire*.

²² Bandyopadhyay, 'Caste, Social Reform and the Dilemmas of Indian Modernity'. Hodges, 'South Asia's Eugenic Pasts'.

eugenics movements. Though she almost entirely avoids credentialed scientists and focuses on what might be dubbed the popular science dimension of eugenics, the transnationalism she describes is, as we shall soon see, equally an element of *science as such*. Similarly, Bandyopadhyay, despite being only marginally concerned with the actual sciences of anthropometry and craniometry, perceptively points out how the largely homogenous nineteenth-century culture of colonial raciology was becoming by the second decade of the twentieth century much more sophisticated, politically diverse and methodologically plural. The scientific study of race was no longer undertaken by a ethnologists alone, and their political, methodological and disciplinary backgrounds were much more diverse. Yet, neither Hodges or Bandyopadhyay nor any other historian has systematically explored these diverse entanglements of science and race with a putative focus on the practical, material and operational aspects of such science.

This is not to suggest that the histories of science and race have remained unacquainted with each other. Indeed, several historians of science, such as David Arnold, Mark Harrison, Deepak Kumar, Waltraud Ernst, Kavita Philip, Abha Sur and others have demonstrated how race frequently inflected scientific and medical theories and careers.²³ But here, 'race' is what intervenes and refracts a discussion of scientific engagement with other things. The object of critical investigation is not the 'sciences of race'. By the 'sciences of race', I mean those sciences that are putatively, rather than incidentally, focussed on the definition, measurement and analysis of inheritable, human bodily difference. Craniometry, Cacogenics, and Sero-anthropology are just some of these 'sciences of race'.

In stark contrast to South Asian historiography, history of science in other postcolonial locations has robustly increasingly engaged the material, technical and practical dimensions of the science of race. Warwick Anderson's works on South East Asia have been particularly inspirational in centre-staging the technical and material aspects of science and in unabashedly opening them up for scrutiny.²⁴ Nancy Stepan's work on Latin America has also been hugely influential in making the nexus between politics and the science of race explicit.²⁵ Recently, the new interest in the history of genetics has opened up the field even further and enriched it with studies from Brazil, Mexico, South Africa and elsewhere.²⁶

At the same time, amongst medical anthropologists working on South Asia, we can notice a very similar move towards engaging the practical, technical and material elements of science, instead of remaining satisfied with the discursive constructions. Lawrence Cohen's fascinatingly original work on transfusions and transplants, Aditya Bhardwaj's work on stem cells, Kaushik Sunder Rajan's work on bioinformatics and Jacob Copeman's work on blood transfusion, all demonstrate a new confidence in critically engaging the materiality and the technicalities of science.²⁷ Though none of them speak of what I have been calling the sciences of race, nonetheless in their tenor and

²³ Arnold, "An Ancient Race Outworn". Harrison, *Climates and Constitutions*; Kumar, 'Racial Discrimination'. Ernst, 'Idioms of Madness'. Philip, *Race, Resources and Modernity*. Sur, *Dispersed Radiances*.

²⁴ Anderson, *Colonial Pathologies*; also, *The Collectors of Lost Souls*.

²⁵ Stepan, *The Hour of Eugenics*.

²⁶ Lindee & Santos, 'The Biological Anthropology of Living Human Populations'.

²⁷ Cohen, 'The Other Kidney'. Bharadwaj & Glasner, *Local Cells, Global Science*. Rajan, *Biocapital*. Copeman, *Veins of Devotion*.

intellectual resources they have much to offer any historical investigation of the sciences of race in South Asia.

It is within this scholarly landscape that I will undertake my interrogation of the transnational conversations that animated the then-newly emergent world of sero-anthropology in the Inter-War period. These conversations were explicitly concerned with the question of racial identity and the mutual relationship of races. Materially, they were operationalised through the extraction, analysis and discussion of blood samples. And, though the conversations were neither always between Indians nor in India, India came to play a significant part in them.

A New Race Science: 'B' is for India

The conversations over blood and race as well as India's crucial role in it commenced in earnest in Thessaloniki (then known as Salonika) in Greece. The man who started the conversation was a brilliant Polish bacteriologist and serologist, Ludwik Hirszfild. At the start of the Great War, Hirszfild and his wife, Hanna, had given up lucrative professorial and nursing careers in Vienna to volunteer in Serbia. Being generally opposed to the War, they had felt the medical service was a good way of being part of the huge drama unfolding around them without compromising on their principles. They had been drawn to Serbia initially by the terrible typhus epidemic that had broken out at Valjevo.²⁸ Gradually however, they came to identify with the hopes and aspirations of the Serbians and became quite close to the Serbian military, political and medical elite. When the Serbians lost and were driven into Greece, the Hirszfilds briefly went back to Vienna. But soon the Serbian authorities invited them back and requested them to take charge of a central bacteriological laboratory for the Serbian army in exile in Greece. Ludwik Hirszfild's serological interests led to the Serbian army becoming one of the first to use large-scale blood transfusions. He also trained a brilliant cadre of Serbian bacteriologists who avidly took to the study of blood and the cultivation of germs. In the meantime, a small combined Anglo-French force had also landed in Greece in the hopes of aiding the Serbians, but by the time they arrived the Serbians had already lost. The Anglo-French troops, however, decided to remain in Greece. It was this unexpected coming together of various troops in Thessaloniki that stoked Hirszfild's interest in anthropology.

'Here we were within an unique agglomeration of various races and nations', wrote Hirszfild, 'and a project requiring many years of study and travel could be accomplished within several months'.²⁹ The original idea of studying the world's populations from a serological perspective, Hirszfild said, had its genesis many years ago in Heidelberg amidst conversations with Emil von Dungern. Hirszfild had been a post-doc under Dungern and described the latter as a 'spiritual poet and an aristocrat' while also crediting him as being a 'creative force'.³⁰ It was during their time in Heidelberg that Dungern and Hirszfild had discovered the inheritability of blood groups.³¹ It was this in turn that, amongst other things, allowed legal paternity elimination tests to emerge. Whatever the original context for thinking about inheritance of blood groups and racial difference,

²⁸ Hirszfild, *The Story*, p. 31.

²⁹ *Ibid.*, p. 58.

³⁰ *Ibid.*, pp. 15-16.

³¹ *Ibid.*, p. 20.

Hirszfeld was clear that it was the unexpected opportunities provided by the Great War that made it a reality.

It is important to clarify here that Hirszfeld's interest in race did not make him either a bigot or a racist in any simple sense of the term. A Jewish convert to Catholicism, he would himself later suffer greatly at the hands of the Nazis. He also confessed to being 'shocked' by the racist behaviour of the British army officers towards Indian soldiers and doctors: 'The only thing that shocked us ... was their [the British] attitudes toward the Hindus. An English physician would never sit at a table with a Hindu physician, and yet we met there several highly educated and cultivated Hindu physicians'.³² Yet, despite these enlightened and liberal sentiments, there was a soft-racism to Hirszfeld. Throughout his memoirs, he frequently stereotypes nationalities—though always in a good-humoured way. Thus the French—men and women, officers and nurses—are repeatedly presented as being sexually promiscuous, the Britons are similarly frequently shown to be snobbish and conceited and so on. We witness this soft racial tone even in his descriptions of the tact that was necessary to actually collect blood samples from the different armies at Thessaloniki. 'We had to speak in a different way to each nation', he wrote. 'It was enough to tell the English that the objectives were scientific. We permitted ourselves to kid our French friends that we would find out with whom they could sin with impunity. We told the Negroes [sic] that the blood tests would show who deserved a leave; immediately, they willingly stretched out their black hands to us'.³³

Based on the blood from those stretched out black and white hands, the Hirszfelds made a striking discovery. They found that while all four major blood groups were present in all nations, their proportions were significantly different. Amongst Europeans there was a preponderance of the blood group A, whilst amongst Asians and Africans there were more people with the blood group B. Reasoning from this data, they thought that, 'a group factor had been formed on two opposite ends of the world: the A factor somewhere in northern Europe, and the B factor in Asia, perhaps in the distant highlands of Tibet or somewhere in India'.³⁴

Initially, the Hirszfeld study created no stir. In fact, the husband and wife team who co-authored the paper announcing their findings struggled to get it published. The *British Medical Journal* to which they had first submitted it, sat on the paper for months and then eventually rejected it. This, combined with the exigencies of war, meant few people knew of their findings. Yet, both the Hirszfelds were convinced that they were onto something really significant. Ludwik Hirszfeld fondly remembered later how, during an unexpected air-raid, his wife rushed to save their research data and findings for this project before anything else.³⁵ Unable to publish, the couple took to lecturing local chapters of Medical Societies in a bid to get their findings out. Hanna Hirszfeld spoke at the English Medical Society in Thessaloniki on June 5th 1918, whilst her husband spoke at the French counterpart.³⁶ It was anonymous English members of the Salonika Medical Society [Thessaloniki was then called Salonika] who urged the Hirszfelds to resubmit their paper to another British journal, *The Lancet*. Still keen to get the research published and

³² *Ibid.*, p. 51.

