

fought together against Rome in the second Samnite war, and remained loyal after Cannae. A similar pattern is identified in Apulia, although the textual evidence there is less robust (85–98, especially 89). F. draws on archaeological and numismatic material to argue that the communities who defected (namely Arpi and the neighbouring settlements of Herdonia, Salapia and Aecae) were bound together by economic and political ties stretching back to the fourth century B.C. Arpi's main rivals (Teaenum Apulum and Canusium) remained loyal to Rome, echoing the pattern in Campania where rival powers adopted opposing sides, and dragged their 'satellite' settlements along with them. F. is nevertheless careful to leave room for contingent developments that deviate from this pattern. One important example is the case of Thurii and Taras, which were major rivals yet both revolted to Hannibal (211–30).

Practically all of the Italian states that F. takes into consideration are city-states. A slightly more peripheral place is given to the non-urban political communities of the central Apennines, where Hannibal seems to have had very limited success in eliciting defections. F. refers to central Apennine communities as 'tribes' (291–4), although there is evidence that by the late third century these communities were already complex polities which minted their own coins. F.'s treatment of the central Apennines is understandably less detailed, given the lack of written sources. Yet he rightly suspects that resentment towards Rome may have been more pervasive in the region than ancient historical writers suggest. For example, he challenges Livy's assertion that all of the Pentrian Samnites remained loyal after Cannae, by noting that the Pentrian settlement of Fagifulae defected to Hannibal (Liv. 24.20.5). This comes as a timely contribution to broader debates about the extent to which ethnic identity influenced allegiance and political action in republican Italy, especially in areas where the city-state was absent. It is becoming increasingly clear that ethnic identity was one of several factors that shaped military and political action, as recently demonstrated by Isayev with regard to élite networks among the Hirpini in the second and early first centuries (E. Isayev, 'Italian perspectives in the period of Gracchan land reforms and the Social War', in K. Lomas, E. Herring and A. Gardner (eds), *Creating Ethnicities & Identities in the Roman World* (2012)).

The potential of F.'s approach to interstate relations goes beyond the Second Punic War, and opens up new interpretative possibilities for understanding Roman expansion in Italy. He makes some thought-provoking points about Rome's tumultuous relationship with the Samnites, suggesting that it may have resulted from an enduring rivalry where previous wars made subsequent wars more likely, rather than from any 'pathological' bellicosity on the part of Rome — or, for that matter, the Samnites, who are often accused in ancient as well as modern historical writing of being distinctively prone to war.

On a methodological level, F. makes a convincing case that aspects of Livy's narrative cannot be summarily dismissed as pro-Roman distortion, as upon closer inspection he reveals the complexity of relations among Italian communities. There is clearly some pro-Roman bias in Livy's image of Italic communities who repeatedly appeal to Rome for help. Yet, from the standpoint of *realpolitik*, F. convincingly argues that such an image is coherent with a highly competitive political setting such as that of Republican Italy, suggesting that it was by manipulating the local rivalries between Italian communities that the Romans justified their interference and extended their influence.

Overall, F.'s book contributes substantially to the history of the Roman Republic and appeals to a wide readership, from undergraduate students to specialists on the politics, society and culture of Republican Italy.

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D. DZINO, *ILLYRICUM IN ROMAN POLITICS, 229 BC–AD 68*. Cambridge: Cambridge University Press, 2010. Pp. xvii + 223, illus. ISBN 9780521194198. £55.00/US\$95.00.

The long coast of the eastern Adriatic that modern scholars call Illyria was of varying importance to Rome in the Middle and Late Republic and Early Empire. The southern coastal sector, south of the Lissus River, was of strategic significance because it was the gateway, via the Pindus Mountains, into Greece proper. This was a crucial route to control during the Roman conflicts with the kingdom of Macedon, but less important after 167 B.C. The northern sector was of little importance at any time under the Republic. It was a true backwater, and Roman aims were limited to preserving the few

coastal polities from occasional threats from a hostile tribal hinterland. In the 30s B.C. one can detect a change as Octavian pushed north-east from Aquileia towards the Danube; and the importance of the region increased as Roman conquests along the Danube intensified, because it was a link between those areas and Italy. This is reflected in the establishment of formal administrative organization, but this came to Illyria late. There then emerged the Roman administrative construction called 'Illyricum', which artificially cobbled together different ethnic groups at varied levels of development. This history is the subject of Danijel Dzino's monograph.

