GETTING THE QUESTION RIGHT: INTERDISCIPLINARY EXPLORATIONS AT MAKERERE UNIVERSITY

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Contents

Acknowledgements  IX

Introduction
Mahmood Mamdani, Makerere Institute of Social Research  3

Part I: Gender in the Public Sphere

1. Critique and Feminist Achievements in International Criminal Law
Janet Halley, Harvard University  33

2. Disciplining Theories, Indisciplined Worlds: Doing Research in the Global South
Nivedita Menon, Jawaharlal Nehru University  51

3. Research on Muslim Women: Impelled by the World
Lila Abu-Lughod, Columbia University  75

4. Journey through Academe: Pathways to a New Definition of Gender
Oyeronke Oyewumi, Stony Brook University  97

Part II: History

Peter Gran, Temple University  113

6. Knowledge about the Peoples of North Africa: Contemporary Debates
Fatima Harrak, University Mohammed V – Rabat  131
The actual history of criticising Hegel and Eurocentrism in a Middle Eastern context—i.e., criticising Hegel’s theory of the Rise of the West and the stagnation of the East—doubtless began in Egypt in 1798, before Hegel put pen to paper, when Napoleon invaded the country and when a popular resistance led by ʿUmar Makram evolved after the defeat of the Mamluks. Throughout the nineteenth century as the Egyptian state was increasingly trapped in the orbit of the West and forced to reform, various Azhar Shaykhs and Sufis opposed this reform. Sometimes there were revolts.

This opposition to reform had different dimensions, one of which was most certainly opposition to the project of turning Egypt into an exploitable Orient for international business interests. With the revolt of Ahmad ʿUrabi (1879-1881), it became clear even to a number of English that the struggle for Egyptian independence was a natural and just cause and they began in their own way to question Hegel’s assumptions of East and West. Individuals such as Wilfrid Scawen Blunt distinguished themselves in their analysis of the real motives of British colonialism. In the years of the British Occupation which followed, the Egyptian national movement reformed itself under the leadership of Saʿd Zaghlul, shaking off the defeat of the ʿUrabi years. Matters came to a head in the 1919 Revolution, in which the Egyptian struggle forced the British
Getting the Question Right

to make political concessions. The most important of these was the 1923 Constitution, which recognised Egyptian independence. Increasingly, events such as these stirred European intellectuals; many came to sympathise with the anti-colonial struggle. A few began to write on these matters.

At the same time, by the 1930s, one finds a deepening orientalising trend within Egypt itself. Leading intellectuals like Taha Husayn were coming to accept Hegel in what one contemporary writer has been calling a wave of self-orientalisation. What these intellectuals did was fit Egypt into Hegel's model, doing so in a somewhat less demeaning way than did Hegel himself. Recall that Hegel took the Egyptian gods as part animal, meaning part African and part divine; when their creativity was burned out, the spirit went on to Greece and Israel, which represented pure reason and pure spirit respectively. What Taha Hussein postulated was that the ancient spirituality of Mediterranean civilisations lived on and stood as a counterweight to that of the modern West.

Given the continuing British presence in Egypt up to 1956, and then given the other threats to the country's sovereignty thereafter because of Suez and Palestine, Egyptian intellectuals—liberals and leftists—remained torn between the way in which the Hegelian paradigm was a force of oppression and the way in which it valorised civilisation, nationalism, and a place for Egypt in the modern world. These contradictions continue up to the present.

What I encountered in 1968 in Egypt was demoralisation following the defeat in the 1967 war and a feeling that internal social analysis, i.e. the anti-Hegel trend, had run its course, and that more attention had to be given to the game of nations and to the play of forces affecting East and West—in other words the Hegelian model and not some critique thereof.

At the time, I was a student working on a dissertation. Much of my time was spent thinking about the Vietnam War. It seemed to me that in the United States, our rather Eurocentric model of history coming down from Hegel aided, abetted, and rationalised imperialist wars of the Vietnam type. I wanted to try this assumption out in my dissertation in Egypt in relation to the Napoleonic invasion of that
Criticising Hegel and Eurocentrism

country and its subsequent justifications.