³³ *Ibid.*, p. 58.

³⁴ *Ibid.*, p. 58.

³⁵ *Ibid.*, p. 60.

³⁶ *Ibid.*, p. 58.

convinced of its value, they submitted it to the *Lancet* in September 1918. But the War was about to end and in the chaos and happiness at the cessation of hostilities, the paper seemed to have dropped through the cracks once again. The Hirszfels never heard back from *The Lancet* and assumed its fate had been no better than on the previous occasion. It came as a huge and welcome surprise then that in November 1919, back in their native Warsaw, Ludwik Hirszfel unsuspectingly picked up a newspaper and saw their research reported on at length. *The Lancet*, despite misspelling their names, had published the paper in October 1919.³⁷ Later, the French journal, *L'Anthropologie*, also published their paper.³⁸

In the published version, the Hirszfels first went over the older research of Dungern and Hirszfel, explaining in detail through the experiments they had done on dogs, the basics of antibodies and agglutinins and the exact mechanisms of their inheritance, stressing that such inheritance is unrelated to the inheritance of visible traits. They then presented their serological data from Thessaloniki and provided a set of statistical analysis of the data. Lisa Gannett and James Griesemer have demonstrated how the tabular and graphic visual forms in which the data and its statistical relationships were represented can be used to reveal a subtle interplay of objectivity and subjective judgements on the part of the Hirszfels.³⁹ By ordering the graphs according to the incidence of the blood group B for instance, the European, Inter-mediate and Asian groups fall into a neat array. Had the Hirszfels chosen to draw the same graph according to the incidence of the blood group A however, the ordering would have been much less consonant with extant cultural and geographic identities. They bolstered the impression of perfect objectivity by creating a single series of numerical values called the 'biochemical race index'. Based on this indexical number, the Hirszfels concluded that, 'The figures arrived at by us are most easily explained by the assumption that A and B had different points of origin and that there are two different biochemical races that arose in different places. In this case the mutual infiltration of the two races is the cause of the varying proportion of A and B. Since the greatest frequency of B is found in India, we should then look for the moment on India as the cradle of one part of humanity—namely, of the biochemical race B'.⁴⁰

The impact generated by these publications was immense. It gave rise to an entirely new branch of anthropology that came to be called sero-anthropology. Hirszfel's discovery was compared to that of Retzius, who had first suggested that cranial measurements were racially distinctive. Less than a decade later in 1928, Ludwik Hirszfel, a rank outsider to anthropology, was invited to the International Congress of Anthropologists in Amsterdam to lead an entire section of the Congress devoted to sero-anthropological research.⁴¹ By the end of the century, the Hirszfels were being retrospectively hailed as the pioneers of yet another new discipline, i.e. 'anthropological genetics'.⁴²

³⁷ *Ibid.*, p. 59.

³⁸ Gannett & Griesemer, 'The ABO Blood Groups', p. 121.

³⁹ *Ibid.*, pp. 133-35.

⁴⁰ Hirszfel & Hirszfel, 'Serological Differences', p. 679.

⁴¹ Hirszfel, *The Story*, p. 59.

⁴² Gannett & Griesemer, 'The ABO Blood Groups', p. 121

As sero-anthropology grew as a field and with it grew awareness of Hirszfeld's research, the theory that the blood group B had originated in India also grew apace. Though its direct influence on the Human Genome Diversity Project [HGDP] remains a matter of some scholarly controversy, it is unquestionable that the legacy of the blood group variation research remained incredibly powerful until the 1950s. It is also clear that at least some of the later scientists working on the HGDP, such as the famous Luca Cavalli-Sforza, acknowledge their intellectual debt to the earlier blood group research begun by the Hirszfelds. In any case, eminent scientists such as Laurence Snyder, JBS Haldane and William Boyd, all followed the Hirszfelds in attempting to visually map the dispersal of blood groups across the globe.⁴³ By 1954, when Arthur E Mourant produced a comprehensive synthetic work bringing together the entire gamut of sero-anthropological knowledge accumulated in the foregoing decades, he could assert that, 'nearly all Indian and Pakistani communities have very high B and rather high A frequencies...' and again that, 'The high B frequencies...characterise all India and Pakistan...'.⁴⁴

One of the most powerful validations for the Indian origin of blood group B came from a spate of studies on the Roma (formerly called 'Gypsies') people of Europe. By the 1920s many held that the Roma were originally from India and the calculations of biochemical race index values seemed to produce an exact fit between them and the Indians. In December 1921, two researchers in Berlin published findings which claimed to demonstrate that, in the case of the Roma 'a complete accordance with the Indians was proven. The isohemagglutinins A and B is, therefore, a racial character by which races can be differentiated, even after centuries'.⁴⁵ Another study published in the May of 1922, calculated that the biochemical racial index value of the Roma to be 0.6, a figure said to be 'nearly the same as in Indians [which the Hirszfelds had calculated to 0.5]. Philology teaches that the Gypsies [sic.] wandered in from India about the year-1200'.⁴⁶ Such studies, in the process of reinforcing the notions of racial difference towards continental Europe's internal others, ended up further disseminating and popularising the identification of India with the blood group B.

Vernacular Race Science: 'B' is for Low Caste

In the early 1920s, the cutting edge of anthropology in India represented by men such as Nelson Annandale, the brilliant Scotsman who went on to become Director of the Zoological Survey of India, was still looking towards craniometric measurements as the pre-eminent technology for negotiating human racial difference. In fact, it was through a chance meeting with Annandale that the then young, Cambridge-returned, PC Mahalanobis, got interested in human races. In 1922, Mahalanobis published his very first statistical work. A collaborative effort based of Annandale's data, it concerned the measurements of the Anglo-Indian (Eurasian) crania and stature.⁴⁷ There was no hint of any interest in blood groups as a basis of racial difference. In fact till the very end of the

⁴³ *Ibid.*, p. 125.

⁴⁴ Mourant, *The Distribution of the Human Blood Groups*, p. 104.

⁴⁵ Verzar & Weszeesky, 'Researches on Racial Biology', p. 736.

⁴⁶ Verzar, 'New Experiments on Isohemagglutinins', p. 167.

⁴⁷ Mahalanobis, 'Anthropological Observations on the Anglo-Indians of Calcutta'.

1930s, Mahalanobis continued to work on both statistical and mechanical tools for improving and perfecting craniometric methods of racial identification.⁴⁸ The huge interest generated in Europe by the work done by the Hirszfels was slow to catch on in India.

One of the most important people who eventually popularised the serological approach in India was an American scientist, Dr Eileen Macfarlane. Dr Macfarlane was a remarkable woman for her time. She had obtained two successive doctorates respectively from the University of Michigan (1928) and the University of London (1932). At least one of her PhDs was in genetics. The origins of her interest in India remain somewhat obscure. A Rotary Club pamphlet announcing a lecture by her in Calcutta states that she first went to India as a Visiting Professor in Genetics at the Maharaja College of Science in Travancore in 1933.⁴⁹ A marriage notice in *The Times of India*, in October 1934, announced her marriage to James Borthwick Macfarlane of Madras at the Presbyterian Church in Putney.⁵⁰ We are, hence, left wondering whether she had originally gone to Travancore to be near her future husband, or if love had bloomed whilst in India. In any event, the marriage provided reason for her to return repeatedly to India. Twice thereafter she returned as University of Michigan's 'Collaborator in Asian Research'. In this capacity, she carried out blood group surveys 'from Malabar and Madras to the borders of Tibet'.⁵¹ She published at least eight academic articles on blood groups and race in India. She was also known to have been a popular public speaker on the topic to the Anglophone audiences in India.

