

BACK TO PETER GRAN AND THE ISLAMIC ROOTS OF CAPITALISM: REFLECTIONS ON THE PERIODIZATION AND HISTORIOGRAPHY OF MIDDLE EAST STUDIES

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In one form or another, the problem of Napoleon and his role in the modern development of the Middle East tends to resurface in academic publications. Naturally, the core of the issue is not the French occupation as such but rather the paradigmatic differences which have found their manifestation in this historical incident and its interpretations. It is not my purpose to write a comprehensive analysis of the paradigmatic situation in Middle East Studies, nor is there any reason to. This has recently been done by Dror Ze'evi (2004) in his meritorious article "Back to Napoleon? Thoughts on the beginning of the modern era in the Middle East". The present article can be considered as a minor addition, or an extended marginal note to his presentation.

In his article Ze'evi surveys the theoretical suppositions in Middle East studies, applying the well-known distinction between the older paradigm often labelled as "Orientalist" and the new one, the so called "revisionist". The struggle between these rivaling approaches has been personified by Napoleon and his military expedition of 1798. According to the traditional view, the late Ottoman era of Egypt was mostly a period of cultural stagnation and political isolation. All central innovations and institutions characterizing modernity, like the conscript army and secular education, were automatically taken as purely Western in origin and thus borrowed mechanically by the Middle East, without having any connections whatsoever to the native social and economical structures. The French expedition was not just an important historical incident but an essential prerequisite to the modern development.

This paradigm has been defined by many terms, "Orientalist", "colonialist", "conservative", etc. In my opinion, there is no need to present the weaknesses of this approach here, because in view of current anthropology and sociology they are quite obvious. The paradigm which the pre-war generation took pretty much

for granted has been a subject of considerable criticism since the 1960s, and in the 1970s Edward Said brought the issue of the underlying values and presumptions of scholarly studies to the core of academic discussion. It can be stated that his epoch-making work *Orientalism* was the turning point after which these questions could not be ignored or trivialized. For a wider public the book is the best known example of this new scholarly tradition, which consciously and explicitly challenges the classical Orientalist tradition. However, Said was never alone in his mission. There were many others launching attacks against this staggering, but still reigning, paradigm from various starting points, among them many distinguished writers like Amira Sonbol, Samir Amin, Maxime Rodinson and Peter Gran.

Whereas the orientalist option saw in the native and economic background nothing more than a passive receiver of European modernity, the new approach emphasized indigenous developments to the degree where the European influence was almost omitted entirely from the analysis. At most, it was deemed to be a marginal factor in the passage to modernity. This logic is described by Ze'evi:

If we assume that this is a new phenomenon beginning in the seventeenth and eighteenth centuries and gaining power in the nineteenth, we may then claim that we are looking at a historical development that anticipates modernity: a new social class, claiming political power, amassing great wealth and knowledge, and mediating between state and society. Are these not the local roots of capitalism? Is this not the budding power behind nationalism, pluralism, devolution of power and all other signifiers of modernity?¹

From the revisionist point of view, the significance of Napoleon and his army has been grossly exaggerated. In the formation of modernity it was a minor factor compared to the indigenous developments which were totally overshadowed in historiography by this "coming of the West". According to this stance the periodization built on Napoleon's expedition reflects heavily biased presumptions of the colonialist historiography and thus there is no actual reason to draw the line at the year 1798. The inevitable outcome was a clearly articulated need for a new periodization, free from the burden of colonial mentality, and this became a burning issue in the field of Middle East studies. However, the task turned out to be a complicated one.

In his article Ze'evi notes that, in retrospect, a new periodization has failed to materialize. In his view, this is not to be taken as an indication of the enduring orientalist paradigm. Rather the whole question of the agent of modernity has lost its significance and it is time to move on from this dichotomic thinking. Many features of modernity, especially colonialism, were much more complicated phenomena than previously realized by either the conservatives or revisionists. Ze'evi

¹ Ze'evi 2004: 88.

suggests a new approach, a third option in Middle East studies, and delineates some new starting points for a fresh historiography. In this article my purpose is to look back on the venture of the revisionist paradigm and point out one possible reason why a clear new periodization has turned out to be such a difficult task.

