

HAYIM LAPIN

Economy, Geography,
and Provincial History
in Later Roman Palestine

*Texts and Studies in
Ancient Judaism*

85

Mohr Siebeck

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Martin Hengel and Peter Schäfer

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Provincial History
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Mohr Siebeck

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Preface

This study began some time ago in conversations with Steven Rubenstein (now of the University of Ohio, but then a fellow graduate student at Columbia University) who suggested it as a “quick” paper. Other projects, teaching, and occasional administrative responsibilities have intervened between those initial conversations and the present, and these in part account for the number of years that it took for the completion of this project. But the work of data collection and entry have also played a substantial role in postponing completion. In the interval, I have benefited from support from an Iwry Summer Fellowship from the Joseph and Rebecca Meyerhoff Center at the University of Maryland (1995), a Memorial Foundation for Jewish Culture Fellowship (1996–1997), an NEH Fellowship at the Albright Institute for Archaeological Research (1996–1997), a General Research Board Fellowship from the Graduate School of the University of Maryland (Spring 1999), and generous additional research, editorial, and production support from the Meyerhoff Center and from the Kaplan Fund. During a semester leave from Maryland in 1999, the Department of Religious Studies at the University of North Carolina at Greensboro gave me an academic home. In addition to the libraries of the institutions with which I have been affiliated during the writing of this project (the University of Maryland, UNCG, and the Albright Institute), I have also made ample use of the libraries of the Hebrew University (especially the Institute for Archaeology Library) and Duke University.

A number of people have read or heard all or part of the manuscript as it took shape, and to them I am especially grateful: Steven Rubenstein (who was kind enough to read chapters of the finished product), as well as Roger Bagnall, Cynthia Baker, James Brooks, Kenneth Holum, Derek Krueger, Eric Meyers, Jonathan Reed, Michael Satlow, and Seth Schwartz, and the 1996–1997 Fellows of the Albright Institute. Mordechai Avi‘am, of the Israel Antiquities Authority, generously took me to several Galilean sites. Professor Martin Hengel, one of the editors of this series, was gracious and encouraging in his acceptance of this manuscript. This is a fitting opportunity to thank Professor Peter Schäfer, the other editor of this series, not only for offering the manuscript a publication home, but also for his support of my work over a number of years. A preliminary version of the argument was presented at the Association of Ancient Historians Annual

Meeting (1999) and I am grateful for the discussion that resulted from that presentation, as well as the comments from the members of a graduate seminar on ethnicity, indigeneity and nationalism at the University of Maryland. It is not merely a pious commonplace on my part to insist that errors or conceptual problems that remain in this book are my own and not theirs. The same is true of the contribution of two other people without whom the manuscript would not be in its present form: Ronda Angel, who provided editing for the manuscript, and Laurence J. Aurbach II, who drew the maps to my (changing) specifications.

Immeasurable thanks are due, finally, to Maxine L. Grossman for her multiple readings, extended discussions and criticism, and long-standing patience and encouragement. It is to her that this book is dedicated.

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Note on Abbreviations and Citations

In general, abbreviations of the titles of both ancient texts and modern works have followed standard guides (in most cases approximating *The SBL Handbook of Style*, P. H. Alexander *et alii* eds. [Atlanta: Society of Biblical Literature, 1999]). Bibliographical abbreviations used in the notes are filled out in the Bibliography. Citations from the Mishnah, Talmud Yerushalmi, and Talmud Babli are given according to the standard printed editions, the Venice *editio princeps*, and the Vilna editions respectively. The Tosepta is cited by tractate, chapter, and *halākā* according to the Lieberman edition, and where this is not available, from the Zuckerman edition. For most other texts citations include page references to modern editions (listed in the Bibliography by editor). A similar citation style is used for texts in Greek and Latin, although editions and page numbers have generally been omitted for texts available in the *Loeb Classical Library*.