In one of her very first articles, Macfarlane spelt out her objections to the Hirszfeld data. 'Unfortunately, they [i.e. the Hirszfels] lumped together all the data from Gurkhas, Garowas, Jats, Kumaons, Rajputs with that from low caste people from all over India who were in the Labour Transport Corps and called the lot 'Indians'. Because the sample was racially heterogeneous, the classical Hirszfeld data are of little value anthropologically, although they have served to draw attention to India as possible locale for the mutation which produced the agglutinin B'.⁵² This was not all. Further errors, Macfarlane pointed out, had taken place due to R. Ottenburg having republished the Hirszfeld data in America after changing 'Indians' to 'Hindus'. Naturally, not all Indian 'Sepoys' were 'Hindus' and calling them as such was comparable to calling 'the American Doughboys Protestants'.

Macfarlane's critiques of the Hirszfeld data were not unique. One other study, conducted by Major RH Malone of the Indian Medical Service and MN Lahiri, MB, had published some of these critiques in October 1928.⁵³ But Macfarlane's intervention is significant for two reasons. By publishing widely and regularly, she made the criticisms much more visible. The Malone and Lahiri article, which Macfarlane duly cited and acknowledged, had appeared in an Indian journal and had not been followed up by any further publications by the authors. More importantly, Malone and Lahiri, having

⁴⁸ Mahalanobis, 'On the Accuracy of Profile Measurements with a Photographic Profiloscope'.

⁴⁹ Anon., 'Eileen Macfarlane'.

⁵⁰ Anon., 'Macfarlane-Erlanson'.

⁵¹ Anon., 'Eileen Macfarlane'.

⁵² Macfarlane, 'Blood Group Distribution in India with Special Reference to Bengal', p. 225.

⁵³ Malone & Lahiri, 'The Distribution of the Blood Groups in Certain Races and Castes of India', pp. 963-968.

criticised the Hirszfels for lumping dissimilar groups together, had then gone ahead and done something similar themselves. They had adopted HH Risley's categorisation of racial types in the Indian population and had organised their own data accordingly. In fact, the Malone and Lahiri article was something of a hybrid between the craniometric racial anthropology of yore and the emergent new sero-anthropology. As a conspicuous testament to its hybrid character, the article carried both tabular blood group data and photographs of racial 'types' as is seen in earlier works such as in Risley's *People of India*.⁵⁴ Macfarlane pithily pointed out this shortcoming in the Malone and Lahiri piece.

Having refuted the tendency to 'lump', she came out openly in favour of splitting. 'Hindu society is endogamous in varying degrees within the caste, and miscegenation is rare in most regions', she stated. Consequently, she proposed that, 'Each caste is therefore a biological strain differing more or less from the others. For a true picture of Indian blood-group distribution adequate unmixed samples from any of the numerous castes and tribes are needed from each province'.⁵⁵ This was to become one of the most powerful and influential ideas. Even contemporary genomic research continues—mostly without knowing its source—to work within this framework laid down by Macfarlane.

Whether caste-groups can be viewed as being the discrete 'biological strains' that Macfarlane thought them to be is doubtful. We know that at least notionally, a large number of particularly middling or lower castes speak of their origins having been through the miscegenation of any two Varna, or occasionally even Jati groups. That apart, there is now enough evidence to show that castes undergo both fission and possibly even fusion over time.⁵⁶ The Mahisyas of Bengal, for instance, originated as a distinct group within the parent caste of Kaibarta, initially calling themselves 'Chasi Kaibartas' before eventually gaining recognition as a distinct group altogether.⁵⁷ The opposite process of fusion seems to have brought together some Chamars and Dusadhs in northern India giving rise to a new caste called Chamar-Dusadh.⁵⁸ Even more complicated is the question of actual, everyday sexual mores. It is doubtful to what extent didactic discourse about strict endogamy was ever actually adhered to. In fact, it is quite plausible to hold that the highest castes, through their monopolistic access to social status and wealth, had the greatest chances to take sexual liberties with members of the lowest and most impoverished castes.

Despite such possible opposition, Macfarlane's proposition held firm. She backed it up with a large number of publications that always presented new data from still newer caste groups. Like Snyder before her, she also modified the 'biochemical race index' to accommodate the shift in scientific consensus introduced in the mid-1920s by Bernstein. Unlike the earlier hypothesis that held that there were only two inheritable alleles that determined blood groups and that the group O resulted from an absence of alleles, Bernstein proposed the existence of three alleles. Bernstein's hypothesis had almost universally been accepted as being correct by the end of the 1920s. Macfarlane also developed the graphic techniques used by the Hirszfels to present their data. In place of different nationalities, she merely presented different castes. The tabular forms too were

⁵⁴ On the visual culture of anthropometry, see Pinney, *Photography & Anthropology*.

⁵⁵ Macfarlane, 'Blood Group Distribution in India', p. 226.

⁵⁶ Hardgrave Jr. 'Caste: Fission and Fusion'.

⁵⁷ Sinha, 'Unabingsha Shataker Shesh'.

⁵⁸ Crooke, *The Tribes and Castes*, p. 177.

developed to take account of the fact that, unlike at the time of the pioneering Hirszfeld study, now there were several different studies undertaken at different times and by different scholars and data could be drawn from multiple such studies. Consequently an extra column was introduced to identify the study from which the data had been extracted. Gannet and Griesemer have rightly argued that these graphic and tabular forms constitute what Bruno Latour speaks of as the 'circulating referent', i.e. that part of an object of study that can be extracted from its embedded material context and re-inserted into a wholly distinct, but more pliable, material form.⁵⁹ We can see the changes wrought by Macfarlane in the same light. She sought to make the 'circulating referent' more pliable while at the same time referring to a different object, i.e. castes rather than nations.

Her major conclusion was to stand Hirszfeld's 'Indian origin for the B agglutinin' hypothesis stand on its vertical axis. Whereas the Hirszfelds had traced the dispersal of the blood groups horizontally across the face of the earth, Macfarlane now traced it vertically down the caste hierarchy. She concluded that, 'That B has been in India for millennia and may have originated from the ancestors of the lower castes of the north-east, where the highest concentrations are found now, whence it has diffused into the higher castes. The amounts of O and B vary inversely; therefore there might be genes for O in these low caste people with a relatively high mutation rate to B'.⁶⁰ Elsewhere she wrote, 'If agglutigen B has spread through the world largely from India ... then one of the chief sources here seems to have been the ancestors of the present Depressed Classes'.⁶¹ Other studies, done both by Macfarlane and others, seemed to add grist to this contention. One study for instance, reported the 'unusually high percentage of the Group B' in a sample extracted from 80 members of the 'low' Bagdi caste from 24 Parganas in Bengal.⁶² Another study from the United Provinces spoke of a similarly 'unusually high percentage' of B amongst the so-called criminal tribes, referring particularly to Bhatu, Karwals and Doms.⁶³ One early study from the Tamil country even argued that low caste Hindus and tribals differed in the former having larger number of B individuals amongst them.⁶⁴ Looking ahead at the discussion of regionalism in the next section, it is cogent to point out that Macfarlane had been explicit in clarifying that caste mattered more than region when it came to the question of group B distribution and origin. She wrote that, 'The blood group proportions among these people [i.e. Depressed Classes] in the Deccan are strikingly similar to those of the Depressed Classes of Bengal south of Calcutta ... It is over a thousand miles from Calcutta to Hyderabad, Deccan...'.⁶⁵

Vernacular Race Science: 'B' is for Bengal

Throughout most of the 1930s, when Eileen Macfarlane produced the bulk of her work, both the stalwarts and the rising stars of the Indian scientific establishment who were

⁵⁹ For the 'circulating reference', see Latour, 'Circulating Reference'.

⁶⁰ Macfarlane, 'Blood Group Distribution in India with Special Reference to Bengal', p. 236.

⁶¹ Cited in Chatterji & Mitra, 'Blood Group Distribution of the Bengalis', p. 204.

⁶² Macfarlane, Un-titled Letter to the Editor on Bagdi Blood, p. 284.

⁶³ Majumdar, 'Blood Groups of the Doms'.

⁶⁴ Aiyappan, 'Blood Groups of the Pre-Dravidians'.