It is often stated that these two above-mentioned paradigms still dominate the academic field. While there certainly are those who understand this categorically, I personally would like to believe that it was more true in the 1970s than at present. In most cases the works written back in those days can be placed in these two categories without any major effort. After the 1970s the task becomes much more complicated. An important turning point in the paradigmatic dichotomy was the celebrated monograph *Egypt in the Reign of Muhammad Ali* by Afaf Lutfi al-Sayyid Marsot (1984). The book was an undeniable disengagement from the Orientalist approach. In her interpretation, Napoleon's invasion did not herald the arrival of the new era nor did it mean a drastic break in the country's past. The whole event is more like a historical fact which both interrupted some internal developments and gave a new impetus to others. However, it is hard to see the book as a direct offspring of the revisionist school either. The external influence is not excluded from the analysis but considered as a quite natural factor in the history of Egypt. There is no "arrival of the West" but there is an international dimension in the history of Egypt, and it had strong economic, political and cultural implications in the development of Egyptian modernity. For some representatives of the revisionist stance, Marsot's work was slightly heterodoxical because the West still had a role to play in her analysis. In retrospect, it can be seen that the impartiality of her analysis regarding the source of modernity indicated that the revisionist paradigm had reached the zenith.

As Ze'evi points out, the rigid revisionist approach had its shortcomings as well. The scholars of the new paradigm were eager to find out what had been ignored and pushed aside by the old paradigm, and undeniably they found a totally new pattern of historical dynamics. The problem was that in doing so they pretty much turned a blind eye to all external influence. So the analysis tended still to be quite one-sided, although this time in the other direction. The features they found and pointed out were real but they were not the whole picture. Central to the analysis was still the agent of change but now it was "the East" instead of "the West".

It is quite obvious that in the 1980s the new paradigm, in its most uncompromising form, started to have grave difficulties with its own presumptions. It simply could not tackle the complexity of modernity. The problem was not so much who was the agent of modernization but the centrality of this agent itself in the historical narrative. When the revisionists concentrated on this issue they did not much problematize the nature of modernity itself. The pattern was usually

defined according to the Western model though seldom articulated clearly as such. Occasionally, as in Peter Gran's *Islamic Roots of Capitalism*, the main subject of this study, modernity is almost analogical to capitalism. The indicators were mostly cultural features like nationalism or positivism, or social and governmental institutions such as the conscript army and state bureaucracy. The outcome was a concept of modernity which was drawn according to the existing Western model but which was assumed to develop independent from it. The revisionist thus ended up adopting a highly mechanical evolutionary thinking where the laws of nature inevitably guide development on the path to modernity. To quote Ze'evi again:

If anything, this new revisionist narrative conforms even more closely to an idea of time that is imbued with a power of evolution: the world evolves in one direction, and any process of change is bound to take us there. Granted, it happened sooner in Europe, but the process could and did repeat itself in the Middle East.²

Undoubtly, one of the most influential and outspoken representatives of the revisionist paradigm is Peter Gran, and perhaps the most pronounced study of this stance is his *Islamic Roots of Capitalism*, the first edition of which was issued in 1979. The book is not only a representative of this approach but also its most ardent champion. This mission is made very clear in the introduction of the book. The author is very specific in his thesis that modern Egypt had indigenous roots and in this development the Western influence was secondary at best. Following the revisionist approach Gran argues that the modern features which became visible in the reign of Muḥammad ʿAlī, and after, were not borrowed from Europe but were based on social and cultural changes which took place in the previous century. But for Gran the issue is not just a theoretical one. He makes a considerable effort to show this development in his sources, that is to say, in concrete historical events and mentalities. The book contains the whole social and cultural history of Egypt from 1760 to 1840 and even material from a wider context of the Ottoman Empire.

Gran has always taken great care to emphasize that he is not the only representative of the revisionist paradigm, nor has he ever claimed any kind of intellectual leadership within the revisionist movement – if we can even use this term for a large group of scholars with a wide range of differing theoretical viewpoints and methodologies. However, there is no doubt that *Islamic Roots of Capitalism* is the most comprehensive work of revisionist historiography and it could be claimed that the book is of specific importance considering the current academic discussion of the periodization and the origins of modernity in the Middle East.

² Ze'evi 2004: 85.

The first reason for this is that *Islamic Roots* was reissued quite recently in 1998. This so-called Syracuse edition includes a new and quite voluminous introduction, where Gran himself contemplates his position in the current academic field.

Secondly, upon its publication the book was – and still is – the only comprehensive presentation of its kind about an indigenous modern development actually taking place without the Western influence personified in Napoleon. This is the feature which takes the book far beyond anything else written about the passage to modernity. It is not just an alternative paradigm, it is an alternative narrative.