Introduction

The cultural and social historiography of later Roman Palestine must be rebuilt from the ground up. By this I mean not only the writing of history “from below” (a project against which both ancient literary evidence and contemporary practices of archaeology generally conspire), but also, quite literally, that a primary starting point for any reconsideration of the history of Palestine is the question of where and how people lived on the ground, and how their living spaces their inhabited landscapes were organized. Such a reconsideration requires a fundamental rethinking of household, local, and regional economies, of power within imperial and provincial political economies, and, ultimately, of such seemingly “primordial” cultural or social ties as kinship, religion, or ethnicity that framed the moral economies of historical agents.

That all of these aspects of the history of later Roman Palestine, and their mutual connections, require rethinking may be illustrated through an examination of two quotations from recent works on ancient Palestine. Neither work is a full-fledged treatment of the Palestinian economic or social landscape in antiquity as such, but, precisely for this reason, they illustrate the dichotomous historiographical use of two rather standard views of the Palestinian economy. They therefore further demonstrate how assumptions about the economy are implicated in a series of judgments about the identity, culture, and history of the people whose economy they describe. The first of these quotations comes from Yizhar Hirschfeld’s study of rural settlements in Byzantine Palestine:

Another, more significant, example of the [typical or generalized] village’s lack of planning is the absence of public market squares. Among the many villages known to us, none contains a space that can be interpreted as a market square. This phenomenon corresponds well with the fact that the villages had no rows of shops. It appears, therefore, that the commercial activity reported in the rabbinic sources may have taken place in shops located in private dwellings as at Khirbet Susiya.... Another means of maintaining commercial relations between the cities and the villages was traveling salesmen, or peddlers, who are mentioned several times in rabbinic literature¹

The second is from an attempt on the part of Richard Horsley to place Roman period Palestine within an imperial political economy in which relations of power are exploitative, and where agricultural production is “traditional”:

¹ Hirschfeld 1997: 63–64.

Virtually nothing indicates that towns and villages had established stores or even small markets. No buildings have been found in rural settlements that could be associated with stores or markets. No texts attest the appointment of an *agoranomos* (market inspector or supply officer) in villages. Rabbinic passages adduced for “market days” refer instead to “days of assembly” and court.... There would have been no need for a local village market since there was apparently little division of labor in village communities based on agricultural production.... As in agricultural production proper, so in the production of clothing and other crafts, the household members undertook most or all the stages themselves. The rabbinic ideals of a self-sufficient household and hostility to commerce simply reflect the traditional practices of peasant households attached to their ancestral lands. In this regard peasants in Galilee were no different from those elsewhere in the Roman empire. If there was a surplus or if craft-production was for sale, the producers apparently did most of the selling or bartering themselves.... At times the house functioned not only as “workshop,” but as “store” as well.... “Most transactions in the village [sphere] were between local householders.” Far from the village being a market economy, it appears that most village communities as well as their constituent households were relatively self sufficient.²

It is only a slight rhetorical exaggeration to state that, in this refraction of the formalist/substantivist debate, for Hirschfeld village marketing exists because it does, while for Horsley it does not exist because it does not. It is striking that despite their differing conclusions, both authors actually make similar claims, at least about material remains: the evidence for rural market *places* is lacking (do we know what these would have looked like?³) and the locus of rural production may have been in, or attached to, domestic spaces.

Underlying these two disparate readings of the Palestinian economy is a whole series of assumptions or questions that need revisiting, of which I will highlight here only a very few. One set of problems has to do with how literary texts are mined for meaningful or useful analytical concepts (“market” or “assembly”?) or for what “people” actually did (where did textually attested marketing, apparently largely invisible in the archaeological remains of the built environment, take place?). Another set, rather surprisingly (especially since Hirschfeld is particularly interested in the typologies of rural settlement⁴), is raised by the apparent willingness on the part of both authors to treat “villages” as a fairly autonomous and undifferentiated field. Yet, need all (or typical) villages have “maintain[ed] commercial relations with cities” (Hirschfeld) in the same ways, or, for that matter, at all? And why must we assume that villages in Palestine, let alone in the entire Roman world, typically were organized around the farming of “ancestral land” (Horsley)? This last expression implies stability of both kinship structures and the distribution of property, and with them a mode

² Horsley 1996: 74–75; I have omitted parenthetical citations. The sentence in quotation marks is from Safrai 1994: 231.