⁶⁵ Macfarlane, cited in Chatterji & Mitra, 'Blood Group Distribution of the Bengalis', p. 204.

interested in anthropometry, remained aloof from serology. The hugely influential Biraja Shankar Guha, who originally mooted the idea of establishing a separate Anthropological Survey of India, in his own biometric survey done as part of the census operations in 1931 completely ignored serology and stuck to external physical measurements.⁶⁶ The lack of interest amongst the mainstream meant large-scale resources were unavailable and hence major, systematic surveys largely impossible. Researchers like Macfarlane achieved much despite these drawbacks, but their sample sizes were usually small and scattered. More than the numerically small samples, what hampered these studies was the somewhat serendipitous nature of collection. Macfarlane's Cochin studies for instance, were based on 600 blood samples taken from out-patients attending the General Hospital in Ernakulam.⁶⁷ In another study conducted jointly by Macfarlane and Sasanka Sekhar Sarkar of the Bose Institute in Calcutta, though the sample seemed to be large and systematic on the surface, it was actually an aggregate of smaller batches of serendipitously collected data. Thus the data on the Mundas had all been collected from amongst mine-workers in Singhbhum by Macfarlane herself, whereas the data on the Kanikkars had been obtained at the Travancore University by a certain Dr CO Karunakaran and there are no details about who the subjects were.⁶⁸ None of this is surprising when we learn that Guha, despite having the much stronger support of the Census office behind him had actually had to collect measurements for Bengali Brahmins and Kayasths, not in Bengal, but in Ahmedabad where many of them worked together as officers in textile mills and for Marathas, amongst students of Ferguson College, Pune.⁶⁹

In the fourth decade of the twentieth century, this trend began to change. The small studies based on opportunistic collection of data such as those conducted by Macfarlane gradually came to be replaced by massive, regionally focussed, systematic studies. In fact, by the fateful year of 1947, it was stated at the annual meeting of the Indian Science Congress, that serology's claims to being a 'suitable tool for anthropological taxonomy' had been firmly 'established'.⁷⁰ Four major studies undertaken in this period reflect the trend towards larger, more systematic and explicitly regionally focussed enquiry. Three of these studies were done by an young Cambridge-educated, Bengali anthropologist called Dhirendranath Majumdar (better-known by his initials as DN Majumdar). The other major study was conducted by a talented Chitpavan Brahmin anthropologist from Pune with a PhD from Berlin, who would go on to become one of the greatest Indian anthropologists of the twentieth century, Irawati Karve. Geographically, two of the studies were on different parts of the erstwhile Bombay State and its neighbouring areas, while one each was on the United Provinces and Bengal. None of the studies were exclusively sero-anthropological. They combined the collection of serological data with more traditional anthropometric data. They also reflect a growing trend towards more complex statistical analysis of the data—often through collaborations with professional

⁶⁶ Guha, *Census of India, 1931*.

⁶⁷ Macfarlane, 'Preliminary Note on the Blood Groups of Some Cochin Castes'.

⁶⁸ Macfarlane & Sarkar, 'Blood Groups in India'. That Dr. Karunakaran's data was obtained at the Travancore University is mentioned in LA Krishna Iyer's *The Travancore Tribes and Castes*, p. 44.

⁶⁹ Mahalanobis, Majumdar & Rao, 'Anthropometric Survey of the United Provinces,' p. 97.

⁷⁰ Datta-Majumdar, 'Anthropology During the War: IV India', p. 161.

statisticians—as well as a deeper engagement with genetics, particularly with ‘Bernstein’s Genetic Theory’.⁷¹

DN Majumdar undertook the first of the above-mentioned four major surveys in the United Provinces between 1941–43. Majumdar had completed his PhD from Cambridge in 1935, but he does not seem to have found a faculty position right away. At the time, an Indian ICS officer, Bhagwan Sahay, had become the Superintendent of the provincial census operations slated to begin in 1941. The census in India had long been the de-facto home of both anthropology and anthropometry and Sahay naturally took an interest in the subject.⁷² Keen to include anthropometric data in the provincial census, it was Sahay who found Majumdar. Sahay persuaded the provincial government and the University of Lucknow about the utility of such a project and got them to fund the survey. A grant-in-aid was also made to the Statistical Laboratory that had been set up by PC Mahalanobis in Calcutta to compute the results.⁷³ The survey collected and analysed 5,000 blood samples.

Interestingly, the main publication of the anthropometric data resulting from the survey, led to both the elision of most of the serological data as well as any prominent discussion of Bhagwan Sahay’s role. Instead, the Census Commissioner, MWM Yeatts, was given most of the credit for initiating the survey. The serological data similarly appeared separately in the first issue of a new journal launched by DN Majumdar called *The Eastern Anthropologist*.⁷⁴ Whereas top-notch statisticians like Mahalanobis and CR Rao worked on the rest of the anthropometric data, an obscure statistician, Kunwar Kishen, employed in the Agricultural department of the United Provinces, did the statistical analysis of the serological data. These may be small but significant hints that the mainstream of Indian anthropology still considered serology to at best be of marginal interest. Majumdar himself must have, however, been convinced of its value to have published it in the very first issue of the journal he launched.

It was in the course of the UP survey that Majumdar began to gradually articulate a regionalist perspective on the caste question. JH Hutton, Census Commissioner of the 1931 Census, had repudiated the earlier theory advanced by Risley and others that caste-status co-related with racial difference. Majumdar, in his opening remarks to the UP survey, emphatically rejected Hutton’s position. He quoted Hutton as writing that, ‘the view that caste corresponds with race has been rejected ... the Brahman of the United Provinces has a long head (c.i. 73.1), he of Bengal a round one (c.i. 79.0); the cephalic index of the Chamar of the UP is 72.8, and that of the Bihar Chamar is 76...’. Having thus stated Hutton’s position in detail, Majumdar went on to say that, ‘This however, does not dispute the racial basis of caste. It probably means that the Brahmins of Bengal differ from those of the United Provinces; just as the Brahmins of UP differ from the lower castes of the province...’.⁷⁵ Incidentally, Majumdar here was parroting a thesis advanced by Mahalanobis in 1927. In an article entitled the ‘Analysis of Race Mixture in

⁷¹ ‘Bernstein’s Genetic Theory’ referred to a mathematical expression developed by Felix Bernstein to calculate the real and expected frequencies of the three alleles responsible for the ABO blood group system.

⁷² Census Commissioner, MWM Yeatts, called Anthropology in India the ‘peculiar concern of the census’. Yeatts, ‘Foreword’ in Mahalanobis et al., ‘Anthropometric Survey of the United Provinces’, p. 91.

⁷³ Majumdar, *The Fortune of Primitive Tribes*, p. xvii

⁷⁴ Majumdar & Kishen, ‘Blood Groups Distribution’.

⁷⁵ Mahalanobis et al. ‘Anthropometric Survey of the United Provinces’, p. 93.

Bengal', Mahalanobis had re-analysed Risley's data from 1891 to argue that, 'Bengal Brahmans resemble the other Bengal castes far more than they (the Brahmans) resemble castes from outside Bengal'.⁷⁶ This close fit between Majumdar and Mahalanobis' position was to eventually prove most fruitful for the former.

In any case, in the serological data he published independently with Kunwar Kishen, Majumdar once again arrived at broadly the same conclusion about regional unity of castes. Naturally, the discussion of serological data turned once again to the vexed question of the blood group B. Referred to now, in keeping with the Bernstein theory, as the q gene and juxtaposed to the p and r genes (responsible respectively for the blood groups A and O), Majumdar and Kishen wrote that, 'If p is the rare gene in India, and it is so, for as we proceed from Europe to Asia the incidence of the gene grows less and less, the dominance of p in any given social group may indicate the status of the group...The rarity or predominance of p in any given population, therefore, assumes biological significance'. Having thus established p as a crucial marker of racial difference, they then went on to baldly state that, 'it must be admitted that so far as the UP castes are concerned the variation in the frequency of blood groups cannot be regarded as racially significant'.⁷⁷ They even invoked an old theory advanced by John C Nesfield in 1885 that argued that caste differentiation was based on function rather than race. Majumdar and Kishen did not however, invoke Nesfield to propagate a non-racial theory of caste, but rather to demonstrate the peculiar uniformity of the 'Aryo-Dravidian' admixture of races that typified the United Provinces.⁷⁸ Overall their report clubbed groups of castes together into 'clusters' of closely related or similarly composed groups—just as Majumdar's anthropometric data had done—within an unified regional setting.

By the second half of the 1940s, this argument in favour of the regional unity of castes began to acquire more and more currency. The UP survey was followed up by Mahalanobis organising a similar survey in 1945 to be conducted all over the then as-yet undivided Bengal. Mahalanobis personally invited Majumdar to take charge of the survey and found the financial, infrastructural and political support to undertake the survey at a highly volatile period in the history of Bengal. This was fortuitous for the young Majumdar who was still without an academic position. The reasons why Mahalanobis took Majumdar under his wings remain somewhat obscure, but the former confessed that one of his motivations for arranging the Bengal survey was to check if his own re-analysis of Risley's data had been accurate. The fact that Majumdar had already explicitly expressed his sympathy for a position akin to Mahalanobis' might then clearly have played a part in organising the survey and entrusting Majumdar with it. As expected, Majumdar's results corroborated Mahalanobis'.⁷⁹ Eventually, Majumdar would go on to become a member of the powerful Research Programmes Committee of the Planning Commission, when Mahalanobis became its all-powerful Chairman through the patronage of Nehru.⁸⁰

⁷⁶ Mahalanobis, 'Analysis of Race Mixture in Bengal', p. 304

⁷⁷ Majumdar & Kishen, 'Blood Group Distribution', p. 14.