Regarding the current academic discussion, the most important factor is that the issue of periodization is very central in Gran's work. While most theoreticians content themselves with criticizing the conservative Napoleon option, Gran comes up with a clear new alternative as a watershed in historiography. He suggests that the year 1760 is a much more convenient date as the starting point of modernity because most features characterizing later Egyptian history have their origins in the great transition of the economic and social structure during the second half of the eighteenth century. To quote Gran:

The year 1760 marks the date when struggle over distribution of the surplus began to result in a transformation of production. It marks the beginning of a long process in which the commercial sector was wrested from indigenous hands, a process in which other subordinate social formations, slave and tribal, likewise give way as capitalism developed.³

Upon its publication in 1979 the book was a minor academic sensation and triggered a very animated discussion, which has continued up to this day. Gran was exposed to hard criticism but he also found understanding among the international scholarly community. The most outspoken critics were Fred De Jong and the late Gabriel Baer.⁴ Against this critique Gran was determined to defend his views. The Arabic translation appeared in 1992 and opened the book for a wider public. It was met with an enthusiastic reception particularly in Egyptian leftist circles, though Gran himself has some reservations with their interpretations.

As previously stated, the core of the theory is that most modern features in Egypt have indigenous roots which date back to the pre-Napoleon era. In order to form a coherent narrative, Gran delineates an era which he terms as “merchant capital”. This period covers the years roughly between 1760–90 and it forms the basis of the social, cultural and economic developments taking place in the following decades. According to Gran, the impetus behind modernity was not the French military conquest but an economic transition when the Egyptian trade re-

³ Gran 1979: 11.

⁴ Baer 1982; De Jong & Gran 1982; De Jong 1983.

oriented itself from being internal and Ottoman to external and global. The economic transition caused changes in other clusters of the society, affecting the mode of production, which in turn affected the religious framework of the era and at the end, profoundly transformed the cultural features in a direction resembling capitalism.

In Gran's theory, the impetus for the transition came from France, but this happened two generations before Napoleon. The context of the world market was the major factor, compared to which the French occupation was of marginal importance. The actual causative event was the Seven Years' War between England and France, which devastated the French overseas empire. After losing the colonies in America and India, France was forced to look for new markets in the Mediterranean region. At the same time took place "the rise of the Mamlūk Beylicate, a semi-independent configuration of warriors with a strong commercial orientation, which marks the decline of the older Ottoman military caste system."⁵ Gran incorporates these two events in his theory, thus forming the roots of the dynamics which were to lead to modernity. The starting point in this development is approximately the year 1760 which he thus sets forth as a new candidate as watershed in Egyptian history.

Of course, the whole narrative in *Islamic Roots of Capitalism* is much more complicated and detailed than can be summarized here, but the logic of the narrative is that the context of the world market opened up new economic opportunities for certain social classes who were in the position to take advantage of the new situation. These were mainly the ruling echelons of society, namely the commercial elite (*tujjār*) which were in possession of the necessary skills and connections required by this new economic system. Another group was the *Mamlūk* military elite which controlled the agricultural sector. The third group was the upper stratum of 'ulamā', whose role in the production of the nascent modern culture is very central to Gran's theory.

Three phases can be distinguished in the theory. At the first phase there was an economic revival brought about by the potential of the world market. This new prosperity affected the religious framework of the time, where it took the form of a *Šūfī* revival. The *Šūfī majlis* literary salons were the cultural institution where the new secular culture was produced. According to Gran's thesis, this new culture was supportive of capitalism. The pre-Napoleon capitalistic development was thus mainly a juridical and cultural reflection of economic well-being, which created its own cultural framework. This was a new body of thought or an ideology, where Gran sees features like utilitarianism and individualism, which were not borrowed from the West but which were an outcome of the internal

⁵ Gran 1979: 3–4.

dynamics. Here we have in detail a presentation of the development which led to the birth of a new kind of social and economic reality. This was a radical, new theory of the local roots of capitalism – but was it tenable?

Obviously the academic public was not convinced. The outspoken criticism is perhaps less revealing than the infrequency of references to the book found in scholarly works. Around *Islamic Roots of Capitalism* hovers something like an awkward silence. When it is quoted, the main thesis is usually left outside and only some minor details are taken.

Gran is well aware of the fact that his theory is not widely accepted, nor does he see any paradigmatic revolution in the offing though he would certainly welcome one.