³ Cf. Hirschfeld 1997: 63 n. 118.

⁴ Cf. Hirschfeld 1997: esp. 37–39.

of production (village and household autarky), lasting generations. Yet the extent to which agricultural land changed ownership in Palestine in late antiquity is in fact unknown, and evidence from tax registers from the Hermopolite nome in Egypt has been taken to suggest a fair amount of turnover through sale.⁵ Furthermore, as I will argue in Chapter 2, there were substantial demographic shifts over the long period covered by the “Roman” and “Byzantine” periods, which saw the growth in the raw number of settlements throughout the region (and in some, perhaps, a later decline). Such shifts might plausibly be expected to have affected patterns of cultivation (including the bringing of new sub-regions under cultivation), and (although this is of necessity invisible to us) may well have impacted competition for agrarian resources and their expression at the regional, village, and household levels.

In both cases, too, the respective views of villages carry with them assumptions about religious and ethnic culture and how these are manifested. For Horsley, villages are said to be structurally like villages throughout the Roman empire; but they are also presumed, in their particular northern Palestinian setting, to link back in a meaningful way to an Israelite (i.e., not a Judean/Jewish) historical identity.⁶ For Hirschfeld, settlement patterns in “Jewish” eastern Galilee and the Golan, and in “Samaritan” Samaria, with few or no rural farmsteads, reflect cultural needs of Jews and Samaritans for communal institutions (synagogues) that regulated their religious and ethnic lives.⁷ There are specific problems associated with these views. For instance, Horsley’s links between Roman-period Galileans and Iron-Age northern “Israel,” for instance, are elusive at best.⁸ And synagogues themselves emerged for the first time as distinctive buildings in the period that Hirschfeld is surveying, so that the phenomenon of synagogues might equally be described as the product of regional settlement patterns and rural economic structure and not their determining cause.⁹ For present purposes, however, it is more important that neither author takes sufficient account of ethnicity as a changing, historically constructed, phenomenon. The relations that bind villagers to one another and to other villages, and that bind non-elites to elites – whether in cities or villages – are among the

⁵ Bagnall 1993: 72; Bowman 1985 on the Hermopolite Nome in the fourth century; cf. Rowlandson 1996: 178; 192–98 on the Oxyrhynchite.

⁶ Cf., e.g., Horsley 1995: 172–75.

⁷ Hirschfeld 1997: 42–43, 67; see also Pastor 1997, critiquing Hirschfeld 1996.

⁸ See, e.g., Reed 1999; 2000: 23–61.

⁹ See, e.g., Magness 2000 for the late dating of Palestinian synagogues. It may also be worth pointing out that the phenomenon of “gathering day” (discussed below in connection with *m. Meg.* 1:1–2), even as an idealizing construct of rural religious interactions by rabbis, may presuppose the role of central villages serving the religious and cultural needs of a wider rural population that gathers there on a periodic basis.

processes that need further elucidation if we are to develop a historically sensitive approach to Palestinian ethnicities in antiquity. These processes, in turn, are economic: they include, for instance, market-mediated ties where these exist, the distribution of landownership, and, with these, patterns of local or regional patronage. They are also inevitably spatial in that they span geographical space, and extend beyond the confines of the individual dwelling, farmstead, or village.