A major issue in the early history of this region is whether the Roman conflicts with the Ardiaean Illyrian dynasts in the late third century were merely punitive expeditions aimed at the suppression of Illyrian piracy (so, e.g., E. S. Gruen) or hid a larger design: either ruthless Roman expansionism (so W. V. Harris), or a conflict with Macedon over influence in Illyria (so M. Holleaux). D.'s contribution to this important controversy is not very coherent: the Senate was concerned not only with piracy, but also with the regional impact of an Illyrian-Macedonian alliance (44) — but this factor was not yet important in the two wars of 229 and 219 B.C. (45). D. then argues that the key to understanding Roman behaviour in this early period, and indeed for two-thirds of the period covered in the book, is not Roman geo-strategic considerations, and not 'imperialism' as moderns use the term, but emotions: Roman fear of barbarians or angry Roman reaction to perceived insults (19–20 for the general hypothesis; 48–9 for this early period; general hypothesis repeated in the Conclusion, 179–80). This is an interesting idea which recognizes the Romans' foreignness to us. But after stating baldly that the First Illyrian War 'was caused primarily by Roman reaction to the murder of their envoys' sent to the ruler of the Ardiaei in 230 B.C. (50), D. then brings in all sorts of geo-strategic and economic causes which he says are equally important — including alleged Roman fear of an Illyrian invasion of Italy, which no source posits: 'all appear to be the primary reasons for Roman intervention in 229 B.C.' (ibid.; my emphasis). Then he returns to the murder of the envoys as the primary cause of the war (ibid.). But Polybius says that the envoys were only in Illyria because of complaints from merchants about Illyrian piracy — or, according to the tradition in Appian and Dio, because of Illyrian attacks on the island republic of Issa — which, in fact, points to deeper issues than the (alleged) murder of an envoy.

A similar incoherence appears in D.'s discussion of Rome's post-war arrangements. On the one hand, the Senate 'did nothing more' than create individual relationships with some of the Greek cities on the coast; yet a major feature of the post-war settlement was that the Romans also created 'a Roman protectorate' in the region, as posited by Holleaux (50–1). This reconstruction has recently been strongly disputed, but the point is that you cannot have it both ways. Similarly, D. leans towards rejecting Peter Derow's contention that the Romans established formal treaties with states along the Illyrian coast in this early period, but then posits that the references to 'friendship' which we find in the sources (*amicitia*; *philia*) are 'informal alliances'. And D. means by this not *de facto* supportive relations, but 'informal treaties of alliance' (30, cf. 50) — whatever they would be.

We find incoherence again in D.'s account of the Roman war against the Delmatae in 156–155 B.C. This conflict has been stressed by those scholars who see the Romans as pathologically aggressive, because Polybius declares that the main reason for this war was to exercise the Roman armies, which the Senate felt had become debilitated by thirteen years of peace since the conquest of Macedon. Of course, if the Romans were pathologically aggressive, one has to wonder why there were thirteen years of peace in the first place. But as D. rightly points out, Polybius acted as an advocate of his friends the Locrians of southern Italy in an effort to get them excused from contributing fighting-ships for this war (Polyb. 12.5.1–3), so there is no need to take his evaluation of the worth of the conflict at face value. D. sees the Delmatae as formidable opponents (63, cf. 67), who had caused much trouble with raiding, had murdered envoys sent by local allies of Rome to remonstrate with them, and had then insulted the Roman envoys who were sent to do the same (Polyb. 32.9). So the war looks to be more than an excuse to exercise the armies, and indeed it was no promenade: the Delmatians defeated the consul C. Marcius Figulus in 156 B.C., and it required another year's campaigning before they were brought to heel. So far so good — but then D. states that Roman action was 'relatively swift and efficient' and 'fully confirmed Roman hegemony' (63). This looks like a version of Polybius' evaluation, then — but D. later shows that the Delmatic tribal coalition re-emerged to cause severe trouble in the first century, including the defeat of several Roman armies (92, 94).