The persistence of 1798 as an assumed watershed is something of an exception to any general rule. It has managed to survive for a long time even in the face of considerable criticism. The question one needs to ask is – why is this the case?⁶

I believe it is time to take seriously this question posed by Gran himself. In his opinion the reasons that the book was poorly received lies in cultural and ideological factors. He wrote his other main work *Beyond Eurocentrism* (Syracuse University Press, 1996) about these very issues. I would like to emphasize that in my opinion this book contains many sharp observations concerning Western self-understanding and relationship between the west and other cultures. It is not the purpose of this article to claim that these factors were of no significance. However, there are other reasons as well, more technical in nature, and only those are the subject of this study. I am convinced that these can be studied unrelated to more theoretical paradigmatic questions.

In the new Syracuse edition, Gran has added a separate section entitled “A note on critical reception” to counter the criticism. However, taking into account the extensivity of the criticism in the 1980s this chapter is quite brief and incomplete. Gran is a bit selective in what he answers and what he does not. In addition, he tends to take purely technical and methodological issues as paradigmatic questions. This is a great pity because many open questions still go unanswered.

After all, there is no escaping the fact that when issued in 1979 *Islamic Roots of Capitalism* was a very unpolished work, almost half finished. As one of the main critics of the theory, the late Gabriel Baer noted: “To conclude this review on Gran’s book, we cannot refrain from noting that it also abounds in mistakes of all kinds.”⁷ This is regrettably true. Both Baer and De Jong listed examples at great length. Some of these mistakes are solely due to negligent proof-reading. There are errors in transliteration, names, persons and dates, some of which are

⁶ Gran 1998: xi

⁷ Baer 1982: 221

possibly just simple misprints. It appears that the book was published in a great hurry because an elaborate revising would have eliminated most of these totally unnecessary mistakes. Why was this not done is a puzzling question since the author must have been well aware that the book would meet with opposition.

It is evident that the general carelessness has serious implications for the soundness of *Islamic Roots* as a scholarly work, and to some extent put the credibility of the whole theory under question. Graver still, this feature is not limited to a large number of factual errors but it is as obvious in the presentation of Gran's theses and standards applied in the methodology. The handling of source material in particular, both primary and secondary, was clearly carried out in a haphazard manner. Obscurities in his references are found regularly, lastly pointed out by Khaled el-Rouayheb (2005: 18, quotation 53).

Here again, careful editing would have eliminated these obscurities, as well as clarified the terminology of the book. In its current form *Islamic Roots of Capitalism* has been compiled from various sources which have only been partly incorporated in the general narrative. Occasionally it is hard to comprehend how the issues dealt with in the text are related to the conclusions or even to the title of the chapter. To take one example:

The incoherence of the book is particularly apparent in the passage which is most central regarding his theory, i.e. the question of capitalization. It is quite peculiar that he has entitled the first chapter of his book "The social and economic history of Egypt, 1760–1815: A study in merchant capital and its transformation", but in the text the term "merchant capital" is not mentioned at all. On the whole, the chapter just contains a selective synthesis of scholarly works of the 60s and 70s. No new data is added. He delineates the general features of the Egyptian economy at the time, without any apparent connection to a capitalist development. The substance of his economic analysis is pretty much that certain echelons of the society were gaining wealth but this is hardly under dispute.

To get to the matter of issue Gran should point out how the period between 1760 and 1790 was more "capitalistic" than all decades before it. This is nowhere explicitly stated. Upon dealing with the rise of the *Mamlūks* as the dominant class of society Gran writes: "Their wealth rose steadily as the value of the land rose."⁸ On the next page, this "wealth" has suddenly turned to "capital": "Not only did Mamlūk capital penetrate the Ottoman financial administration, thereby altering the way in which it functioned."⁹ It is quite obvious, that Gran does not show the formation of capital as an economic phenomenon or cultural concept, he simply renames the traditional wealth.

⁸ Gran 1979: 12.

⁹ Gran 1979: 13.

Surely, this was not Gran's purpose in the chapter in question but an outcome of an inadequate editing. Nevertheless, this is a serious defect. In an analysis related to capitalism, great care ought to be taken not to confuse capital and wealth. As shown by Raymond and others, there has always been a wealthy merchant elite in Egypt. In the Middle Ages these were the *kārimī* spice traders and later coffee was the main source of profit. Gran does not deny these facts; he simply leaves without clarification why wealth in the seventeenth century turned to capital in the eighteenth. He notes only that the *Mamlūks* were far wealthier than their predecessors but even this is a problematic statement. Of course they had enormous income but their expenses were even greater, and they were in constant need of cash. Gran himself admits that there were "economic strains".¹⁰ Clearly, the *Mamlūks* did not live within their means but spent their income in unproductive ways, mainly on luxury items and weapons which they bought from Europe. This can be seen in the Egyptian balance of trade which turned increasingly deficitary towards the end of the century.¹¹ It appears that wealth did not accumulate in Egypt to facilitate capitalization but flowed abroad with increasing speed. This is not a very promising basis for a capitalist development.