Finally, the place of political economy in all of this needs reconsideration. For Hirschfeld's treatment here it appears to play no particular role (except to the extent that the Roman suppression of two revolts is thought to have played a role in the demography and settlement pattern of Jews in Palestine). For Horsley, however, since the rural economy is subsumed under a regime of "traditional practices of peasant households and villages" and their relationship, marked by exploitation, with cities and elites, political economy is all-important but under-examined. Exploitation, for instance, can take many forms and need not be limited to the direct exaction of tribute.¹⁰ How might particularly Roman and late-antique modalities of imperial rule have transformed the practices of labor, residence, and competition throughout the landscape? Might cities, for instance, have played a transformative role through their own peculiar consumption needs, including not only subsistence and luxury goods for consumption, redistribution, and display, but also labor for building and maintaining the urban agglomerations themselves, or for working the lands that provided the consumption or trade goods?

This book cannot, of course, offer definitive answers to all these questions. Rather, it offers a kind of exploratory exercise: to sift through literary and archaeological evidence and to ask how we might begin to lay the groundwork for a different kind of historiography. In doing so, I have made use of central-place theory, a set of models of geographical interaction, as a way of collecting and organizing material for further study. The pages that follow examine archaeological evidence for the distribution of sites of various kinds and sizes, some of the material evidence for economic activity in those sites, and literary evidence for the organization of economic activities across the northern Palestinian landscape. In this

¹⁰ Cf. Wolf 1957, arguing that "closed corporate communities" of the kind Horsley appears to be envisioning (as merely "traditional") are the product of particular histories of exploitation. (Carlsen 1997: 47–50 discusses responses to Wolf, locating the closed corporate character of Mayan villages in pre-Columbian Mesoamerica, rather than in the aftermath of the Spanish conquest as Wolf had argued, but even so, there is no need to assume that all "traditional" villages everywhere are organized in this way.) Wolf 1966: 37–48 surveys a number of types of engagement of "peasants" with markets. See also the comments on Wolf in Roseberry 1989: 117–118; Cancian 1989: 134–137, 158 n. 8; and compare the marketing systems elaborated for China in Skinner 1964, 1965.

context I have asked, first, whether these remains from antiquity fit with ideal central-place landscapes, and second, whether the fit (and its failures) might contribute to reframing the way we understand the regional and economic history of later Roman Palestine.

The book as a whole is organized as follows. Chapter 1 describes one of the standard formulations of central-place theory and some of its implications for the study of a regional economy in later Roman Palestine. The archaeological material assembled for this study is reviewed in Chapter 2, with particular attention to five *Archaeological Survey of Israel* surveys. In addition, I have pointed out areas where archaeological material, as it is reported and published, poses interpretive problems for historians.

Chapter 3 offers hypothetical constructions of central-place models for northern Palestine. Chapter 4 deals with geographically specific literary traditions about marketing, and with texts dealing with marketing and commercialization more generally. Initially the goal of this study was the construction and comparison of detailed central-place “maps” of northern Palestine based on archaeological and literary evidence respectively for the distribution of economic and other institutions. As I argue in Chapters 3 and 4, below the level of the very largest sites such maps are beyond the limits of the evidence available. However, the theoretical framework discussed in Chapter 1 remains useful for thinking through the spatial aspects of the organization of the late-antique Palestinian economy. In general I believe that central-place considerations support a view of the regional economy in which the organization of a commercialized sector of the economy centered on cities may have structured rural production and exchange, but not necessarily or entirely through market mechanisms. Thus, although my general assessment is close to the view of an “embedded” economy, associated among ancient historians particularly with Moses Finley,¹¹ I have also been interested in the way that markets may have participated in the making of particular patterns of embeddedness. Finally, Chapter 5 reconsiders the history of northern Palestine in late antiquity based on the results of the preceding chapters.