⁷⁸ *Ibid.*, p. 15. For Nesfield's theory see Dirks, *Castes of Mind*, p. 212.

⁷⁹ Mahalanobis, 'Foreword'.

⁸⁰ Madan, 'DN Majumdar'. For Mahalanobis' powerful position in the Planning Commission and the political controversy around it, see Kudaisya, "A Mighty Adventure".

Having undertaken two major regional surveys and argued for the regional unity of castes in both regions, Majumdar was recruited into what was rapidly becoming one of newly independent India's worst regional conflicts, viz. the Marathi-Gujarati conflict over the re-organisation of the erstwhile Bombay State. Though there are long antecedents to the movement, a focussed political movement for the separation of the Marathi-speaking areas as a separate province with Bombay as its exclusive capital began to take shape from the early 1940s. Through the formation of a series of organisations such as the *Samyukta Maharashtra Sabha* (1940), the *Samyukta Maharashtra Parishad* (1946) and eventually, the *Samyukta Maharashtra Kirti Samiti* (1955), Marathi politicians such as Senapati Bapat, Shankarrao Deo, SM Joshi, SA Dange and others, and intellectuals such as PK Atre, DR Gadgil amongst others, politicised and mobilised what was eventually a successful, but also highly emotive and occasionally violent struggle for a separate Marathi-speaking state. Gujarati politicians on the other hand had seemed largely in favour of the bilingual state, mainly because of their economic and cultural ties with the city of Bombay. By 1956 however, an association called the *Maha Gujarat Janata Parishad* emerged and began to agitate in favour of the break-up and the creation of a Maha Gujarat State.⁸¹

One of the most controversial areas in this bitter dispute was the tribal-dominated Dangs. Some, like BG Kher, erstwhile Chief Minister of Bombay, claimed that the Dangs were more closely allied to the Marathi areas, while Gujarati leaders like KM Munshi and Indulal Yagnik made the opposite case. It is not difficult to see the resonances of these movements with the kind of work that Majumdar had been doing. TN Madan, in a biographical essay on Majumdar, mentions that he was one of the first anthropologists to be interested in the interaction of tribal and non-tribal societies. His publications, such as *The Fortunes of Primitive Tribes* had eminently demonstrated his credentials as well as his framework for uniting tribal and non-tribal societies through physical and cultural traits. Finally, his strong commitment to regional unity augured well with the tendencies of the linguistic movements of western India. It was hardly surprising therefore that a group called the 'Gujarat Research Society' of Bombay would invite and fund him to undertake a survey of the Gujarati-speaking regions.

What is remarkable in fact is how explicit this connection between the rising political temperature and Majumdar's survey is and how, strangely, it has thus far passed without comment. The publication resulting from Majumdar's study and published by the Gujarat Research Society is plainly titled *Racial Realities of Cultural Gujarat*. Published in 1950—more than a decade before an actual political entity called Gujarat state was to emerge—'cultural Gujarat' was far from being a self-evident toponym. The subtitle tellingly clarified the issue: 'Report on the Anthropometric, Serological and Health Survey of Maha Gujarat'.⁸²

In the report, working on the serological data together with his old collaborator, Kunwar Kishen, Majumdar once again presents a regionally unified picture. 'Except for the tribal groups and that also in a general way', wrote Majumdar, 'no other social group in cultural Gujarat can be safely placed in any particular constellation'. There could only be two possible reasons for this: '(1) Cultural Gujarat is a racially homogenous area ... (2) Cultural Gujarat had a migrant population. From the early pre-historic times, Gujarat

⁸¹ Palshikar, 'Breaking Bombay', pp. 27-74.

⁸² Majumdar, *Cultural Gujarat*.

had swayed her influence all over India so that the composition of the population in Gujarat could not remain stable'.⁸³ Developing the latter point, Majumdar represented 'cultural Gujarat' as a culture marked by its constant and even inter-mixture where wide racial variations could be seen within members of the same caste. Both the possible reasons to explain the serological data therefore produce the same conclusion. Whether owing to the even-keeled heterogeneity of an original population, or a tremendously assimilative culture that rendered each racial trait evenly distributed throughout every section of society, Gujarat was a homogenous region.

Even on the vexed tribal question, while Majumdar did notice differences between them and the rest of the groups, he also simultaneously pointed out that the tribal groups were in themselves heterogeneous. Thus, the Bhils of different areas had significantly different serological frequencies meaning that 'the Bhil is a generic name including different racial strains'. Moreover, in respect of their *p*-values and if we read the corresponding '*q*-values a little liberally', we can put all the tribal groups, Bhils, Kolis, etc. in one 'constellation'. Having thus put them in a constellation, Majumdar argued, we could see that the Kolis—particularly from Kutch—were the conduits who had 'channelled alien blood into the veins of other castes, as well as their own'.⁸⁴

Just as Majumdar's survey resonated with the interests of the Maha Gujarat movement, Irawati Karve's survey resonated with the *Samyukta Maharashtra* movement. Whereas Majumdar was a bit of an outsider to the controversy, Karve was very much in the thick of it. After 1948 as the Congress-led central government in Delhi repeatedly opposed and sought to stamp out the *Samyukta Maharashtra* movement, they had attempted to undermine the movement by presenting it as a movement dominated and controlled by Chitpavan Brahmins from Pune. One anonymous pamphlet depicted the movement as a covert Chitpavan plot to restore Peshwa rule. The Congress had hoped to cash into an older strand of anti-Brahminism (and specifically anti-Chitpavanism) promoted by British through the restored Bhonsle principality at Satara, as well as the eruption of anti-Chitpavan violence in the wake of Nathuram Godse's (a Chitpavan) assassination of Gandhi.⁸⁵ Yet, the *Samyukta Maharashtra* movement at the time, even under the socialist leadership of Joshi and Dange, repeatedly fell back on the memories of Peshwa rule to drum up support. Crucial meetings, such as that called by DV Potdar to protest against the first case of police violence on agitators (1955) or the public meeting presided by Joshi that launched the *Samyukta Maharashtra Samiti* (1956), all took place at Shaniwarwada, the historic Pune fort that had been the seat of Peshwa power in the eighteenth century.⁸⁶ Karve, as both a Chitpavan Brahmin and a long-term resident of Pune, obviously would have been intimately caught up in the swirling cultural and political currents around her to a much greater degree than Majumdar could have been.

Yet, Karve's conclusions and framework often seemed almost to be the mirror image of Majumdar's work on Gujarat. A much better writer than Majumdar, Karve was

⁸³ *Ibid.*, p. 10.

⁸⁴ *Ibid.*

⁸⁵ Patterson, 'The Shifting Fortunes of Chitpavan Brahmins'; For the development of anti-Chitpavan resentment at the Bhonsale court and the promotion of the Satara princelings by the British as a counterweight to Brahmin influence, see O'Hanlon, *Caste, Conflict and Ideology*, pp. 15-50. For the Congress pamphlet alleging *Samyukta Maharashtra* would bring back Peshwa rule see, Palshikar, 'Breaking Bombay', p. 71.

⁸⁶ Palshikar, 'Breaking Bombay', pp. 58, 64.

more convincing in laying out the uniqueness of the Marathi-speaking region. She argued that despite being a region suffused with the Sanskritic culture of the north, the Marathi-speaking areas were sufficiently influenced by the Southern cultures to have given it a unique character. She also hinted at cultural reasons for biological specificity by drawing attention to the unique version of cross-cousin marriages allowed amongst the Marathi-speakers that was neither like the cross-cousin marriages of the South, nor like the cousin-marriage-averse North.⁸⁷

Karve almost echoed Majumdar when she wrote that, 'It might seem as if the division of Indian society in castes cuts through the linguistic regions and that investigation about Indian races should follow the caste classification and not the linguistic region ... the caste system however almost never cuts through the linguistic divisions ... A Maharashtra Brahmin will only marry a Maharashtra Brahmin and not a Brahmin from another linguistic region'.⁸⁸ In discussing the serological data, Karve, once again almost exactly like Majumdar, stated that, 'Though there are large and significant differences between different castes as regards these gene frequencies [i.e. those of p , q and r], the several castes fail to arrange themselves in any understandable pattern'.⁸⁹ In fact, even those patterns that had emerged on the basis of anthropometric measurements seemed to be blurred by serology. Karve concluded that, blood group data was of 'little value' in discovering 'caste configurations within Maharashtra'. Despite the apparent frustration expressed in her conclusion, in a slightly tangential light it might well be read as an assertion that despite the many visible differences that anthropometry figured, defined and measured, there was an underlying unity of all Marathi-speakers—namely, that Chitpavan Brahmins, contrary to propaganda, were not an alien group at odds with the rest of Marathi-speaking society, but literally of the same blood as the rest.