The confusing discussion of the Dalmatian War occurs within a general discussion of the century between 167 and 59 B.C., in which D. says that for Rome the Illyrian area was only 'peripheral to their

interests' (62), and yet Rome saw the area 'as its zone of interest' (ibid.). In this period the Romans sought to 'exercise control' without 'direct trans-Adriatic rule' (74), while the region was 'left under the control of Rome's allies' (ibid.), but Rome exercised 'a loose, indirect control' (79). What is the meaning of 'control' here, and what would 'loose, indirect control' mean, and how exactly would it work? In fact, as D. shows, Rome was only interested in protecting its few friends along the Adriatic coast from raiding by the peoples of the hinterland; military action was rare, and occurred for the limited purpose of restraining and punishing those raids. That is quite a different thing from 'control', and even from 'loose, indirect control'. For a later period, D. argues first that 'Illyricum' in the 50s B.C. was an appendage of Gallia Cisalpina under Caesar's special command created in 59 B.C. by the Lex Vatinia (81–2); then that Illyricum in 59 B.C. was a truly separate province created *de iure* by the Lex Vatinia (98, cf. 179); and then that Illyricum was established as a regular province only sometime between 32 and 27 B.C. (118). It is hard to see how these statements can all simultaneously be true. Whatever a 'province' is, the fact is that from 230 B.C. down at least through the 30s B.C. there was no lasting commitment of Roman troops to this region. And it may even be (123–4) that there were no permanent Roman troops in Illyricum before A.D. 6.

According to D., however, a fundamental change of policy appears in the mid-30s B.C. with Octavian's two large campaigns to extend Roman influence far beyond the coast. The fighting reached through the Ljubljana Gap and well beyond modern Zagreb, extending Roman power towards the Danube. But was this merely a change in the scale of intervention, not the aim? Octavian's justification to the Senate for these operations was increased raiding by inland barbarians and to take revenge on the Delmatae for previous Roman defeats (in the 40s). Revenge for previous defeats was a powerful motif at Rome, as Riggsby has recently shown (*Caesar in Gaul and Rome: War in Words* (2006)). D. heartily agrees (103; cf. 180), but then claims that in the Roman view Octavian's operations were a preventive attack concerning the threat from Dacia (ibid.) — which is hardly Riggsby's point. D. then emphasizes that there was plenty of immediate provocation by the Illyrian tribes (104), 'the crisis of 35 B.C.' (105), a military crisis so serious that Octavian changed his plans for the year, which were originally in Africa (ibid.). If so, this would indicate that Octavian was not engaged in a preventive attack. Yet later (106), D. returns to the position that the aim of these campaigns was to create a buffer zone to protect northern Italy from a threat from Dacia — which would indicate that Octavian was engaged in a preventive attack after all. But later still (114), D. asserts that this campaign was a response to a serious crisis 'rather than some preconceived plan'. Octavian's campaigns, according to D., show a fundamental change in Roman policy, from merely repulsing the raids against the coastal towns to punishing and absorbing the inland peoples (115); yet he then says it was not a real annexation (ibid.), and later (125) that regional strategic thinking in this period remained deeply Republican, concerned with warding off occasional raids down to the coast while avoiding a commitment in the difficult and mountainous hinterland. One of these positions is correct; but it is hard to see how both can be. The latter one seems more correct to me.

It is only forty years later, with the Great 'Pannonian' Revolt of A.D. 6–9, that one begins to see a real Roman clamping down on the inland tribes. In A.D. 6 there was still no Roman road-building in the region and the Roman inland headquarters at Siscia south-east of Zagreb was a mere timber fort (154). But Pliny the Elder speaks of the greed of Roman *negotiatores* and *pubicani* (tax-collectors) as the source of the rebellion (*HN* 7.149); D. (143) quotes Dyson: 'The province was undergoing Romanization and the interior regions were getting the first real sense of what Roman conquest meant for native customs and power structure' ((1971), 253). The revolt is thus depicted as a typical indigenous response to the start of pressures for rapid economic and cultural Romanization (180). Yet D. elsewhere indicates that there was little Roman cultural penetration of the hinterlands even a half-century after this war (174). Conscriptio for the vast campaign against the Marcomanni across the Middle Danube, planned by Augustus, may have been the final spark (144). At any rate, the huge rebellion reached from Apollonia to Sirmium, and took four years to put down. It wrecked the plans of Augustus to remove the threat of the Marcomanni from the Upper Danube, because Illyricum was to be the eastern base for these operations; the disruption demonstrated that the region had finally achieved a strategic significance. Velleius Paterculus claims that at one point Augustus even feared a massive barbarian invasion of Italy (2.110–11). D. accepts that this was a real threat (154) — or, maybe not (148–9).

