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## REVIEW ARTICLE

### Middle Byzantine Numismatics in the Light of Franz Füeg's Corpora of Nomismata\*

JONATHAN JARRETT

FRANZ FÜEG, *Corpus of the Nomismata from Anastasius II to John I in Constantinople 713–976: Structure of the Issues; Corpus of Coin Finds; Contribution to the Iconographic and Monetary History*, trans. H. Thomas Hofmänner, ed. Italo Vecchi (Lancaster, PA, 2007).

FRANZ FÜEG, *Corpus of the Nomismata from Basil II to Eudocia 976–1067: Corpus from Anastasius II to John I 713–976 with Addenda; Structure of the Issues 976–1067; The Concave/Convex Histamena; Contribution to the Iconographic and Monetary History*, trans. H. Thomas Hofmänner, ed. Italo Vecchi (Lancaster, PA, 2014).

#### The State of the Field

Few large scale works have been published in the field of Byzantine numismatics since a number of monographs in the 1980s.<sup>1</sup> Research on Byzantine coinage has continued to accumulate, of course, and that time has seen the completion of the

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\* This review was entrusted to the author when he was Interim Curator of Coins at the Barber Institute of Fine Arts in the University of Birmingham between 2014 and 2015. The pressures of that job and of the author's subsequent return to full-time academe need not concern the reader but they are his only excuse for the very late appearance of the final review-article. The author owes considerable thanks to the tolerance and care of the editors of the *Chronicle* and also to Dr Rebecca Darley and Maria Vrij, my successor in post at the Barber, for useful discussions and guidance in a field that was new to me not so long ago. I must also thank my pupils on HIST2006 *Money and Power in Europe 284–1100* at the University of Leeds; nothing exposes inadequate answers like testing them on an untrained audience.

<sup>1</sup> Most obviously P. Grierson, *Byzantine Coins* (London, 1982), and M.F. Hendy, *Studies in the Byzantine Monetary Economy* (Cambridge, 1985); one could also mention D.M. Metcalf, *Coinage in South-Eastern Europe, 820–1396*, RNS SP 11 (London, 1979), despite its mixed reception, and T. Bertelè, *Numismatique byzantine*, trans. and rev. by C. Morrisson (Wetteren, 1978).

monumental Dumbarton Oaks Catalogue as well as innumerable shorter studies.<sup>2</sup> Yet there are some major issues where opinion is divided. Was the copper-alloy coinage of the empire valued by weight or by tale,<sup>3</sup> what control did the imperial capital seek, to exercise over regional mints, and how far was it able to do so,<sup>4</sup> and to what degree was the Byzantine economy monetised (in which is implicit the size of the Byzantine

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<sup>2</sup> Of course, DOC is only one of several catalogue publications which present numerous insights in their own right, e.g. MIB and C. Morriſson, *Catalogue des monnaies byzantines de la Bibliothèque Nationale* (Paris, 1970). Morriſson's voluminous other work can now be accessed via C. Morriſson, *Monnaie et finances à Byzance: analyses, techniques*, Variorum Collected Studies Series 461 (Aldershot, 1994), Morriſson, 'Byzantine Money: Its Production and Circulation', trans. S.H. Tenison, in Angeliki E. Laiou (ed.), *The Economic History of Byzantium from the Seventh Through the Fifteenth Century*, DOS 39 (Washington DC, 2002), vol. 3, pp. 909–66 and Morriſson, 'Précis de numismatique byzantine' in C. Morriſson, G. Schaaf and J.-M. Spieser, *Byzance et sa monnaie, IV<sup>e</sup>-XV<sup>e</sup> siècle: précis de numismatique byzantine. Catalogue de la collection Lampart à l'Université de Fribourg*, Réalités Byzantines 15 (Paris, 2015). To all this one must add Morriſson's collaborative work in the analysis of Byzantine coins' metal content, most obviously C. Morriſson, J.-N. Barrandon and Jacques Poirier, 'La monnaie d'or byzantine à Constantinople purification et modes d'altérations (491–1354)', in C. Morriſson, C. Brenot, J.-P. Callu, J.-N. Barrandon, J. Poirier and R. Halleux, *L'or monnayé I: purification et altérations de Rome à Byzance*, Cahiers Ernest Babelon 2 (Paris, 1985), pp. 113–87, which effectively replaces her earlier work in this area.