The discussion of archaeological material is based on a database of more than 900 records compiled from published surveys and archaeological reports. The database covers roughly the Galilee, the Bet Shean valley, and the Golan. Initially, the study was to focus on Galilee alone, but for reasons both practical (most notably, the regions best covered by published archaeological surveys) and analytical (the question of the history of regions in relation to one another), it quickly became clear that I needed to expand my research to adjacent areas. For present purposes (particularly for the

¹¹ Finley 1985. The characterization of premodern economies as “embedded” goes back to Polanyi 1944: 57.

production of extensive archaeological surveys) those areas fell largely within the borders of the modern State of Israel or territory controlled by it – but did not entirely correspond to late Roman organizations of territory. In this way the politics of contemporary nation-states (and the practices of archaeology within their territorial borders) have shaped my work, a feature whose significance was again underscored only late in the process of revision, when I was evaluating the possibility that what became the fifth-century province of Palaestina Secunda, largely split between present-day Jordan and Israel, could be construed as the unit of regional economic integration (see Chapter 2).

The ancient literary texts that I have drawn upon, preserved in Hebrew, Aramaic, Greek, and Latin, were largely, although by no means exclusively, written or compiled by Jews and Christians working as such. As noted above, the initial plan was to draw a map based on literary evidence, that might be compared in detail to a map based on published archaeological evidence. Although it subsequently became clear the various literary corpora were too spotty in their coverage (or, to put it another way, too rooted in their own specific, and frequently urban and elite, concerns) to allow for such a comparison, this initial goal constrained the way material to be discussed in Chapter 4 was collected, and that chapter therefore is focused primarily on texts (descriptions, anecdotes, rulings, and so on) that specifically localized certain kinds of economic activity in a particular city or village. In this regard, as in many others, given the geographical areas covered by this study, rabbinic texts were by far the richest body of material.

Central-place theory was developed in the early part of this century to describe the distribution of production, exchange, and markets over a geographical space, and like all analytical models starts with “unreal” assumptions, most notably the hypothesis of a flat featureless plain. Standard paradigms are based on assumptions about modern retail trade or production, precisely notions about whose relevance to the ancient economy historians are apt to disagree. Rather than simply provide normative or ideal models, therefore, I have spelled out in Chapter 1 my understanding of the “classical” derivation of central-place theory¹² and some of its implications for thinking through problems of economic history in Palestine. However, two particular features have made central-place theory useful for this study, and bear stressing here.

The first is that despite the ideal assumptions that theorists might make in laying out a model landscape, it is primarily a tool for describing the distribution of individual places over a geographical space and the different kinds of hierarchical relationships places may have to one another. It

¹² Based primarily on Christaller 1966, with Skinner 1964; 1965; C. A. Smith 1976b.

therefore has been useful for proposing possible analytical frameworks for discussing archaeological materials that are typically excavated, published, and interpreted first in connection with a given site, and where it is all too easy to elide the differences between artifacts (for instance, coins, imported pottery, or the presence or absence of pig bones) and the social formations such as trade or ethnicity that might account for their distribution. Slippages between individual examples and “the world” are possible in the case of literary material as well. A case in point is the insistence on the part of some scholars on describing in terms of regional specializations in market crops or in terms of market oriented trade, an episode that Josephus narrates about John of Gischala and the sale of oil to Jews in “Syria” under specific circumstances.¹³

The second feature (although one that will surely raise the hackles of some readers) is precisely the fact that central-place theory neither derives from the disciplines that specialize in the study of ancient Palestine, nor was it developed with the geography of Palestine in mind. It has, however, been used productively to analyze the spatial organization of agrarian economies in both contemporary and historical settings, and to link other questions of social organization to regional economic patterns. Hence in “applying” central-place theory to late antique Palestine, one is constrained to articulate historical questions in terms that are somewhat more abstract, and therefore more readily comparable to other examples across historical and geographical divides, than the terms of a particular geography, a particular political, ethnic, or religious historiography, or particular bodies of textual evidence in which such questions usually are posed.