One wonders whether the apparent lack of the finishing touch is a reason for this certain incoherence in Gran's presentation and whether the same goes for the conciseness of the book. In her recent presentation of medieval Islamic political thought, Patricia Crone needed four hundred pages and she did not have a single redundant word. The development of modernity during the timespan of eighty years is hardly a lighter theme but definitely more controversial. To put it in the scale, Maxime Rodinson used three hundred pages in his *Islam and Capitalism* and still the work is quite preliminary, though systematic and analytical. In the case of Gran's work, conclusions are coming at a breathtaking rate but the reasons for these do not always come at the same pace. These arguments are often based on activities and writings of some individual or individuals, which are used to highlight more general social and economic trends. However, in most cases the examples given are quite far from convincing and even more so when he studies some case in greater length. Muḥammad Bey Abū al-Dhahab is a case in point.

One of the quite controversial theses which Gran presents in his book is an evolution in the ethos of the military class which changed it into something resembling a capitalist class. He tries to demonstrate this in the life of an individual,

¹⁰ Gran 1979: 17.

¹¹ An excellent summary of Egyptian economic and social history is included in al-Sayyid Marsot's *Egypt in the Reign of Muhammad Ali* (Marsot 1984: 1–23). Both Gran and Marsot derive their information from the same source, various writings of André Raymond, but draw totally different conclusions. Raymond's own opinion, however, is clearly supportive to Marsot's view (e.g., Raymond 1973–74: 98–100, 105).

Muḥammad Bey Abū al-Dhahab, the last sovereign *shaykh al-balad* before the devastating civil war of the late 1770s.

One figure, Muḥammad Bey Abū Dhahab, more than any other, came to symbolize the new way of life. He demanded of the French merchants on one occasion that they construct for him at their expense a four-wheeled gold carriage at an estimated cost of 7,200 French livres before shipping. Such was the profitability of the French position in Egypt that they proceeded to have the carriage made.¹²

This is simply an untenable argument. The lavish lifestyle was not “the new way of life” but the traditional one. Again, he is showing that the revenues of the *Mamlūk* households (*bayts*) were increasing, because they ousted the *ojak*-regiments from the dominant positions in Egypt, but this does not signify a commercial orientation or economic rationality to any extent, and it is hard to see how the extortion of the merchants is related to the capitalist development that Gran is striving to show. Muḥammad Bey Abū al-Dhahab was milking the merchant community just as he milked his *iltizām*-lands and during the reign of his successors this extortion grew even more severe, and in the end the profitability of the trade was gone. By the 1790s the French merchant community, once flourishing in Egypt, had been greatly reduced in number and bankrupted because of the shortsighted and despotic actions of the *Mamlūk Beys*.¹³

It must be stated that there is nothing fundamentally wrong in Gran’s methodology, on the contrary, this is the field where the merits of *Islamic Roots of Capitalism* are undisputable. The book is the first attempt to create a synthesis between micro- and macro history, to relate life stories of individuals in a wider social and economic context. Gran has set an example to other scholars who have followed his path. Undoubtedly, the most celebrated case is Nelly Hanna and her pioneering work about the literary culture of the Egyptian middle classes.

So the problem of Gran’s presentation is not the idea of utilizing cultural material as a source of the sociohistorical study. It is quite remarkable that throughout his work the problem in his argumentation lies in a much more trivial matter – in the dates. First of all, most of the features he deems as indicators of the nascent capitalist culture are not characteristic to Egypt in the period between 1760–90 but are found around the Middle East in various locations at various times, without having any connections to capitalism. Individualism is a case in point.

According to Gran’s thesis, the age of “merchant capital” had a strong individualistic tinge. This can be seen in phenomena like *khalwa*, the ritual seclusion of the *Šūfīs* and in the literary genre of biography, which grew during this period.

¹² Gran 1979:18.

¹³ Crecelius 1998: 84–85.