<sup>3</sup> Compare Hendy, *Studies*, pp. 496–500, with C. Morriſson, 'La monnaie fiduciaire à Byzance, ou «Vraie monnaie», «monnaie fiduciaire» et «fausse monnaie» à Byzance', *BSFN* 34 (1979), pp. 612–16. Grierson, *Byzantine Coins*, pp. 46–7, was caught between the two poles: 'Even though the copper was of a token character it is inconceivable that Anastasius's small folles can have had the same value in relation to the solidus as his large ones, although both are marked as worth 40 nummi...'. Morriſson, 'Précis', p. 76, adopts the ambiguous description, 'légèrement fiduciaire'. This question is not pursued further here, but A. Gândila, 'Heavy money, weightier problems: The Justinianic reform of 538 and its economic consequences', *RN* 168–9 (2012), pp. 363–402, may open a way through the debate.

<sup>4</sup> The best example of unified monetary change across the empire may be Justinian's reform of the follis (see p. 3 below and n. 8), but on the other hand cf. Grierson, *Byzantine Coins*, pp. 67–9, for the imperfect implementation of this and other reforms at Antioch and Alexandria, and *ibid.*, pp. 165–71, for the increasing non-conformity of Sicilian and Italian mints over the ninth century, to say nothing of Cherson. See also Morriſson, 'Nouvelles recherches sur l'histoire monétaire byzantine: évolution comparée de la monnaie d'or à Constantinople et dans les provinces d'Afrique et de Sicile', *Jahrbuch der österreichischen Byzantinistik* 33 (1983), pp. 267–86, repr. in Morriſson, *Monnaie*, no. X.

coinage)?<sup>5</sup> There are also a number of specifically Byzantinist versions of areas of general numismatic argument, such as the respective authorial initiatives of die-cutters, their supervisors or the ruler in the design of coinage, its use as a medium for political and religious messages and the degree to which such messages could be understood by the coinage's user-base.<sup>6</sup> There are also more specialist questions with only partially-accepted answers, such as the purpose of the gold tetarteron,<sup>7</sup> why the copper-alloy coin bore regnal dates for a while and then stopped,<sup>8</sup> why Byzantine

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<sup>5</sup> Cf. Morriison, 'Byzantine money', pp. 946–50 and N. Oikonomides, 'The Role of the State in the Byzantine Economy', in Laiou, *Economic History of Byzantium*, vol. 3, pp. 973–1058 at pp. 978–80.

<sup>6</sup> The initiative of die-cutters and the use of coins as 'propaganda' are addressed at pp. 22–24 and 18–20 below. On the question of audience see p. 20 below and also M. Crawford, 'Roman Imperial Coin Types and the Formation of Public Opinion', in C. Brooke et al. (eds), *Studies in Numismatic Method Presented to Philip Grierson* (Cambridge, 1983), pp. 47–64.

<sup>7</sup> M.F. Hendy, 'Light weight solidi, tetartera, and the Book of the Prefect', *Byzantinische Zeitschrift*, 65 (1972), pp. 57–80, repr. in Hendy, *The Economy, Fiscal Administration and Coinage of Byzantium, Variorum Collected Studies 305* (Northampton, 1989), no. IX, does not really explain the apparent existence of two or three standards of low-weight coin. Morriison, 'Précis', p. 75, seems briefly to argue that the tetarteron was a failed experiment, despite its long run, and that it was not the same thing as light-weight solidi although they were used in the same way. Füeg, *Corpus 976–1067*, p. 13, sets out the weights of tetartera as opposed to histamena but then finds (*ibid.*, p. 17) that the weights overlap. By *ibid.* p. 53 it is clear that he is in fact distinguishing types iconographically whose weight and fineness are not meaningfully different, and also vice versa. Was the state so fiendish in rendering uncertain the value of its own coinage, or were tetartera really just a different style of nomisma?

<sup>8</sup> The conventional explanation is that the coins were in some sense state documents and thus fell under Justinian I's Novel requiring those to be dated by regnal year, consulate, indiction, month and day (Hendy, *Studies*, pp. 499–500, with the clause in translation; cf. Morriison, 'Précis', p. 21). The chronology of this works, with the Novel being issued in 537 and the first dated coins appearing in 537/8, but dates were applied only to the copper-alloy, not the gold that might be thought more obviously an imperial concern (as in M.F. Hendy, 'East and West: Divergent models of coinage and its use', in *Il Secolo di ferro: Mito e realtà del secolo X, Settimane di Studio del Centro Italiano di Studi sull'Alto Medioevo 38*, (Spoleto, 1991), vol. 2, pp. 637–78 at pp. 651–2), and even the base-metal coins bore only some of the information required by the law. Against the former objection Hendy, *Studies*, p. 500, argued that the fiduciary nature of base-metal coin (cf. n. 3 above) made it more like a document, an intriguing idea—the coins were cheques?—but unsubstantiated. The latter difficulty has not been acknowledged anywhere that I can find. The edict, of course, does not specify its own application to coin, but no better explanation has yet emerged.

mints often overstruck copper-alloy coins (but not always),<sup>9</sup> the priority of efficiency at a Byzantine mint, and why the eleventh-to-thirteenth-century coinage was concave in shape.<sup>10</sup> Doubtless every specialist could add further favourite queries.