No amount of theorizing can replace the contextualized study of the data that are available, with all their vagaries, lacunae, and problems. Part of the burden of this book has been to address some of the textual and material data, and I have also attempted to show in the last section of Chapter 1 the extent to which some literary passages are readily interpretable in economic and geographical terms derived from central-place theory and related fields. However, it is primarily the disciplining effects of working with an explicitly theoretical and generalizing framework, rather than pretending to no theoretical presuppositions and letting the “evidence” “speak for itself,” that I have found most productive for reexamining some of the conventions of the historiography of later Roman Palestine. Central-place theory has been useful for disentangling the dichotomous terms in which the history of Palestine is frequently discussed – between archaeological and textual scholarship or evidence,¹⁴ between “historical” and “literary”

¹³ Josephus *BJ* 2.591–92; *Vita* 74–76. See below, Chapter 4.

¹⁴ See, e.g., the contributions to Edwards, McCollough, eds. 1997. *Ma'oz 1997* is a peculiar contribution, written by an archaeologist but insisting on the primacy of literary evidence. See also Reed 2000: 1–22.

understandings of texts,¹⁵ between formalist and substantivist understandings of the ancient Palestinian economy (or in the somewhat cruder terms in which economic history in Roman Palestine is discussed, between assumptions of a free market and the firm knowledge of its absence¹⁶), not to mention such naturalized dichotomies as that between “Jew” and “gentile” – and hopefully for setting them aside. Perhaps unwisely, given a complex of academic fields that resist “theory” – whether in the form of “positivist” social-science modeling or of “postmodern” notions of indeterminacy, overdeterminedness, or multiplicity of meaning, as coming from outside the “evidence” – this study invokes one such social science model only to insist that the resulting reconstruction of a late antique landscape is at once fictive and at the same time productive for a regional history of later Roman northern Palestine.

The effects of this exploratory exercise – manifested textually in the kinds of arguments about evidence and models made below, and graphically in occasional calculations, diagrams, graphs, or tables – should not be confused with false claims to rigorous proof of historiographical hypotheses. The fit between “model” and “evidence” is never more than approximate or impressionistic. Moreover, it is a certainty (barring accidental resemblance) that no areas of later Roman northern Palestine matched in detail one of the models proposed in Chapter 3. However, the process of asking, for instance, what a maximal distribution of markets might look like, starting with certain kinds of base assumptions (Chapter 3), in turn has led to strategies for rereading texts that are frequently mobilized for discussions of the Palestinian economy, but with more explicit attention to regional contexts in which marketing and commercialization may be differentially distributed across the landscape (Chapter 4), and for placing both “commercial” patterns and “cultural” history within a regional political economy (Chapter 5). In focusing on a somewhat constraining set of analytical problems, I have thus tried to draw attention to the ways we go about asking the discursively powerful questions that make up the historiography of later Roman Palestine (itself a highly ideologically charged field), by bringing to bear on the conventional corpus of evidence a supplementary (“external”) set of questions.

The chronological scope of this study is, nominally, the fourth century. This “nominality” bears some discussion since it reflects both my understanding of the evidence and my methodological decisions about how best to address it. The fourth-century present is the result of a somewhat

¹⁵ Cf. Lapin 1995: 1–36. See also Hayes 1997: 1–30; 1998.

¹⁶ The conventional terms of the discussion in recent years are given in the published debate: Horsley 1995a; Meyers 1995; Horsley 1995b. For a capsule description of the formalist-substantivist debate see Hodges 1988.

artificial assignment of a variety of kinds of evidence to a period of apparent convergence. By the fourth century the distribution of *poleis* (juridically recognized cities) and roads was more or less fixed, although there were later adjustments. The period of densest settlement and richest archaeological attestation in the areas covered by this study frequently is dated as simply “Byzantine,” by which archaeologists working on Palestine typically mean a period that begins in the early fourth century and extends to the early seventh century, although there are alternative periodizations (see Chapter 2). The fourth century also saw substantial overlap between a developing Palestinian Christian literature relating to the history of Palestine and “amoraic” rabbinic texts. Christian texts with a distinct Palestinian connection began in the third century particularly with Origen but, in terms of the quantity and variety of material that has been preserved, intensify with Eusebius, whose writings straddle the period before and after the triumph of Constantine. “Amoraic” rabbinic texts, that is, texts whose primary named tradents fall between the early third century and the middle fourth century, were compiled and edited no earlier than the end of the fourth century.