However, the academic environment I encountered was not so supportive. A very broad spectrum of Egyptian professors took the view that the Ottomans had held the Egyptians back, that the French, whatever their motives, had liberated the Egyptians from this oppression in 1798, and that this amounted to some kind of rupture after which one saw signs of revival and renaissance. It was in this way that Hegel entered into the core of Egyptian historiography with its claim about the coming of the West. What was also apparent, and what made sense given the assumptions about the so-called 1798 watershed, was that the Ottoman period in Egyptian history (1517-1798) would remain poorly studied. Despite considerable progress over the past thirty years, that remains the situation to this day. As a result, even now, it is difficult to find the argument that Egypt's early modern history matters to its modern history, which it surely does.

My dissertation subject was the biography of an individual, Shaykh Hasan Al-'Attar, whose adult life straddled the two periods—that is, the period before 1798 and the period after. Al-'Attar was known, although not well-known, for his role as a reformer in the circles of Muhammad 'Ali (i.e., in the period after 1798).

What I found, given the questions that I was asking, was that while indeed he was a reformer after 1798, he had also been active before 1798. In that earlier period, he had been part of a cultural flourishing in the second half of the eighteenth century, a cultural flourishing which I found to have been virtually ignored in modern scholarship despite the fact that information about it is available in the main source for the study of the period, the chronicle of 'Abd al-Rahman al-Jabarti. What I argued in my dissertation was that the origins of modern secular culture in Egypt could be traced back to this eighteenth century cultural flourishing, that it led directly to what took place later. My protagonist was a link between the two periods. In other words, I rejected one of the central parts of Hegel, the coming of the West.

I entitled my dissertation the "Indigenous Roots of Modern Secular Culture in Egypt" and after graduation submitted it with that title to a Middle East monograph series in 1974. The series editor liked the manuscript but refused to publish it unless I changed my title to
Getting the Question Right

a more saleable one—that is, a more orientalist title. Thus emerged *The Islamic Roots of Capitalism, Egypt 1760-1840* (1979). From that experience, what I learned was that just because there was a lot of criticism of orientalism in the air, as there was in the 1970s, it did not mean that a university press would touch it.

After the book was published, what I then remember was a sense of frustration that it was so frequently attacked by reviewers and so disingenuously, almost never in terms of its claims but almost always in terms of my choices of spelling or the meaning of words, and this persists to the present day, some thirty-two years later, even though, as I pointed out in the second edition, the spelling corrections and corrections of word meanings proposed by reviewers were themselves often not correct and sometimes they were obviously incorrect. Now all this is of course much clearer, but then it wasn't. Fortunately, I also received a few quite interesting observations and these I'll discuss shortly. At the time, however, I simply did not know what to make of the insinuation that if I did not agree with a particular meaning for certain words, my work could be dismissed as incompetent. It was only later I understood that the relevant point was that I drew the wrong conclusions from my material and this disrupted the coming of the West to the sleeping orient paradigm idea. This was taboo then and more or less still is. That was all that was going on.

The publication of my dissertation not only led to this curious set of reactions, which I have been outlining, but had various other consequences as well. When the book came out I was declared in absentia persona non grata in 1980 in Egypt—in other words, an Egyptian security risk. I was thereafter denied entrance. I learned this through a phone call from a friend in Cairo. I only got back in 1990 after many diverse appeals, the last of which was undertaken by a Republican senator from my home state.

There were other consequences from publishing the *Islamic Roots*, as well. I was hired, but not renewed, for two college teaching jobs after my Ph.D. in 1974. The first was at UCLA and the second at the University of Texas at Austin. Important personalities on the scene at the time told me informally how disappointed or angry they were with my book. Fortunately, I found a job in a branch of a state system
Criticising Hegel and Eurocentrism


These details shed some light on why I have the opinions I do about the importance of Hegel and Eurocentrism, and why I believe this set of ideas controls both liberalism and the left. Various trends come and go but Hegel seems to stay. What this means in practice is that the more one goes up the social structure in academia, and the more that money is involved, the more closely the Hegelian paradigm is guarded, so the safer place to be if one can not function critically and creatively inside this paradigm is a moderately good second or third tier university, not an elite school. At the same time, as I also came to realise, free speech even there is not free; one pays for it in a myriad of ways and sometimes free speech is even basically illusory, perhaps no matter what kind of institution one is in. Sometimes elite institutions, I came to realise, have their niches for critical thought, so a critic can make the elite listen as Edward Said did and as Professor Mamdani has been doing more recently.