The above is an inadequate summary of where research stands, but it remains true that for a while no grand synthesis has attempted to resolve many of these questions or present a new view of what one can only call, with Hendy, the Byzantine monetary economy. It is onto this scene that the two volumes under review emerge.<sup>11</sup> Although they make no claim to be such a synthesis, still they are the most substantial presentation of data on the Constantinopolitan gold coinage for a long time, perhaps indeed ever, and they enable new approaches. This review's purpose, after giving an account of the two books and their value, is to identify and extend several of the points where Füeg's new work bears on the questions above, and often demands their re-evaluation. In particular, I focus on the question of coins as a means of political transmission, on the inner organisation of the Byzantine mint and its implications for the state's deployment of money and on the arguments over the reasons for concave fabric in those Byzantine coins that display it. My overall argument is that many of our current answers to questions like these are discomfited by the data that Füeg's work presents, and that it is time at last to seek a new synthetic view of Byzantine coinage.

### *The Books' Achievements*

Füeg's two volumes, most of all, are a thorough and painstaking study of the Byzantine imperial gold coinage from the early eighth century to the mid-eleventh,

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<sup>9</sup> On overstriking, cf. Grierson, *Byzantine Coins*, pp. 45 and 92, offering no explicit explanation but associating it with cost-cutting and 'slovenly' mint practice, and Hendy, *Studies*, p. 288, seeing it as marking of copper-alloy coins as they passed through the mint; but why was this practice, rather than the two alternatives he delineates, sometimes preferred? If labour or cost was the problem, why not just recirculate the coin as was? Or, if the imperial image was so important to strike upon coin, why do so in so slapdash a way? See also Hendy, 'East and West', pp. 665–9, for the chronology of the phenomenon.

<sup>10</sup> These last two concerns are dealt with at pp. 27–30 below.

<sup>11</sup> Hereafter I make reference to these volumes as 'F2007' (Füeg, *Corpus of the Nomismata from Anastasius II to John I*) and 'F2014' (Füeg, *Corpus of the Nomismata from Basil II to Eudocia*).

with some excursions into other and later coinages. Their core is the presentation of a corpus of 15,474 nomismata and 899 other coins, of which total 602 are illustrated. A substantial proportion of this immense body of material is used in a die study that presents a wide range of conclusions in iconographic, economic and other spheres.<sup>12</sup> The material involved came from not just the major international collections such as Dumbarton Oaks, the Bibliothèque nationale de France, the American Numismatic Society and the Hermitage but also a range of others, many unpublished, across Europe and North America. Even if, as Füeg admits, he has often had to work from photographs rather than the coins themselves (as anyone doing a die study using multiple collections must), no-one has ever produced such a large Byzantine numismatic study corpus of this period before. Füeg deserves recognition for the sheer scale of effort and dedication involved in its production. While the data is not uniform in composition, on which more below, to have not just assembled this information but also made it intelligible is a major achievement. Just to have reasonable figures for the relative survival rates of each coin type is new and important. Füeg's statistical tables of the various issues under examination, moreover, make it possible to check and indeed contradict his conclusions and may be the foundation of many new studies. The illustrated 'Catalogue of the Issues' in each volume is clearly laid out and easily usable, making it possible to look things up in these books that were previously hard to find out.

Special praise is also merited by the presentation of the Corpus in the first of the two volumes.<sup>13</sup> It manages to display die-links graphically over a long series of issues in a way that renders it possible and, before long, intuitive to trace die links easily and quickly between coins, economically but thoroughly referenced, all within the constraints of an A4 page (see fig. 1). This is no mean feat, and while the author

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<sup>12</sup> The size of the sample for Füeg's die study is unclear. F2007, p. 158 gives us: 'Of the 7780 examples in the Corpus, less than 3000 were published and accessible in public collections by 1980. Consequently, less than 40% of coins were available for examination. They allow gaining only about 15% for both side [sic] – and about 30% for one side....'. F2014, p. 126 adds 1002 coins to the coverage for 713–976 but there is no indication of sample size for the die study from 976–1067.

<sup>13</sup> F2007, pp. 201–380. This Corpus is added to and extended by that in the second volume, but on the difficulties with that see pp. xx–xx below.

thanks his son Thomas for the technical work, it is clearly a numismatist's labour, and one worthy of emulation.