None of this is to suggest that either Karve or Majumdar were dishonest scientists. Far from it. They did not tailor their conclusions to suit their political sympathies or cultural identities. They tried their best to produce accurate and legitimate scientific conclusions. But the relationship between the creative imagination of a scientist and the scientific conclusions she produces need not be recuperated solely through the figure of corruption and perversion of truth. As Ashis Nandy, in his fascinating study of JC Bose and Srinivasa Ramanujan, has so effectively taught us, the imagination has a range of subtle and pre-reflexive paths by which to connect a scientist's identity, aspirations and anxieties to her creative imagination.⁹⁰ Rather than suggesting corruption of Majumdar or Karve's science, what I would want to draw attention to is the sheer fact that popularity of serological surveys seemed to grow in inverse proportion to their capacity to actually generate any conclusive insights. I will suggest that perhaps somewhere along those mysterious paths that connect the imagination to the subliminal forces that shape a scientists' identity, what contributed to the growing appeal of sero-anthropological surveys was precisely its inability to distinguish. Its promise to submerge all visible difference into a deeper sympathy and commonality of blood, not unlike that famous final scene in

⁸⁷ Karve & Dandekar, *Maharashtra*, pp. 7-8

⁸⁸ *Ibid.*, p. 10.

⁸⁹ *Ibid.*, p. 118.

⁹⁰ Nandy, *Alternative Sciences*.

the film *Amar, Akbar, Anthony* where visible difference is seen to be erased by the underlying commonality of blood.⁹¹

Despite the almost mirror-image quality of these surveys, there is one striking fact that distinguishes three of these studies from the fourth. In the surveys of UP, Maha Gujarat and Maharashtra, while the allegedly greater frequency of *q* or blood group B amongst the lower castes is touched upon, its question of origin is left unexplored. Possibly the hallowed tradition of arguing that northern or western Indian upper-castes are 'Aryan' cousins of Europeans, might have made it difficult to reconcile such traditions with the new arguments for regional unity and yet be able to accommodate the question of the origin of the *q* gene. The one exception to this was Bengal. Unencumbered by the anthropological traditions similar to those about the northern Brahmins, and in fact heir to a long legacy of arguments that demonstrated the dissimilarity of Bengali Brahmins from northern or western Brahmins, the Bengal survey was much more forthright in discussing the high prevalence of the *q* gene amongst Bengali Brahmins. 'The high incidence of B Blood in Bengal is in agreement with the results obtained by others who have worked on blood groups in Bengal. The Brahmins of Bengal have a lower incidence of A than is obtained amongst the Brahmins of Uttar Pradesh...'.⁹² Later, in 1960, another researcher, DK Sen, once again emphatically confirmed that both upper and lower castes in Bengal had a higher percentage of B blood than elsewhere in India (though the respective proportions between the Bengali castes did vary). Finding even lower frequencies for A blood amongst the Bengali upper castes.⁹³ Mourant, in his authoritative work of synthesis in 1954, therefore found it proper to state that though high frequencies of B blood characterised all of India and Pakistan, it reached its 'highest values in the north, and more especially the north-east...'.⁹⁴

By the end of the 1950s, when the Bengal survey was belatedly published, the high incidence of the *q* gene for all castes, including Brahmins, was therefore widely accepted. From being a metonym for all of India in 1918, to one for the lower-castes in the mid-1930s, by the late 1950s, B blood was the metonym for Bengali identity.

Debating Serosociality

By the end of the 1950s, large scale serological surveys began to give way once more to smaller-sized studies. More importantly, the nature of these later studies, done after 1960, was quite distinctive. Armed with narrower, but also increasingly more precise conceptions of genetics, aided by better technologies for preserving and transporting blood and undertaken in the laboratory rather than the field, these later studies began to peer deeper into the blood sample instead of the wider social relations from within which the blood sample had been extracted. By contrast, one of the most characteristic and significant features of the sero-anthropological studies in the period we are studying were the debates over what should be the proper social unit of serological analysis.

Majumdar, in the introduction to his *Fortune of Primitive Tribes*, wrote that, "Race" is a biological concept. The ideal definition of race would be 'a biological group

⁹¹ On *Amar, Akbar, Anthony* and the poetics of blood transfusions see Cohen, 'The Other Kidney'.

⁹² Majumdar, 'Blood Groups of Tribes and Castes of Bengal', p. 321.

⁹³ Sen, 'Blood Groups and Haemoglobin Variants', p. 163.

⁹⁴ Mourant, *The Distribution of the Human Blood Groups*, p. 104.

or stock possessing in common an undetermined number of associated genetical characteristics by which it can be distinguished from other groups and by which its descendants will be distinguished under conditions of continuous isolation'. But he hastened to point out that there were no statistical definitions of words like 'purity' and 'isolation'.⁹⁵ As a result, 'isolation' and 'purity' became terms that had to be defined as matters of fact. The object of the sero-anthropological studies of the period between 1918 and the 1950s, in seeking to determine the appropriate social unit of racial purity, had to constantly read the abstract, biological and transnationally recognised category of race through a series of shifting, locally relevant and lived socialities. It was this image of race refracted through other socialities that we call serosociality and it is this serosociality—as an object of study and debate—that disappears from later genetic investigations after 1960.

Serosociality as an object of serious academic debate and study first surfaced when researchers like Macfarlane, Sarkar, Malone, Lahiri and others began to question the automatic national 'isolation' and racial 'purity' that had been assumed by the Hirszfels. In pointing out that different groups within India retained their purity and isolation from each other through strict endogamy, they opened up the relationship of the social and the biological to scientific enquiry. Once this was done, the matter continued to be hotly contested and indeed worked out on a case-by-case basis.

The initial emphasis on caste as the basic unit of endogamy and hence isolation began to gradually crumble. BK Chatterji and AK Mitra, in a relatively little-cited study for instance, took issue with Macfarlane and others for clubbing Bengali castes from Rarh, Banga and Barendra together.⁹⁶ These are cultural regions within Bengal, but they also function as notional homelands that further splinter castes internally into smaller endogamous units. Thus Bengali Brahmins or Kayasthas, for instance, from Rarh would only inter-marry with those fellow Bengali Brahmins or Kayasthas who claimed descent from Rarh (the actual residence here mattered little, and even one resident for centuries in a eastern district could claim descent from Rarh in the west). LD Sanghvi, who studied blood groups in the Bombay State, described an even more complicated picture of serosociality. To begin with, he clarified that the caste was not always an endogamous unit. Amongst the castes he chose to discuss, the Chandraseniya Kayastha Prabhus (CKP) were an endogamous unit, but the caste 'Brahmin' were a collection of several endogamous units that retained their own distinctiveness. Broadly the Brahmins of Bombay, Sanghvi explained, were divided into four basic endogamous groups by reference to notional geographic origin, viz. Gurjar, Maharashtra, Sarasvat and Karnataka. Of these again, the Gurjar comprised of 93 divisions. One of these 93 divisions was called Nagar and had six subdivisions of its own. One of these six divisions of the Nagar, was the Vadnagar. The Vadnagar, once again, were subdivided into four groups, i.e. Suddha, Bhikshuk, Dungarpur and Barad. The Maharashtra Brahmins in their turn, were divided into two broad divisions, i.e. the Desasth and the Konkanasth. The latter was an endogamous unit by itself, but the former was again divided into Rigvedis and Yajurvedis. Once again, the Rigvedis were endogamous, but the Yajurvedis further

⁹⁵ Majumdar, *The Fortune of Primitive Tribes*, p. ix.