This is simply an arbitrary interpretation. Using these standards, individualism can be shown in almost any material. The *khalwa* was not an innovation nor typical feature of the late Ottoman period but an essential part of the *Ṣūfī* practice since the earliest days of Islam. As for biographies, their role in the literary culture was marginal in comparison to the heydays of this genre in the times of the *Mamlūk* sultanate hundreds of years earlier. To take up these features as evidence for something like a “bourgeois individualism” in the late Ottoman period is far fetched at best, and totally arbitrary at worst.¹⁴

Regarding Gran’s core thesis, problems in the dating are fundamental defects. It is of the utmost importance that he could show the capitalist features as they were during the very period of 1760–90, because after Napoleon it is much more problematic to identify their origins. They can be proved indigenous only by showing them in the period in question, not before and certainly not after, when contacts with Europe had become commonplace. Gran himself does not deny the opportunities of borrowing from Europe, which were evident during the reign of Muḥammad ‘Alī and after. What he questions is the need of this borrowing. Regarding these facts, the haphazardness in the dating is truly a peculiar feature. To take one example.

One of the most important social changes that took place in Egypt during the period was the birth of a new kind of elite, created by social mobility which draw the military and economic elites closer to each other.

To conclude this discussion of the social structure of the Delta during the late eighteenth century, it seems safe to state that, while the majority of the inhabitants were *fallāḥīn* rooted in the subsistence economy, the possibility of being uprooted from it existed as never before. One such case was that of al-Ḥājj Ṣāliḥ al-Fallāḥ, a poor merchant from al-Minūfiya who progressed from selling farm produce locally to selling slaves, and finally became a *Mamlūk*, in spite of the fact that he was an Egyptian. How many others were able to take similar advantage of conditions in the Delta?¹⁵

First of all, this example is beside the point. Gran tries to show a transition in the labour market and social structure based solely on the fact that one man rose to the *Mamlūk* class. This would be a weak argument in any context, but in the case of al-Ḥājj Ṣāliḥ al-Fallāḥ it is not relevant at all. Apparently Gran is using some rather concise secondary source in this passage, because he has failed to notice that the person in question passed away already in 1755. He could not have had anything to do with the transition, which according to Gran’s own time scale was not even to begin until five years later. Neither was he exactly “a poor merchant”.

¹⁴ Gran 1979: 37.

¹⁵ Gran 1979: 26.

If we take a closer look at the life of al-Ḥājj Ṣāliḥ al-Fallāḥ it becomes obvious that he had nothing to do with the methods of enrichment which can be termed capitalistic or considered somehow characteristic of the late eighteenth century. As a young boy he was pawned in order to pay a debt to a household of an eminent *ojak*-officer. There he prospered, bought *Mamlūks* and formed a household of his own. He gained influence by lending money at interest to persons in power and by placing his own *Mamlūks* in high positions in the *ojak*-regiments. Unlike what Gran relates, he did not become a *Mamlūk* himself as the disparaging nickname clearly indicates. Even at the height of his power, al-Ḥājj Ṣāliḥ al-Fallāḥ rode a donkey despite the fact that he owned a number of *Mamlūks* riding horses.¹⁶

To make his point clear, Gran has not trusted solely on the case of al-Fallāḥ but also raises another example of capitalistic structures in formation. This is the case of Shaykh Ḥasan ibn Ḥusayn al-Kinānī al-Ḥanafī. Again, the method of enrichment was not making a profit in the market. Gran presents the following piece of information: "When Muḥammad Khūsraw Pasha, the Wālī, came to Egypt, Shaykh Ḥusayn went to him immediately and was rewarded with presents and made *nāzir* of an important *waqf*".¹⁷ Once again I fail to understand Gran's reasoning. Functioning as *nāzir*, an inspector of property donated to a mosque, or receiving presents, is hardly an indicator of a major economic transition. The *waqf* was by no means an innovation of the period, but the more like the most traditional economic institution imaginable.

In addition to this, we have the question of dating. A historical fact is that Khūsraw Pasha came to Egypt with the Ottoman army in 1801. That means the whole incident happened ten years after the age of "merchant capital" and even after Napoleon's expedition. It is also considered doubtful whether al-Kinānī ever benefited much from this new source of income. In those days chaos and anarchy were widely spread in the country, and administering this property was most likely far from easy. Khūsraw Pasha was not of much use to him for very long, because after a year and a half Muḥammad 'Alī and Muḥammad Bey al-Bardīsi ousted him from power in Egypt in a quite unscrupulous manner.¹⁸

These passages demonstrates how the examples given by Gran are not just badly chosen but often misdated. Occasionally this is just too obvious, as in the passage where Gran takes up the imaginary genealogies of the *Mamlūks*, which he deems indicate the collapse of the organic solidarity of the *bayts* and the assimilation of the military elite with Egyptian society. In this case, he misdates by

¹⁶ Winter 1992: 69.