**Leon III. und Konstantin 5 (724-731)**

5	Die Ikonographie wie jene der Serie 3:	Die Ikonographie wie jene der Serie 4, aber ohne 'Offizinzeichen':	Von gleichen Stempeln: * Avers o Revers
	JNDLEO NPAMYL	JNCONST ANZINHS	
5.A.1	4L*	4SM	BMC 13 Petersburg (3062) = To 59 DOC 4f Frankfurt (151/06) = Kre 135,1966,936 Perpignan (1260) Kre 129,1964,1052 Hir 79,1972,1435 Hir 85,1973,4758 Ste, 28/11/1975,685 = Kas 10,1976,359 SBV 5,1979,566 Ari, 9/12/1981,502 = NCirc 1992,3400 Ste 12,1982,934 Mal 39,1989,609 Peu 330,1991,516 SoV, 21/6/1991,526 <sup>2</sup> = Bai 15,1994,39 Mor, 28/2/1992,625 Aus einem Fund (Leu,1993,2) = Els 48, 1997,745 = 51,1997,587 Ber 83,1994,40 Leu 65,1996,604 = Els 47,1996,831 = 63, 2000,1203 Spl 121,1997,518 = 125,1998,231 Bou, 2/1997,36 Ber 100,1998,36A Sup, 31/5/1999,1229 M&M 605,2000,127 = MMD 8,2001,480 Ari, 13/4/2005,261

Fig. 1. Leo III and Constantine issue 5.A.1 in F2007, p. 214, die-links at right

Further contributions of this kind come in the iconographical sphere. Füeg's schematic tables of imperial portrayal on nomismata will be invaluable for future students of such matters. There are also important insights of interpretation. The greatest of these is probably the demolition of any possibility that the Byzantine controversy over icons can be found reflected in the coinage, where supposedly iconoclast and iconodule rulers are not differentiated from each other or their predecessors. This has needed saying more clearly for some time and with this kind of data behind it, it is no longer deniable.<sup>14</sup> Then, in the second volume, Füeg is able to point out two series of coins of Andronicus I Gidon of Trebizond (1222–35) which unexpectedly but undeniably depict the Chalkê icon of Christ in Constantinople, naming the image as O XAAKHTHC.<sup>15</sup> This is a matter of note mainly because, while

<sup>14</sup> F2007, pp. 131–34, largely following P. Speck, 'Bilder und Bilderstrie: Byzanz', in M. Brandt and A. Effenberger (eds), *Die Macht der Bilder* (Hildesheim, 1998), pp. 56–67. F2014, p. 98, however, retreats to the older position.

<sup>15</sup> F2014, pp. 86–7.

it is often asserted that coin designs were based on icons, the icons in question almost never survive, and just as rarely do the coins actually identify their prototype in so unambiguous a fashion.<sup>16</sup> An exactly contemporary series of John Vatatzes at Magnesia which also names the Chalkê icon (DOC IV John III 21) does not, however, use a distinct iconography to do so. Thus it is the Trebizond coin which makes it clear that other coins for which the Chalkê has been suggested as a prototype do in fact resemble this one in some characteristic details of attitude and costume, the most notable resemblance being to a type of the Empress Theodora's which Füeg also studies.<sup>17</sup> This is not only a significant numismatic insight but also a valuable contribution to the wider debate in art history about intermedial imitation.<sup>18</sup> It is obviously also probably significant that these two coinages belonged to exilic imperial claimants: their publics, unlike the users of the Constantinopolitan issues depicting the icon, presumably needed the reference to the lost capital made manifest. This has something to tell us about the way in which Constantinople remained active, but apparently unfamiliar, in the Byzantine imagination between 1204 and 1261.

### Presentation and Selection

As well as a useful illustrated catalogue, Füeg presents accessible summaries of the state of our knowledge on, for example, the manufacture of Byzantine gold coin and its use by the state.<sup>19</sup> It is, therefore, likely that these two volumes will find a grateful

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<sup>16</sup> A list of possible prototypes is given in P. Grierson, *Byzantine Coinage*, 2nd ed. (Washington DC, 1999) <<https://www.doaks.org/research/publications/books/byzantine-coinage>> [last modified 19 June 2017 as of 05 October 2017], pp. 34–5; F2014, p. 9 adds more. Grierson, *Byzantine Coins*, p. 203, has one other instance of a named prototype, the Virgin of Blachernae, named as ἡ Βλαχερνίτισα on two-third-miliaresia of Constantine IX, Theodora and Michael.