One result of the failed rebellion of A.D. 6–9 was that the region was henceforth divided for administrative efficiency into two separate provinces (Pannonia and Dalmatia), instead of one

large one (Illyricum). Three legions garrisoned Pannonia (the ‘frontier’ province on the Hungarian Plain), and two more were stationed in Dalmatia, in reserve and to watch the natives (155). Only in the next decades can the full panoply of Roman provincialization be finally seen. The division of Illyricum separated the ethnic Pannonii (whoever they were) into two different administrations, and D. argues that this was intentional, to avoid a repetition of the unified Pannonian effort of A.D. 6–9 (160, cf. 181–2). Or was it simply that the Dinaric Mountains formed an obvious and defensible boundary between regions (161)? Extensive Roman road-building finally began in the hinterland after A.D. 10; this was important for military movement but also for a developing commercial network, including gold-mining in the Dinaric Alps (173). There was the beginning of selective distribution of Roman citizenship to co-operative members of the local élite, and a clear transition towards Greco-Roman urbanism: Salona and Narona became bustling (and self-governing) towns. D. does not skimp on the uglier side of the process: we also find forced resettlement of troublesome indigenous groups, forced removal of their youth through conscription into Roman auxiliary units, direct military administration of the more restless *civitates*, and numerous Roman fortifications in stone. There was a relatively large immigration of foreigners, too (including veteran settlements) — from the point of view of post-colonial theorists a dark development indeed.

D. rightly emphasizes (180) that we only have the Romans’ own imperialist discourse on the conquest of Illyria; we do not possess the narrative of the conquered (their victimhood, the great suffering caused by Roman armies, the loss of what were perceived as ancestral freedoms). But while the coastal towns became prosperous Greco-Roman cities, D. is unclear about how soon or how far the indigenous cultures in the hinterland were affected. Regarding the hinterland, he says on the one hand that increased trade ‘hastened [its] incorporation into the wider imperial macro-economic system’ (174) — yet he also indicates that up through the mid-first century cultural change is not very visible archaeologically (*ibid.*). He states that the Roman road network rapidly transformed the interior (182) — but then admits that the interior lacked urban units until the second or even the third century (182–3). Again, I do not see how one can have it both ways. But on the whole it looks as if for the people behind the coast incorporation into the wider imperial economic and cultural matrix came late. And this is because, despite D.’s assertion that Illyricum became ‘a crown jewel in imperial geopolitical structure’ (177), the region even in the second century A.D. was pretty much a backwater.

This book needed a much better copy-editor. Even the title is misleading: it is not about the rôle of Illyricum in Roman politics, but about the development of Roman policy (such as it was) in Illyricum. D.’s sometimes eccentric English has not been corrected, and it can result not merely in awkwardness but in unintentional historical errors, such as his (inadvertent) placement of the Scordisci at the mouth of the Danube (35), when he means where the Sava and the Danube meet. And D.’s Latin is sometimes eccentric as well: ‘Caius and Lucius Coruncanii’ (48). Finally, it is not acceptable that in a book published by Cambridge University Press we find consistent reference to the work on Polybius of ‘F. W. Wallbank’ (including in the bibliography).

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J. RICHARDSON, *THE LANGUAGE OF EMPIRE: ROME AND THE IDEA OF EMPIRE FROM THE THIRD CENTURY B.C. TO THE SECOND CENTURY A.D.* Cambridge: Cambridge University Press, 2008. Pp. ix + 220. ISBN 9780521815017. £53.00/US\$99.00.

In this book Richardson takes us on a wonderful journey through the ways in which the Romans conceptualized the creation of their empire from the third century B.C. to the second century A.D. He does so by carrying out a judicious and highly calibrated linguistic analysis of the words *imperium* and *provincia*. At the time of the Hannibalic War, *imperium* meant an order, as well as the power to issue orders held either by a magistrate, a pro-magistrate, or by the Roman people, while *provincia* meant the sphere of influence within which the magistrate could exercise his *imperium*. In the course of the four centuries taken into account, *imperium* came to signify an entity limited by geographical boundaries under a unifying authority and constituted by territorial units, the *provinciae*. Key steps in the transformation of what R. vividly calls the biographies of these terms were the activities of Pompey in the 60s B.C. and Augustus’ rise to power. At the