If the fourth century setting is thus artificial (and if, in addition, this study makes regular use of material both predating and postdating that century), the effort to locate the historical present of this study in what is by the general standards of Roman and late-antique Palestinian historiography, a fairly circumscribed chronological framework, is significant for what it tries not to do. There is one tendency among some scholars to assume a slow pace of social change and long periods of cultural continuity, and consequently to ascribe to literary texts and archaeological evidence relevance for elucidating extended chronological periods. Groh, for instance, has called upon scholars interested in the first century to work creatively backward from later, more richly documented periods.¹⁷ Safrai’s study of Jewish community structures in Palestine explicitly argues that institutions described in some detail in rabbinic texts may be read back on periods centuries earlier.¹⁸ Although they may yield valuable results, such approaches are also fundamentally at odds with the view taken here in that they risk assuming precisely the naturalness, or at least the stability, of “primordialities” such as ethnicity, kinship, or religious affiliation, or of modes of social interactions, such as village organization or trade, that need to be further historicized. Instead, I have attempted to theorize a historical “moment” at which discrete bodies of material can be brought to bear on one another, while striving to be sensitive to patterns of both continuity and change. My approach has its own idealizing and leveling tendencies,

¹⁷ Groh 1997: 33–34. Cf. Taylor 1993.

¹⁸ Safrai 1995.

and can offer no more than a fictive construction of a history (as any “historical” work, at least on late-antique Palestine, ultimately must be), but it is presented as one that may help to recast the terms in which we understand that history and the sources upon which we base it.

In part, this book is intended to lay preliminary groundwork for a reconsideration of the history of Roman Palestine as a history of provincialization.¹⁹ The chapters that follow do not do justice to this area of inquiry, but their geographical focus offers a point of entry. I have tried to show in what follows, and particularly in Chapter 5, that in aggregate, and over an extended period of time, Roman imperial administrative decisions – the initial incorporation of territory into a province or its separation as part of a client kingdom; the placement or removal of soldiers; the elevation or reduction in status of particular sites – transformed the human landscape of northern Palestine. The point is not to argue that Roman administrators systematically set about a coherent and systematic policy of provincial transformation. “Urbanization” may serve as an example of the kind of process I am referring to, in the sense that it is difficult if not impossible to demonstrate an actual imperial policy of building cities,²⁰ yet cities were tied to imperial Roman administrative strategies on a number of different levels. Nor, in pointing to a *process* of provincialization, do I mean to suggest a series of inevitable stages that the territory went through. The point, instead, is to stress the consequences of imperial power – manifested, on the ground, in which people were given or took upon themselves certain prerogatives; in where and how they lived; and, in a literal sense, in what and how much was planted, and, of that, where and how and by whom it ultimately was consumed – in structuring the historical agencies of both the “rulers” and the “provincials” (neither forming a coherent or uniform group)²¹ and in organizing the structures of everyday life.

The approach suggested here foregrounds broad structural problems, such as long-term demographic changes or urbanization. This is, in part, making a virtue of necessity. Given the nature, paucity, and distribution of the literary and archaeological evidence that can be mobilized for a history of Roman Palestine it is much easier to speak in telescoped periodizations of generations or centuries, and of general historical trends that mark such periods. For northern Palestine, where so much depends on rabbinic literature, some scholars still attempt to draw out the intentionalities of agents

¹⁹ My thinking on provincialization has been influenced by Mitchell 1993; Alcock 1993; Woolf 1998.

²⁰ Isaac 1992: 333–371.

²¹ My thinking on power (imperial and local) and historical agency has been shaped by Wolf 1982; O’Hanlon 1988; Asad 1993: 1–24; Comaroff, Comaroff 1997: 1–62; Guha 1988; 1997.

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