While *Islamic Roots of Capitalism* was often attacked by Orientalists along philological lines, as I mentioned, it was also criticised by a few historians on a much more substantive basis. Some of their criticisms were quite insightful, and two in particular influenced the direction I would then take. An Egyptian history student asked me shortly after my book appeared why I had privileged Egypt, why other countries did not also have indigenous origins of their own modernity? I had no response at the time, but I realised I needed one. A second criticism came more slowly, although it, too, is important and I have yet to reply. It came from the Ottoman Seminar in Egypt, a group of grad students in Cairo, many who work on the seventeenth century field. It took the form of a query as to how it was that the eighteenth century was so much more important for modernity than the seventeenth century?

To address the criticism about *Islamic Roots* privileging Egypt—and this was a criticism I felt I had to address—I needed to somehow revamp. I needed to show that Egypt was simply one of many old countries progressing out of mercantilism and into the age of the capitalist nation state and modernity. I thus needed a historical sociological model of roads taken by the old states, be they colonised
Getting the Question Right

states or free states, as they progressed to modern times. Egypt would fit somewhere in such a model. The only book I had ever read that dealt with anything like that was Barrington Moore's *Lord and Peasant* (1966), a classic if rather Eurocentric work from my student days. Moore covered a lot of ground but he failed to consider states formed from groups (i.e., from tribes, clans, ethnicities, etc.)—and there are many such states in the modern world—nor did he deal with the Third World.

A work similar to that of Moore's came out in the 1980s. It, too, argued that modern capitalism had only three forms of hegemony, and it, too, omitted tribal-ethnic states; this was the book by Giovanni Arrighi.

From roughly 1980 to 1996, I worked on a book which I entitled *Beyond Eurocentrism: A New View of Modern World History* (1996), which would show that there were four different roads to the modern world, not three—that is, four different forms of stratification, with rulers in each playing the oppressed off against each other and thereby deflecting class conflict. I tried to give examples showing that each of these four could at times be quite dictatorial or quite respectful of human liberties, that in other words, countries were not free or unfree because of their choice of hegemony.

I was interested, as well, in why these hegemonies were successful in the main, given that they were so exploitative. I wound up arguing that persuasion played a big role in the success of these hegemonies and not simply coercion, that rulers tried to deflect protest onto something of secondary importance, and usually succeeded through persuasion. Whatever was primary—such as bad working conditions for the majority—could then be acknowledged, but acknowledged simply as a societal problem, one which could be understood as something that the legal system and all reform-minded people were still trying to address, all the while the lion's share of the media's attention and public outcry was focused on whatever was secondary.

Writing this book, as I did, was a way of answering the critics of my first book, who thought I had no justification for singling out Egypt. As it turned out, it was not a very good way. By 1996, critics were by and large no longer interested in different types of states, at least as I was defining them. The idea of a ruler-ruled relationship seemed almost passé. Conditions had changed. Mainstream thought
Criticising Hegel and Eurocentrism

was becoming neo-conservative, and the more critical intellectuals were reading Foucault. Where once Western intellectuals all stood for the idea of the nation-state, by 1996 this was no longer the case; the shoe was on the other foot. China had emerged and the American middle class and its intelligentsia were in denial. Some went as far as to confront this surprising set of events by taking the position that not just states but history itself was over, that everything was local or global or a bit imperial but not national. I remember telling myself at the time that this would be a passing fad. Everybody knows that capitalism's basis of political power is the nation-state. Everybody knows that the nation-state still creates the consumer through its socialisation and still creates the new generation of workers and still divides the workforces of the world against each other. The state is not going to decline; this is simply a lost generation of intellectuals. As you would know, I was wrong. The idea of "going beyond the nation-state" was no passing fad.