<sup>17</sup> F2014, pp. 73 and 101; cf. Grierson, *Byzantine Coins*, pp. 97–8, arguing for the Chalkê icon as the prototype for Justinian II's first Christ solidus. The icon may however not have existed in Justinian II's time: L. Brubaker and J. Haldon, *Byzantium in the Iconoclast Era, c.680–850: a History* (Cambridge, 2011), pp. 128–35.

<sup>18</sup> A substantial recent contribution in this debate is G. Rippl (ed.), *Handbook of Intermediality: Literature – Image – Sound – Music* (Berlin, 2015).

<sup>19</sup> Esp. F2014, pp. 125–34, based explicitly upon C. Morrisson, 'Monnaie et finances dans l'Empire byzantin, X<sup>e</sup>–XIV<sup>e</sup> siècle' in V. Kravari, J. Lefort & C. Morrisson (eds), *Hommes et richesses dans l'Empire byzantine II : VIII<sup>e</sup>–XV<sup>e</sup> siècles, Réalités Byzantines 3* (Paris, 1991), pp. 291–315 (repr. in



user base which will be brought up to date with a sometimes inaccessible historiography. Unfortunately, along with these benefits come substantial oddities and complications in the works' conception and arrangement and numerous questionable details, some of which threaten the whole work's coherence.

One should begin with a separation of the problems due to the publisher (the Classical Numismatic Group) and its editors and those that must be credited to the author. Füg's text began in German, and parts remain there, most obviously the Corpora (see below) but also, less intentionally, occasional headings.<sup>20</sup> The translation, by H. Thomas Hofmänner, has not always helped Füg's prose: phrases such as 'These are features of an economic system that preclude a well-organized monetary policy', where Füg apparently means rather that they demonstrate one, or 'the Caliph's troupes' are only the most egregious examples.<sup>21</sup> The physical quality of the volumes and the quality of illustration is high, but such care was not maintained in the editorial process. The volumes' bibliographies are especially troublesome: one is presented with page numbers, issue numbers and volume numbers that are indistinguishable; journals are not distinguished from edited collections and items are sometimes listed out of order. Tracking anything down with this becomes difficult if one has not found it before.

In some cases, the choices of presentation are simply bewildering. The core of the volumes is the Corpus of coins, but there are actually three corpora in the first volume and two in the second. Both volumes contain both a Catalogue of the Issues and the actual Corpus, and the first (only) also contains a Corpus of Coin Finds.<sup>22</sup> In that, however, there are entire collections included that are not in the principal Corpus, while some collections used in the Corpus proper do not appear, even though

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Morrisson, *Monnaie*, no. IV), Morisson, 'Byzantine Money', and Oikonomides, 'Role of the Byzantine State', but considerably shorter.

<sup>20</sup> E.g. F2014, pp. 30 and 76.

<sup>21</sup> F2007, pp. 151 and 152.

<sup>22</sup> Catalogues: F2007, pp. 45–102; F2014, pp. 37–91. Corpora: F2007, pp. 201–380; F2014, pp. 201–558. (F2014 appears to have no pp. 162–200.) Corpus of Coin Finds: F2007, pp. 101–50.

some of them certainly include coins with find locations.<sup>23</sup> Not all the coins in the Corpus of Coin Finds even have find spots recorded. There is no cross-referencing between the Corpora. Most peculiarly of all, as well as remaining in German, the Corpora proper are presented not in print, but on CD-ROM (although their lists of contents are given in the print volumes, not on the discs). The digital medium is barely exploited. On the discs are only flat files intended for printout, readable at A4 but optimised for A3, in the first volume in PDF and in Microsoft Word format in the second. This latter is an unfortunate choice, as it requires not only the requisite proprietary software but also the unspecified font in which the coin legends are set: without it, they appear as a meaningless array of substitute characters. The clarity of the die-link notation is also lost in this format, removing most of its value. In any case, the choice of hard digital media to save money when fewer and fewer computers carry CD-ROM drives, for what is essentially a print document, will serve increasingly few readers whatever the file format. The Catalogues of Coin Finds, Corpora and Corpus of Coin Finds should really have been unified, translated and presented in print, in which state they would have remained usable and useful for many years to come.

The value of the Corpora is weakened when one understands that their two parts were not assembled in the same way. Whereas the 2007 volume rests on two decades of travelling to far flung coin collections and assembling photographs, for the latter volume, 'examples have been compiled with less intensity than for the Corpus [covering the years] 713 to 976. Most examples cited are from trade publications and catalogues and only very few from museum collections.'<sup>24</sup> This important information is not presented between the volume's covers, or even on the CD-ROM, but on the jacket flap. Yet it more or less precludes using the two parts of the corpora together for statistical purposes: the samples are of different sizes and have been selected differently not just by the author but by the different priorities of the museums and

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<sup>23</sup> In the Corpus of Coin Finds but not the main Corpus, for example, Pliska and Tirane (F2007, pp. 107 and 109). The Corpus of Coin Finds seems, however, to omit all American, French and British collections.