⁹⁶ Chatterji & Mitra, 'Blood Group Distribution of the Bengalis', p. 198.

divided into Sukla and Krishna groups. One of the groups studied by Sanghvi and Karve, was a subgroup amongst the Krishna Yajurvedi Maharashtra Brahmins.⁹⁷

Adding yet another level of complexity on top of the vexed question of endogamy was the issue of intra-group sectional exogamy. Explaining the system Sanghvi wrote that, 'There is no parallel to this type of exogamy in Western society; it would mean more or less that a Mr Smith cannot marry a Miss Smith, but has to marry a non-Smith, say a Miss Brown'.⁹⁸ What he was referring to was the system of *Gotras*, whereby members of the same endogamous group are still thought to be descended from a mythical sagely founding father and are forbidden to marry within that notional lineage. Thus every endogamous group is internally separated into various exogamous *gotras*. This naturally affects the mathematical probabilities of genetic inheritance. To his consternation, Sanghvi rightly pointed out that the census data was absolutely useless when it came to this kind of information about endogamy.⁹⁹

Clearly what we see in these cases is a vigorous engagement with actual socialities of conjugation and reproduction. Moving away from the seemingly self-evident and overly simplistic categories of caste, endogamy, etc., what researchers like Sanghvi were proposing and putting into effect was a framework where sociology shaped biology in historically and culturally specific ways. What they were studying was therefore a hybrid object born out of the interpolation of biology with sociology, viz. serosociality.

Studying serosociality was not simply a way of accurately defining apriori units of serological analysis. Sometimes, especially later on, it was also invoked aposteriori to explain trends noticed in the serological data. In Aiyappan's early study for instance, having argued that the Group B was rare amongst the so-called 'Pre-Dravidian tribes', he explained the 7-6% occurrence of B in one such tribe, i.e. the Paniyans, by stating that 'may have been introduced through miscegenation with the men of the plains which though of an imperceptible kind has been going on since the importation of large numbers of estate coolies from the plains'.¹⁰⁰ In Majumdar's serological survey of the UP similarly, when the data for the Doms seemed to contradict the Bernstein Theory, he proceeded to argue that, 'The criminal [sic.] Doms are a heterogeneous group, the women are very lax in morals and clandestine prostitution is the rule rather than exception'.¹⁰¹ About the Tharus, who too contradicted expected frequencies, he wrote that, 'The dominance of women in Tharu society has asserted itself in a great latitude in sex matters [sic.], and extra-marital intimacy both inter-tribal and intra-tribal has resulted in the high incidence of illegitimacy in the tribe'.¹⁰² While there is a clear difference in the deferential tone in which apriori serosociality is discussed and the morally denunciatory tone that articulates aposteriori discussions of serosociality, both of them, in their own ways speak of lived socialities and how these affect biological sera.

Serosociality then, to reiterate, was a specific object of study. Just as the discipline of economics constituted 'the economy' as an object of study, or the discipline of zoology constituted the 'zoon' as the object of its study, so too did these sero-

⁹⁷ Sanghvi, 'Comparison of Genetical and Morphological Features'.

⁹⁸ *Ibid.*, p. 388.

⁹⁹ *Ibid.*, p. 389.

¹⁰⁰ Aiyappan, 'Blood Groups of the Pre-Dravidians', p. 494.

¹⁰¹ Majumdar & Kishen, 'Blood Group Distribution', p. 11.

¹⁰² *Ibid.*, p. 11.

anthropological works constitute a specific object. Unlike in cases such as ‘the economy’ or ‘the zoon’ though, serosociality always remained implicit rather than being defined explicitly. As an object of study, ‘serosociality’ was that space—a hybrid between the visible surfaces of sera and the social—upon which these studies operated or deployed their technical tools and from which they extracted their truth. Serosociality becomes therefore the locus of truth and the operational terrain upon which the technics and analytics of this specific Indian branch of sero-anthropological research could be distributed.

Moreover, let us not forget that eventually these studies are crucially concerned with the issue of clarifying identities. The truth of the identities that are extracted from analysis of the peculiar and hybrid locus of serosociality might also then, by affiliation, be dubbed serosocial identities. Serosocial identities are simply those identities which are established through the scientific study of serosociality. They are neither exclusively based on blood sera nor simply on social practices. They arise from the technical operation upon a hybrid serosocial space constituted by both sera and sociality.

One further point, in this regard, bears clarification. Usually a specific object of scientific study mutually defines the science that studies it. Thus ‘the economy’ for instance is that which is studied by the discipline of ‘economics’, just as discipline in turn is that which studies ‘the economy’. The object and the discipline engender and define each other. The question might then arise as to which discipline defines and engenders the object titled ‘serosociality’. I argue that Indian sero-anthropological studies of the period under review constituted an autonomous and distinctive discipline. The autonomy however, is often obscured from view because these studies appear to simply be a part of the larger international body of work on blood groups at the time. But this apparent seamless unity with the larger body of international works, I will argue, is a red herring. In fact, the repeated attempts to read race through caste and the consequent deep engagement with serosociality, I will contend, made the Indian studies unique and distinctive from the many other non-Indian studies undertaken at the time. The progressive inflection of the abstract and transnational category of race by more and more locally embedded notions of caste-based sociality effectively constituted Indian sero-anthropology as a separate and unique science in terms of both its object of study and the protocols of research it followed.

The distinctiveness of the Indian studies becomes all too clear when we compare them to the developments in colonial Africa. As Lundy Braun and Evelyn Hammonds have point out, in Africa it is precisely in the 1930s and 1940s that the fluid groupings are reformatted by anthropologists into seemingly stable and biologically discrete units of ‘population’ which can then be subjected to genetic investigations.¹⁰³ In India, by contrast, though the language of ‘populations’ was increasingly used, the issue of their discreteness of fluidity was repeatedly complicated by the engagement with serosocialities.

It would be cogent to reiterate at this juncture that the object of disciplinary knowledge is not simply an artefact of the imagination. If it were so, it would be little more than fiction. At best it would be designated ‘science fiction’. To become a real object of scientific knowledge, the object imaginatively or conceptually delineated, as a

¹⁰³ Braun & Hammonds, ‘Race, Populations and Genomics’.

discrete entity must also be capable of actual technical and analytic operations and manipulations. As a result, any realised object of scientific knowledge is intimately connected to the specific technologies through which it is apprehended. Post-WWII changes in tools and technologies, for instance, radically transformed the fundamental conception of 'genes'.¹⁰⁴ Similarly, real material objects that are studied by specific sciences only become knowable to us through specific protocols of nomenclature, visualisation and inscription adopted by those sciences. Once again, the post-War shift in the inscriptional practices of the 'gene' has had a tellingly transformative impact on our understanding of the matter.¹⁰⁵ Serosociality therefore, while being a real object of scientific investigation was also an object engendered by specific tools, technologies and inscription protocols.

The interpolation of sociology and biology in itself, it is worth confessing, is far from being novel. Indeed, Chris Renwick has recently argued that sociology's early roots in Britain were actually deeply entangled with biological concerns.¹⁰⁶ Similarly, in more contemporary times, Paul Rabinow has identified the existence of biosocialities that are formed through the socialisation of biological designations.¹⁰⁷ What makes each of these hybrids specific is the operational context where specific technologies, inscription protocols, tools, institutions and theoretical positions converge. Thus, Renwick's biologically tinged sociology progressively disappears after 1904, Rabinow's biosociality is hardly around in the 1930s and 40s and serosociality is only available in the brief inter-war years and completely disappears after 1960.

Sanguinary Identities

The demise of serosociality as a meaningful object of scientific investigation after the 1950s was not so much a simple matter of the changing tides of academic interest. Instead, the very tools, technologies and inscriptional protocols that had engendered serosociality were up-staged by new tools, technologies and inscriptional practices, thereby undoing serosociality from the inside. At the core of this shift was a newer and narrower notion of genes. As we have seen, 'genes' were not absent from serological work in the 1920s and 1930s, their conception however was fairly vague. There was little agreement on whether they were merely formal or heuristic constructs or actual entities. Even as late as in 1954, a leading plant geneticist, LJ Stadler, was still robustly agnostic about whether the gene really existed or not.¹⁰⁸ Hence many, including the Indian sero-anthropologists who analysed serosocialities, remained vague about the exact nature of the somatic inheritance.

After the War, a host of new tools and technologies entered genetic research. Electron microscopes, radioactive tracers, ultracentrifuges etc. began to produce a much narrower but also more concrete idea about genes and inheritance.¹⁰⁹ Simultaneously, another major shift affected research into heredity. Most of classical genetics had been

¹⁰⁴ Burian & Zallen, 'Genes', p. 440.

¹⁰⁵ Bangham, 'Writing, Printing and Speaking'.

¹⁰⁶ Renwick, *British Sociology's Biological Roots*.

¹⁰⁷ Rabinow, 'Artificiality and Enlightenment'.