The one part of *Beyond Eurocentrism* that some reviewers seemed to accept was the line of thought that different kinds of hegemony require different kinds of historians. This part certainly spoke to real world concerns. One need only recall the problems of the UNESCO world history project. In Russian circles, I had written, the doyens of history in the major cities promoted an ultra secular, ultra internationalist geo-political state history. Social history that served to integrate the kind of bourgeois democracy in which I grew up was frowned upon. I was told this to my face in Iraq in the 1980s—Iraq, I found to be, an example of a Russian-type hegemony. In Italian-type circles—and this includes Egypt—the prevailing idea of history is civilisational history, as I had mentioned when referring above to Taha Hussein and the Mediterranean School of Egyptian history. Social history exists, as well, but has much less status. Finally, where states play the working class gender as in the tribal ethnic states, positivist history is completely subordinated, myth is given greater weight, as are archaeological history, popular history, and local history. Positivist history is more frequently found in the work of foreign researchers who are manageable through gnosis, according to the Zairian philosopher, V.Y. Mudimbe. This accounts for the play of orality against textuality, the use of dance as a historical source, and of course music as a text,
Getting the Question Right

my examples drawn from the case of Albania and Zaire. In Zaire, the history programme in one of the major universities even seemed like something of an afterthought; Lovanium University in Kinshasa was founded in 1954, but only created a history department in 1966. What I took away from this was that reviewers made a distinction between historians and history. Historians were interesting as individuals who wrote things, but the history of countries such as Zaire was not.

In any case, the book did not paint a very optimistic picture about the history profession or its futures. History is under attack in many countries, especially modern history, perhaps because it has the potentiality of revealing that which might be threatening, perhaps because when governments become more and more dictatorial, knowledge of modern history, almost any knowledge, is threatening. Historians realise this. Thus, one could note that as conditions have grown worse, a mutual aid organisation called the Network of Concerned Historians has emerged in the Netherlands to help historians around the world who are in trouble and it seems to be playing a very positive role.

When Beyond Eurocentrism came out in 1996, I thought I had dealt with the earlier criticism of privileging Egypt, as well as the Hegelian idea of the opposition of West and non-West by the way I structured the book. The reception once again disappointed me. These issues did not arise. The critical reactions which I received, apart from a few technical points, were very minor except for one which was very big. The big one came from an atypical reader, a reader who still took states seriously, and it very much influenced my direction from that point on for a while. What the reader noted was that I had failed to introduce some alternative meta-narrative, one which would explain why these hegemonies, which I was studying, were important—in other words, why one should consider their leading figures, assuming they were coming from the Third World, to be more than compradors, as one would do following older lines of thought? Here the metanarrative question re-emerged. What was there to put in the place of Hegel's metanarrative of the Rise of the West that would justify doing what I did? The need to spell out an alternative metanarrative led to writing the now recently published book called the Rise of the Rich: A New View of Modern World History (2008).
Criticising Hegel and Eurocentrism

The Rise of the Rich was, in effect, my third attempt to deal with Hegel. It was also my third attempt to find a readership for my project in the United States. If the first book was addressed to Orientalists, the second to the New Left Gramsci hegemony types, this third book was addressed to the new liberals and the postcolonial intellectuals, these being the most important segment of the academy by the early 2000s.

A little more of a background on liberalism in American thought would help make this point clear. In the 1970s and early 1980s, the old liberalism of the development studies stage went into crisis. After a few years, it was replaced by new liberalism and this is what I am referring to. Some have called this the “cultural turn.” Most who wrote about the cultural turn or embraced it were humanity professors, not historians. Oftentimes they wrote about it in rather glowing terms; my own views as a historian are rather more reserved. New liberalism of which the cultural turn was one expression arose at just the time when the organised working class in Western countries was at its lowest ebb; the global trade unionism which is now beginning to emerge was not yet on the horizon. During this period, most academics in Western countries, for some reason—especially humanities professors—took class struggle to be irrelevant. Most were thus rather easily pushed out of political economy into a rather elite business-oriented version of liberalism. Change would be of the trickle-down sort out of our modern risk society to the subaltern world below.