<sup>24</sup> F2007, p. 4, for the duration of the project. The photographs, we are told, are on deposit at Dumbarton Oaks (F2007, p. 201; F2014, p. 10). One can only wish that they, rather than the Corpus, had been digitised and presented on CD-ROM.

dealers behind them. But one cannot work with the first volume alone, because the second adds further information, collected in the new fashion, to a revised version of the first one's Corpus. A methodologically sound way to use this data is thus hard to identify.

Larger questions of aim and selection may be asked, too. Why, for example, start with Anastasius II (713–15)? No explanation is provided, but it seems especially odd given the overlap with volume 3 of Hahn's *Monete Imperiali Bizantini*, which ends in 741. The cut-off date at 1067 is no more obvious, not least because Füeg goes on to look at four isolated bodies of coins beyond it, some of which are silver and two of which are non-Constantinopolitan.<sup>25</sup> Likewise, why (apart, of course, from those exceptions) do we deal only with full nomismata of Constantinople?<sup>26</sup> The iconography of fractional gold denominations was frequently different from that of the full nomisma, and regional mints, especially Syracuse, sometimes pursued quite different policies of design, weight and even fineness compared to the capital.<sup>27</sup> At times these omissions invalidate Füeg's deductions based on the capital's nomismata alone: for example, an argument that the use of a cross pattée in the representation of Michael III (840–67) and a cross pommée for Theophilus (829–42) on their joint coinage of 840 (as Füeg dates it, on which more below) is an inversion of the usual priority of these insignia, intended to privilege Michael, is undone by the fact that on their semisses the two emperors' crosses are the other way about, as Füeg himself mentions.<sup>28</sup>

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<sup>25</sup> The only explanation provided for this is that the coins are iconographically interesting (F2014, p. 34), which while true (and enabling the findings of pp. 6–7 above) begs the question of the actual purpose of these books.

<sup>26</sup> Again, the exclusion is somewhat theoretical, as 98 miliaresia of Basil II, Romanus III and Constantine X are also included in the Catalogue and Corpus, without explanation.

<sup>27</sup> See n. 4 above, but also M.F. Hendy, 'On the administrative basis of the Byzantine coinage c.400–c.900 and the reforms of Heraclius', *University of Birmingham Historical Journal* 12 (1970), pp. 129–54, repr. in Hendy, *Economy, Fiscal Administration and Coinage*, no. VIII, at pp. 145–7; and also DOC 3.1 pp. 82–94.

<sup>28</sup> F2007, p. 28, where the iconographical terms (but only these, and only here) are given in French.

## Iconographic Interpretation

Observation this keen could, all the same, yield new findings. Füeg's iconographic descriptions are clear, especially compared to the telegraphic notation of the standard Byzantine catalogues. Furthermore, his Catalogues and the tables of iconographical details usually make very obvious the details he perceives. There are exceptions: for example, his Leo VI varieties 3.C and 3.D (the latter of which he tells us 'diverges more or less strongly from the original design', without saying how) appear indistinguishable in iconography or style to me. Dots that are supposed to distinguish his Basil II (976–1025) and Constantine VIII (1025–8) varieties 3.H and 3.I are not visible to me in the illustrations; and his Michael IV (1034–41) Histamenon 1.A.1 and 1.B.1 are identical, both to my eyes and in Füeg's description.<sup>29</sup> But then, after Füeg has told the reader, apparently in all seriousness, that one perceives a coin differently according to one's state of sin, even today, how can one hope to see as he sees?<sup>30</sup> Even with this eternal consideration applied, it is arguable that Füeg over-reads his iconography: few would agree with him, for example, about 'differing ends to isosceles crosses which have followed a hierarchical system since the fifth century, or the trefoil, which has been added to the coins as a symbol of Trinity starting in the middle of the fifth century', or the control marks on coins of the Isaurian emperors all being Christological in significance except only two of Constantine V's (741–75) that instead noted the months of his reign.<sup>31</sup> Unfortunately not all readers will realise that such interpretations are without foundation.