¹⁰⁸ Burian & Zallen, 'Genes', p. 439.

¹⁰⁹ *Ibid.*, p. 440.

based on studies of the fruit-fly *Drosophila melanogaster*. During the War, a range of newer, simpler and more pliable organisms were deployed in research. These included bacterium like *Pneumococcus pneumoniae* and the bread mould, *Neurospora*. One of the consequences of this new research was to point towards the capacity, under certain specific circumstances, for environmental factors and nutrition to impact on bodily inheritance. Another emergent result of the new research was to highlight the complexity of gene-action. It gradually became clear that genes could be switched on or off in the presence of other genes or certain chemicals.¹¹⁰

These new insights gradually confused the picture of racialised inheritance that had informed the investigation of serosocialities. In 1947, Majumdar writing in a book he co-authored with Karve titled *Race Problems in Asia* articulated some of the confusion and vagueness about the nature of inheritance. He pointed to some of the confusing results obtained through blood group surveys and particularly the redundancy of the 'biochemical race index'. Going further he also suggested the possible role of unhealthy environmental factors such as the endemic incidence of malaria as a factor behind some of the high B blood group concentrations.¹¹¹ Yet, he remained staunchly defensive of the basic concepts of race and even blood groups. Read together with other anthropometric data, he thought that the broad outlines of the notion that blood group frequencies were racially significant, was a sound one.¹¹² He argued emphatically that, 'Truth certainly cannot advance by denying the existence of large groups of mankind characterised by more or less by distinctive physical traits'.¹¹³

After the 1950s, this commitment to investigating human physical difference remained intact. But the confusion arising from the new researches overwhelmed the old paradigms for conducting such investigation. In fact, the very loci upon which such investigation focussed changed dramatically. Serosociality as an object of study gradually disappeared altogether. A study carried out in 1962-63 by the Haematological Research Unit [HRU] of the Indian Statistical Institute [ISI] clearly demonstrates the changes. Foremost amongst these was a distinctive conception of physiological mechanism of inheritance. Instead of simple frequencies of genes calculated through rather simplistic correlations of genes and blood groups, we find the HRU study speaking in a much more precise language of 'frequencies of alleles' and 'frequencies of chromosomes'. Simultaneously the blood samples were now tested for a far wider variety of antigens, viz. A, B, C, D, E, c and e, than before. Also studied were Haemoglobin variants such as HbC and HbE. This relatively more concrete but also more elaborate idiom was naturally enabled by a host of new technologies and tools that were previously unavailable. At its simplest, the larger number of antigens required larger variety of testing sera. Besides these new techniques such as Boyd and Shapleigh's method for the preparation of an Anti-H sera, Mourant's method for Rh testing and paper and Agar electrophoresis for studying Haemoglobin variations were also deployed.¹¹⁴ Taken together these new techniques operationalised a new and narrower object of somatic inheritance that was to be studied.

¹¹⁰ *Ibid.*, pp. 440-41.

¹¹¹ Majumdar & Karve, *Race Problems in Asia*, pp. 5-6.

¹¹² *Ibid.*, pp. 5-6.

¹¹³ *Ibid.*, p. 1.

¹¹⁴ Chaudhuri, 'Study of Blood Groups, ABH Secretors, and Haemoglobin Variants', p. 129.

The more elaborate nature of the laboratory work also forced the scientists more firmly back into the confined spaces of the laboratory. The earlier work of scholars like DN Majumdar had often blurred the boundaries between 'laboratory' and 'field'. After 1960 the boundaries between 'field' and 'laboratory' became much more impervious. Two inter-related practical re-orientations were responsible for this. First, the earlier researchers had either directly collected the blood samples themselves or closely overseen their collection. Thus Macfarlane, for instance, when writing of her studies of Bagdi blood groups mentioned that, 'Last week Mr SS Sarkar [her collaborator] and I visited the villages where he had previously obtained Bagdi bloods'.¹¹⁵ Majumdar went further. In his early study of blood groups in the United Provinces, he gave detailed descriptions of what kind of collection equipment had been given to those tasked with the collection of blood samples and commented on the many exigencies of the collection process itself. Majumdar took care to describe the collection equipment made up of things like blood guns, test tubes, bandages, microscopic slides, flasks, Angle Centrifuges and much else in accurate detail specifying models, sizes etc. He also spoke of practical difficulties, such as the unwillingness of all but the Tharus and Bhils to be 'pricked' for a blood sample.¹¹⁶ All this clearly established Majumdar's own presence, supervision and even involvement in the collection process. As a result, the testing was often done *in situ* and Majumdar writes at length of the difficulties of reproducing perfect laboratory conditions *in toto*.¹¹⁷ Later, during the Bengal survey once again we hear of Majumdar's troubles in the field where securing a fresh supply of testing sera proved difficult. Finally requiring Mahalanobis to set up an elaborate supply system whereby a messenger would personally carry fresh blood sera from the School of Tropical Medicine in Calcutta out to Majumdar on every Saturday.¹¹⁸ Despite these problems, one of the things that forced researchers like Majumdar to remain on the field was the difficulties in storing the blood samples for too long. The samples Majumdar used could usually only be stored for two hours in an Ice Box.¹¹⁹

By 1960 these characteristics of Indian sero-anthropology had changed dramatically. Studies such as the HRU study of 1962-63 said absolutely nothing about the actual collection of blood. Neither collection equipment nor collection practicalities were mentioned. The silence was not accidental. The new research was based on blood collected from hospital patients, schools and fellow employees of the institute. The researchers had spent no time embedded in the social context from which the blood had been extracted. This was a crucial difference. As Henrika Kuklick and Robert Kohler point out, 'the field is qualitatively different from the closed and controlled workspace of the laboratory and ... scientific work in the field is unmistakably informed by the cultural conventions of ordinary conduct'. Continuing further, Kuklick and Kohler clarify that, 'Certainly, class identities also shape human relations in the laboratory. But the greater social diversity in the scientific life in the field makes such relations there especially fruitful of study'.¹²⁰ The re-orientation of blood group studies away from the field and

¹¹⁵ Macfarlane, Un-titled Letter to the Editor on Bagdi Blood, p. 284.

¹¹⁶ Majumdar & Kishen, 'Blood Group Distribution', p. 9.

¹¹⁷ *Ibid.*, p. 10.

¹¹⁸ Majumdar et al, 'Bengal Anthropometric Survey', p. 211.

¹¹⁹ Majumdar & Kishen, 'Blood Group Distribution', p. 10.

¹²⁰ Kuklick & Kohler, 'Introduction', *Osiris*, p. 2.

into the confined spaces of hospital wards, schools and most commonly academic laboratories, dramatically reduced the immediate social diversity within which these scientific enquiries were pursued and as a result might have helped generate the misleading belief that socially discrete or endogamous 'populations' could be adequately identified by cursory superficial enquiries. Studies such as the HRU study of the early 1960s and the numerous ones that have followed since then have progressively jettisoned the tradition of actively enquiring into the actual socialities from which blood is extracted (such as was done by Sanghvi and others). Instead these studies assume socialities to be stable, fixed and given and identity being exclusively engendered in material reality of the blood. As the researchers retreated from the field into the deeper recesses of the laboratory, they lost sight of serosociality as an object of study and as the locus of truth about identities. In its place emerged narrowly sanguinary identities.

The retreat was not unique to India. There was a wider, international trend that focussed the scientific gaze more narrowly, but also deeply, at blood and blood alone at this period. Even though the actual analytical possibilities for the molecular investigation of blood remained limited to a small number of meaningful markers in the mid 1960s and 1970s, the period witnessed a decided shift. Joanna Radin describes this shift as one moving 'from a focus on exteriors, the identification of differences that could be seen unaided, to interiors, differences detected through novel molecular technologies of representation'.¹²¹ Radin also points out that one of the crucial factors aiding this shift was the development of freezing technology that allowed blood to be stored and transported.¹²²

Technological changes alone, however, cannot explain why the scientists retreated from the field into the laboratories. The reasons for that retreat deserve further exploration. Its consequences though were clear. Serosociality was erased and the serosocial identities located in it were replaced by narrower sanguinary identities. These new sanguinary identities were no longer inextricably embedded in hybrid milieus engendered by the thorough interpolation of serology and sociology. Instead, they were exclusively haematophilic. The truth about these identities was locked exclusively in the substantive reality of cold blood samples. The sociality of living blood was redundant to the new sanguinary identities.

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¹²¹ Radin, 'Life on Ice', p. 4.

¹²² *Ibid.*, see particularly chapter 3.

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