The Rise of the Rich was an attempt to find an alternative footing for history as a scientific enterprise in this environment, one that could stand up to the pressures of business and government on scholars to retain the rise of the West as a worldview, this time via the cultural turn. My opportunity to be heard I thought would come from the fact that new liberalism was radically presentist and somewhat anti-historical. Many historians found it off-putting. What historians needed, I argued in effect, was to reclaim the ground they once had occupied when history was taken to be scientific. To be once again considered scientific would be—I thought—a great plus for historians, at least in countries such as the United States. However, to be considered scientific, the field would need a dominant paradigm, one that was generally perceived as viable. This is how scientific fields operate. History had once had
such a paradigm, which had been useful for a long time. This was Hegel’s “Rise of the West.” At that time, history did well; but now that this paradigm has been outgrown, the field lacks a replacement for it and has been suffering a bit as a consequence. Today, as I noted, we see a lot of presentism and anti-historical thought in the academy as a consequence, new liberal thought being just one example that one could point to.

What *The Rise of the Rich* takes as its premise is that we should all agree that there is no longer any credible hope of reforming the Rise of the West given its no longer defensible claims that the West rose, that the Orient was static, and that there are people without history itself. I hope I am stating the obvious. Frustrations with this metanarrative are by this point widespread, going far beyond the field of history. In fields such as world history, historians characterise their attempts to confront the problems caused by Hegel as efforts at overcoming Eurocentrism, and many would agree that that is what lies at the heart of what they do.

Reflecting on such matters as I was doing in the late 1990s, one day there came to me the insight—and now I turn to the argument that I call the rise of the rich—that leading politicians and economically important groups all over the world have important linkages to each other, that they collaborate in many spheres, and that this has been the case for a long time.

Therefore, it is not surprising to find that many foreigners have better access even to Great Power ruling circles, in fact, than do the vast majority of ordinary citizens, that therefore to attribute power to such countries (à la the rise of the West) as opposed to these groups distorts the narrative of power. Historians therefore need to give priority to these political and economically dominant elements and then only secondarily to the rise of the West, or core and periphery as in the existing liberalism and political economy. I can say this insight came to me although I’m not positive how new it is and therefore what kind of claim I have on it. Certainly, there is a lot in the scholarship inspired by new liberalism in and around world history which points toward global ruling classes and global modernity which fits with the rise of the rich, at least to a limited extent. There is, as I also indicated, older material as well, material from Marx, from Gramsci, and from the literature of
Criticising Hegel and Eurocentrism

anti-colonialism.

Adopting the rise of the rich as one's metanarrative, which is what I did, led me to realise that the significant development of the past 400 years was not some war or some discovery, but rather that capitalism has served as the basis for the evolving cooperation both in times of war and peace of a relatively small group of individuals and institutions spread out across the world in different countries. The outcome which is all too visible today is what underlies the rather major disconnect between ruler and ruled found almost everywhere, what we in America sometimes sardonically call the Washington Establishment or the Beltway mentality or the imperial presidency. Adopting the rise of the rich as one's metanarrative gives us a way to integrate these observations into a coherent view of world history, one that can hopefully be of service to us in our society today. At the same time, I want to state that adopting the rise of the rich requires an effort. It forces one to rethink a good deal about matters that one would think one already understood.