¹⁷ Gran 1979: 26.

¹⁸ Marsot 1984: 37–39.

almost a hundred years. He refers to the genealogies created for the *Mamlūk* fractions called *Qāsimīya* and *Fiqarīya*. The exact origin and function of these genealogies is obscure, but it is certain that they were not composed in order to hold the *Mamlūk* institution together after 1760. As Michael Winter has noted, this kind of genealogy has already been written for Riḍwān Bey (d. 1656).¹⁹

Taken as a whole the book is full of these kinds of obscurities and mistakes. One possible reason might be that, according to Gran's own testimony, it was originally not his intention to write a social and economic history of Egypt. He was mainly concerned with culture and literature, and it was only after he found what seemed to him distinctly modern features, such as individualism and economic utilitarianism, that he turned his attention to social and cultural factors. This can be seen in his work. The core of the cultural material, the life and writings of Ḥasan al-ʿAṭṭār, is a sound work, but the social and economic material presented in his book is compiled in a hurry, and as examples given clearly demonstrates, very often without giving a second thought to the work as a whole. Unfortunately for his narrative the basic scientific principle is that, if the premises are incorrect, the syllogism is incorrect, however welcome it should be.

This said, one must hasten to add that the purpose of this article is not to discredit *Islamic Roots of Capitalism*. Quite the opposite, as I have previously stated, the merits of the book are undeniable both in methodology and in the questions it poses. It was the first book pursuing a truly comprehensive picture of the social reality of an Islamic society at the dawn of the colonial age. True, it was done hastily and it has its shortcomings but what Gran was attempting to do methodologically was something totally new.

It can also be pondered whether the task pursued in *Islamic Roots* was simply too much in the late 1970s. The substantial data available at that time was quite minimal for this kind of synthesis. The later half of the eighteenth century used to be a totally neglected period in Islamic history, and Gran himself has done much to direct attention to this intriguing era. Currently the sheer amount of information is exponentially greater than back in those days. The eighteenth century, which Reinhard Schulze still in 1990 complained to be "nicht lesbar", is currently reasonably well outlined, though there still are areas we know next to nothing about.²⁰ One would presume that though Gran is determined to defend his views in general, it is very unlikely that he still would defend every thesis presented in *Islamic Roots*.

To take one case in point, it is obvious that during the time of writing Gran understood that manumission meant the breaking of the bond between a *Mamlūk*

¹⁹ Winter 1992: 21; Hathaway 1998: 42–43.

²⁰ Schulze 1990: 153.

and his Master: "Numerous Mamlūks were abandoned that is, manumitted by their former Lords as an expense."²¹ Naturally this is incorrect. The bond between a *Mamlūk* and his Lord was not juridical in nature, so the manumission changed very little in their relationship, being more like a formality. Manumitted or not, a *Mamlūk* was a part of his Master's *bayt* and this bond lasted even after the Master's death. This feature is very apparent in the most important contemporary source, the chronicle of al-Jabartī, where not just *Mamlūks*' names are given but usually their Masters' as well, which determined their social and political alignment to a high degree. When relating the death of ʿUthmān Bey al-Sharqāwī, al-Jabartī formulates all necessary information in the wording "al-Amīr ʿUthmān Bīk al-maʿrūf bi al-Sharqāwī wa huwa min mamālīk Muḥammad Bīk Abū al-Dhahab".²² Naturally, the manumission had already taken place and, furthermore, by that time his Master Muḥammad Bey Abū al-Dhahab had been dead a quarter of a century.

To find mistakes of this kind in the original edition of *Islamic Roots* is maybe not so surprising regarding the obvious haste in which Gran compiled his social and economic analysis. To find these in the new edition of 1998 is a bit more puzzling. Apparently Gran has used his critics De Jong and Baer as proof-readers, judging by the observation that the mistakes pointed out by them are usually corrected when this could be done by changing a few words or numbers. These are mostly mistakes in years, names and so on. Unfortunately, it is as obvious that a new, thorough proof-reading has not been done. Mistakes not listed by the two main critics are left in the new edition, even the most obvious ones like the strangely dated reign of ʿAlī Bey in the quotation on page 17.

Another problem concerns the errors explicitly noted by Baer or De Jong, but which for one reason or another are left uncorrected. I find it remarkable that, according to Baer's own testimony, one of his articles is referred to by Gran in a manner which the text in question does not sustain.²³ Despite this statement, the reference on page 114 can be found in the new edition as well.