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<sup>29</sup> Respectively F2007, p. 32 (illustrated p. 79), F2014, p. 47, and F2014, p. 22 (illustrated p. 61). With this last, a note on p. 62 adds that Michael IV Histamenon 1.A.1 spells MIXAHA with Λ rather than the L of other varieties; but 1.A.1.z has the L, so it seems unlikely that this can be what distinguishes 1.A. issues from 1.B ones. There is no consistency about how variation is catalogued, either: Michael IV's issues are distinguished to such extents as 1.A, 1.A.1, 1.A.1.y, and 1.B.3<sup>1</sup>, but Basil II gets a 2.FF as well as 2.F but never a 2.F.1. Andronicus I's Hyperperon I, meanwhile, is sequenced in descending order from 1.9 to 1.1! There is no explanation given for these labels.

<sup>30</sup> F2007, p. 142: 'For many of this series' examples ... the words used by Patriarch Photius to characterize Christ on a mosaic hold true. 'His eyes only look angry to the sinner – rejecting and inaccessible. To those with a clear conscience, they look gentle and friendly.' Cf. *ibid.*, p. 145, of a later issue, 'Even to the sinner, the eyes no longer appear angry and rejecting.' I cannot see how these sentences are to be read except as referring to modern-day viewers of the coinage.

<sup>31</sup> Quote: F2007, p. 9; Constantine V: F2007, pp. 122–3.

## Chronology and Seriation

This kind of consideration is all the more worrying because so much of Füeg's work rests on his die studies. He gallantly admits the likelihood of errors, though since, as noted above, this reviewer sees fewer differences between the coins than does Füeg, it seems more likely that he has under-estimated rather than over-estimated similarity, and therefore die-links; whatever comfort this may be.<sup>32</sup> But having distinguished his coins, Füeg proceeds to seriation. In this, he makes a number of assumptions, not least that no two series were ever struck simultaneously. He is explicit about this, but he has at this point already concluded that Constantine VII (913–59) at least must have struck issues in his sole name alongside issues for himself and his son Romanus II (959–63).<sup>33</sup> Obviously, if it could happen once it could happen again, but with that admitted Füeg's seriations would disappear.

The bases on which Füeg is willing to seriate coins must in any case be questioned. The starting criterion, naturally, is rulers depicted or named, which is fair enough even if the Isaurian emperors give it trouble. Füeg reassigns several of their issues on the basis of other criteria, such as whether the emperor wears the chlamys or the loros, or whether he carries a patriarchal cross or a globus cruciger.<sup>34</sup> This can lead to special pleading. To have to say, 'Thus, the representation follows a forty-year-old tradition, even if it diverges from the rules', should surely force a consideration of whether there really were rules, not least because it is apparent that some emperors' die-cutters did not know them: Leo V (813–20) and Michael III both appear on their solidi in both chlamys and loros.<sup>35</sup> More worryingly, if one criterion

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<sup>32</sup> Admission of possible error: F2007, p. 165.

<sup>33</sup> F2007 p. 45 for the stated assumption; *ibid.* p. 39 for the analysis of Constantine VII's coinages. Inherent in the assumption is also that there was a meaningful difference between *histamena* and *tetartera*, but see n. 7 above for Füeg's difficulty in establishing this.

<sup>34</sup> He is, of course, not alone attributing significance and precedence to such details: see Grierson, *Byzantine Coins*, pp. 29–33, or C. Morrisson, 'Displaying the emperor's authority and *Kharaktèr* in the marketplace', in P. Armstrong (ed.), *Authority in Byzantium* (Farnham, 2013), pp. 65–80, among many other possible citations.

<sup>35</sup> F2007, p. 28 for quote, pp. 24 and 25 for Leo V and Michael III. Cf. Grierson, *Byzantine Coins*, p. 32: 'there are no good grounds for regarding one costume as superior to another'.

does not work, Füeg will use another, even if it contradicts earlier statements.<sup>36</sup> In fact, his iconographic tables make the inconsistency of such details perfectly clear, and the coinage of Syracuse would have amplified this had it been included.

At times Füeg's distinctions are purely stylistic and chronology is determined on the sole assumption of innovation followed by decline in quality.<sup>37</sup> A bad die-sinker replaced by a better one, or the alteration of a design, as Füeg admits happened in the reign of Basil II, would ruin this logic, but that is not considered.<sup>38</sup> He is not alone here, but his usage of a stylistic chronology goes to show the inherent weaknesses of applying a purely aesthetic analysis when unsupported by other criteria.<sup>39</sup> Meanwhile, issues are distinguished on the grounds of the infamously ambiguous Θ control mark, the number of pellets on a Gospel book, throne support or loros, or the number of waves in the emperor's hair.<sup>40</sup> Furthermore, Füeg believes that, 'the gold coins lack purely decorative elements; everything is part of a message'.<sup>41</sup> Thus, the number of waves in an emperor's hair is no mere engraver's whim: on the contrary, that Artavasdus (742–3) was portrayed with five waves in his

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<sup>36</sup> F2014, p. 158: 'The make-up of the issues' structure varies widely. For this reason the series does not always follow the same principle.' This warning arguably comes a little late.