For example, from thinking of capitalism as a well-understood system, as someone like myself had been doing for a long time, one starts to realise it is not that at all. The capitalism that lies behind the political disconnect I was referring to could only exist if the work done by the citizens of a given country by and large no longer mattered all that much, because if they did, there could not be this disconnect. The rulers would have a stake in staying connected. But, one wonders, how could it not matter? Perhaps it does to a degree, but what really keeps the system going must be something else, something in addition to this—let us say legitimate business, as legitimate business does not produce that level of profit and power that would allow such a disconnect. But what could it be? This led to the idea that the key to understanding capitalism must be that it is something illegal, something like plunder and that I was looking in the wrong place if I was reading business school textbooks in the library about legal capitalism. Now things began to fall into place. It was a mistake to think of accumulation as primitive accumulation, as I had been doing for years—that is, as something which came to an end, for example with the end of slavery. Primitive accumulation never came to an end; indeed, if it ever did,
capitalism itself would probably stagnate. It was even a mistake to think that slavery came to an end. And, while slavery is no doubt one of the major forms of plunder, there are other forms as well, maybe even ones that were more important. Consider the worldwide institution of the unwaged housewife. The existence of the housewife makes one think about all the ways hegemonies and state intellectuals can still force people to work for nothing. Think about all the regimes colluding with all the religious and educational structures to rationalise getting two jobs for one salary by turning housework and child-rearing into unwaged labour and by compensating two people's work with one wage, i.e., the man's. Is it woman's nature to work for nothing? Is it God's will, is it good for children, is it patriotic? Perhaps, but perhaps not. Perhaps really gender studies ought to be a part of plunder studies. But, plunder, to continue, is not just slavery and housewifery; there is also unequal exchange. Permit me to repeat a well-known anecdote to convey what I have in mind. It's about Donald Rumsfeld; if you've heard it, before please just bear with me for a moment. Donald Rumsfeld, the secretary of defence under George Bush, made a number of trips to Iraq to raise troop morale in the early 2000s. On one of his trips, he held a televised press conference in a rug market in Baghdad. Surrounded in this cameo by heavily armed U.S. troops, he held up his purchase—quote-un-quote "purchase"—telling the media, "The market is open! I got five rugs for five bucks." What is important is the first two words, "the market." What do we mean by the words "the market"? Did that rug merchant really want to part with five rugs for five dollars? Was that a market or was it simply an episode of plundering made to look like a market. This goes on across the world all day long; the common term for it is "unequal exchange."

The broader point here is that the more one realises the importance of plunder to capitalism, the more logical it seems that there would likely be designated plunder zones such as the Third World and that they would have to be a major part of the modern world economy. The idea of the rise of the rich, then, is a way to explain—and now I return to the earlier point—why a Third World ruler who manages one of these plunder zones has better access to the White House than I and my neighbours who are ordinary citizens do. He is a business partner
of our leaders, not some outsider as we are.

The rise of the rich as a metanarrative begins with the idea of the expansion of capitalism. The expansion of capitalism seems like a more accurate characterisation of sixteenth century world history than does the more familiar characterisation, that of the expansion of Europe or that of the age of exploration, or even that of the birth of capitalism, which comes along with the rise of the West. The expansion of capitalism was based from the beginning, we discover, on alliances among hegemonic elements, and these alliances were engineered by the go-betweens who forged them. So, it is scarcely an exaggeration to say that it was these go-between figures who created the modern world market and not just supply-and-demand, as is commonly assumed. While there have always been international merchants and markets throughout history as in the Indian Ocean, and while hegemonic elements have had greater or lesser interest in treaties and alliances, it was for the first time in the sixteenth and seventeenth centuries that alliances were formed on a somewhat systematic basis between remote and dissimilar ruling groups, these alliances embodying—and this is another point—some kind of idea of joint political, as well as economic, objectives. The go-betweens who made these alliances I called the new men, the reason being that they were and still are a group on the make within what has continuously been—and thanks to them—the ongoing revolution of capitalism. Who were these new men? They mainly were would-be gate crashers, not official emissaries or proxies. In each period, a few actually did succeed in entering the establishment. If one was to generalise, one could say that the new men as a group were attuned to the market as well as to political conditions and who aspired to belong to the rich to such an extent they were prepared to do its dirty work if that is what it took to get there. And, because of their diverse skill sets—as criminals, pirates, military adventurers, speculators, diplomats, businessmen, translators, professors, missionaries, and so on—these new men coming from whatever country they came from can take credit for a number of important developments, among them the organisation of the periphery of the world market—that is, what later gets called the Third World, the region which serves as capitalism's main plunder zone.