It is quite apparent that time has not treated *Islamic Roots of Capitalism* kindly. The hastiness and haphazardness of the original compilation have become even more obvious in the light of current studies. Arbitrary corrections have not improved the general credibility of information presented; on the contrary, they have made the book even more incoherent.

For Gran himself, the most embarrassing errors in the original edition were most likely serious blunders in the social background of *Shaykh Rifāʿa al-Ṭaḥṭāwī*.

²¹ Gran 1979: 179.

²² al-Jabartī 1904–05, III: 181.

²³ Baer 1982: 219.

On page 97 he claims: "He was the son of a merchant from Delta displaced by the rise of Muḥammad 'Alī". This statement is simply incorrect since al-Ṭaḥṭāwī was a member of a prominent family of religious scholars and his father was *multazim*-taxfarmer.²⁴ Delta as the place of his father's origin in the passage is totally enigmatic, since the family had become established in Upper Egypt for generations and his hometown was Ṭaḥṭā, about 430 kilometres south of Cairo.²⁵ These are quite amazing mistakes because al-Ṭaḥṭāwī's background is quite well documented and the source material is concordant in this matter. As is very often the case with controversial data, no reference is used, so the source of this uncorrect information remains obscure.

In the new introduction of 1998 Gran studies al-Ṭaḥṭāwī in greater length so it is understandable that the original mistakes related to his person placed the analysis in an awkward position. So, in the new edition, the sentence is reformulated as "He was the son of a *multazim* from the Ṣa'īd who was displaced by the rise of Muḥammad 'Alī".²⁶ However, superficial corrections like this tend to cause more problems than they solve. Further in his book, on page 185 he returns to the subject:

al-Ṭaḥṭāwī's social background alone would explain why he would appreciate the virtues of the *dīwān* system of the eighteenth century, a form of republicanism, especially for merchants like his father, and the total disruption brought about by the bureaucracy of Muḥammad 'Alī.

In the new edition only the previous passage has been corrected but in the later, al-Ṭaḥṭāwī's father is still a merchant. This is a sad indication of the negligence of the revision. Furthermore, it is a bit odd that in a theory which lays much stress on the social and economic factors, these can change drastically without affecting the conclusion based on those factors. The words are corrected but not the thought behind them.

In his article Dror Ze'evi pointed out that a new clear periodization has failed to materialize in Middle East studies. This can hardly be denied, and *Islamic Roots of Capitalism* is indicative why this is the case. A periodization cannot exist in a schematic vacuum but needs a narrative which it could be based on. The periodization is not just dates: it is the way in which the story is told, and this story must be in harmony with the updated information. That is, by the way, the very reason why the Orientalist paradigm has not fared much better, and it is easy to agree with Ze'evi that it is time to move on from the whole idea of periodization.

²⁴ Öhrnberg 1993: 523–524.

²⁵ Newman 2004: 29.

²⁶ Gran 1998: 97.

It is quite apparent that not only has a new periodization failed to materialize but above all a narrative behind it. This has truly been attempted only by Gran, and he actually never finished his narrative. Regarding the social and economic facts of *Islamic Roots of Capitalism*, the book is more like a sketch than a finished work. There are some who might disagree on this but it is apparent that the many factual errors of both editions make his narrative untenable.

Islamic Roots was one of the most important works of its generation, and it still is a book of great vision, and definitely worth reading, but its original purpose, to establish 1760 as a new watershed in Egyptian history, has apparently failed. In my opinion, there is nothing amazing in this. The scholarly community is skeptical by nature. The systematic doubt and counter-argumentation are the very means of testing theses and gaining knowledge. There are very few theories in history which are adopted instantly and without revision and elaboration. One can almost state that theories are either elaborated or forgotten.

Considering this logic in which the scholarly world functions, I found it quite puzzling that Gran never updated *Islamic Roots*. After the first hastily compiled edition, he had twenty years to do so and present a new, stronger revisionist narrative where the latest data would be incorporated. Obviously, there is hardly much point to carry on the paradigmatic discussion unrelated to the actual historical facts. However, a new, updated revisionist narrative has not been compiled by Gran or someone else. After *Islamic Roots*, Gran seems to be concerned mainly with broader theoretical issues and left the actual theory behind the academic discussion to the form which it practically had already in the late 1970s. I must say that I found this very regrettable. In its present form, *Islamic Roots of Capitalism* is a book whose apparent lack of finishing touch and other defects in presentation does not do justice to the great significance it had in the field of Middle East studies.

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