The periodisation that I prefer for modern history is one
whose first period runs from 1550 to 1850, with the second period running from 1850 to the present. The first period, 1550 to 1850, saw the expansion of capitalism and the take-off of the modern bilateral relations system created by the New Men. It carried on through the rise and fall of mercantilism, up to the age of the capitalist nation-state or the age of multilateralism, which emerged in the mid-nineteenth century. The second period, beginning in 1850, witnessed the spread of multiparty treaty relations among friend and foe alike, relations allowing each ruler, now backed by all the other rulers, to impose market relations on his society. And, as any diplomatic history book would make clear, by the mid to later nineteenth century, dozens of treaties bound the rich together, giving them a degree of autonomy in relation to their subject populations scarcely imaginable in earlier times, here to return to my point about the disconnect.

As this choice of periodisation suggests, I am agreeing with the traditional claim that capitalism arose in England but I'm questioning whether “arose in” necessarily meant “benefitted from.” Those who benefitted from the rise of modern capitalism were the economically and politically dominant groups worldwide and the new men and only to a much lesser extent the majority population of the United Kingdom and other developed countries. In trying to explain why this would be the case, it seems clear that the issue is one tied up with the capacity to meaningfully contribute to, and therefore claim, rewards. Any group that controlled a subject population and was able to force it to be a consumer or to produce for the market, or controlled a territory which had something of value, would be recognised as a group that mattered in market terms. This would include the ruling circles of even rather small countries, but it would not necessarily include individuals who simply had money, i.e., the rich in that sense of the word. Businesses tied to the international economy along with the hegemonic elements of states and empires are the two main parts of the rich. On the margin, both of the modern and contemporary periods were and are—here to repeat—the New Men.

As for historical evidence about the rise of the rich, one body of material which stood out more than others was the collection of treaties which various historians have assembled over the past generation
Criticising Hegel and Eurocentrism

or so. Take, for example, Volume 1 of the old J. C. Hurewitz’s collection of treaties of the Middle East. What was especially useful, I found, was the text of the treaties that Hurwitz included when read in its entirety. When one reads the text of some of these treaties that way, what one encounters is not just the commercial quid pro quo that appears near the beginning but the political requirements of those who signed these early treaties. These appear further on in the text. They seem to imply that the commitments undertaken in signing these treaties involved risks. I am guessing, the signing of such treaties might arouse adverse public opinion so that, to a degree, these early bilateral agreements—and this is, here to recall, what they mainly were—were an exercise in disguising their true nature. The rich, I concluded, fearing public opinion as they seemed to, were constrained to acknowledge the sensibilities of the country of which they were a part in order to keep their legitimacy, and they did so by appearing to be concerned about religious details, gender relations, and the like, and not simply corn, wheat, and tobacco. As for the treaties of the contemporary period, by way of contrast to those of the early modern period, what is observable is how many signatories and treaties there now are, the result being that the national elites have become tied—together and therein lies the security of today’s market.

In a theoretical sense, the rich thus have a double identity. They are a class in themselves, but not for themselves, a class economically and socially linked, but politically separated by the constraints imposed by their affiliation to the different nation-states. At the same time, the rich are not just a class, but a caste or stratum in the Weberian sense. Their double identity is thus one of class plus caste. If we expanded this discussion we could also show that the rich are racialised as white and genderised as male.

To conclude, it is doubtless too early to gauge the reaction to this book or what criticisms it will elicit. Since publishing it, I have begun to try to apply it to my idea of Egypt as “Italian Road” (North over South). Unless I get derailed again by more fundamental criticisms, what I hope to do is to show a specially audience that one can write the modern history of Egypt without Hegel. If one can, it seems to me the Rise of the Rich would be shown to be useful. So far, what I am noticing is several figures, such as Nubar Pasha in the nineteenth century and
'Adli Yaqan in the twentieth, seem not just to have some international connections, but seem to be bridge figures between Egypt and Europe in some sort of transformative way. They possibly fit the criteria I laid out for the rich. I plan to read more about them.
Criticising Hegel and Eurocentrism