<sup>37</sup> F2007, pp. 58, 61, 78–9 and 99; F2014, p. 22 and 87.

<sup>38</sup> F2007, p. 138, where it rather weakens the argument that nomismata of Basil II perhaps imitated a lost mosaic in the Great Palace, possibly influenced by a later mosaic in Thessalonica whose patron (of course, not the designer) knew someone (the Patriarch) who would have seen the Constantinopolitan icon. The change of details of the design suggests that one or other die had to be corrected in its replication of the mosaic, unless of course neither design was in fact copying a mosaic at all.

<sup>39</sup> Cf. D.S. Whitley and R.I. Dorn, 'Rock art chronology in Eastern California', *World Archaeology* 19 (1987), pp. 150–64. Perhaps unsurprisingly, it is easier to find specialists emphasising problems with scientific dating than to find them questioning humans' ability to guess dates on such criteria.

<sup>40</sup> Respectively F2007, p. 18, F2014, p. 68, *ibid.*, p. 76, *ibid.*, p. 85 and F2007, p. 16. It should in justice be noted that at F2007, p. 70 and F2014, p. 35 Füeg declares seriation of particular issues impossible. On Θ control marks, witness the different functions identified in DOC 2.1 pp. 111–23: part of a numerical sequence on Heraclius's solidi from all mints (p. 114), but specifically indictional dating on those of Carthage (p. 118); a mint-mark for Thessalonica on Constans II's and Constantine IV's hexagrams (p. 117); ubiquitous but unexplained on gold of Syracuse (pp. 118–20). Clearly, a user of the coin could not have extracted the correct meaning from a coin on a first encounter, which surely means that these were not signs intended to be read.

<sup>41</sup> F2007, p. 9.

hair (or six, apparently not a problem for Füeg's seriation), while his rival Constantine V had only three, is for Füeg deliberate one-upmanship by the insurgent emperor!<sup>42</sup>

Self-evidently there is no way in which such features can usefully be arranged in an order except for purely descriptive purposes. To suppose these minor details were consciously maintained requires us to imagine a cabal of die-cutters working in intimate proximity, informing each other of the slightest variations in their design, with records of those variations kept over decades. The obvious alternative, that such features were not agreed or perhaps even conscious and that one die-cutter might very well, for example, put first two, then three, then two pellets again on a staff depending on how his tools met the metal as he began each die, is equally impossible to disprove and easier to believe. But none of this would matter very much if Füeg did not then use it to impute dates to the coins, using not just this minute iconographic variation but also the mathematics of die counts. By this I do not mean currency volume estimates, although Füeg does this too, despite earnest acknowledgement of all the problems with such estimates.<sup>43</sup> Rather, I mean that he counts the numbers of obverse dies observed in all the coinages of each imperial reign, and then averages them to an annual rate of die replacement.<sup>44</sup> This figure is used to calculate the notional duration of each issue.

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<sup>42</sup> F2007, p. 16, hanging entirely on his belief that the issues were concurrent, on the weakness of which see pp. xx–xx below.

<sup>43</sup> F2007, pp. 154–65 and F2014, pp. 125–9, using the estimators provided in G.F. Carter, 'A simplified method for calculating the original number of dies from die link statistics', ANSMN 28 (1983), pp. 195–206 despite awareness of W.C. Esty, 'Estimation of the size of a coinage: a survey and comparison of methods', NC 146 (1986), pp. 185–215 (listed F2007, p. 176). Füeg strives, indeed, to verbalise all the weaknesses of such methods and the data upon which he deploys them, including (F2007, p. 161) the warnings of T.V. Buttrey, 'Calculating ancient coin production: facts and fantasies', NC 153 (1993), pp. 335–51; Buttrey, 'Calculating ancient coin production, II: why it cannot be done', NC 154 (1994), pp. 341–52 and S.E. Buttrey and T.V. Buttrey, 'Calculating ancient coin production, again', AJN 9 (1997), pp. 113–35, also appear in the Bibliography (F2007, p. 175). Buttrey even earns an acknowledgement for his critique (F2007, p. 4), but still it is not heeded.

<sup>44</sup> As close as we get to an explanation for this is given in F2007, p. 45: 'If historical data or plausible clues are missing, then the length of the issue period has been interpolated proportionally to the number of extrapolated obverse dies....', more or less repeated at F2014, p. 37.

The problems with this approach are obvious, even to Füeg, although they do not prevent him following it. In the first place, Füeg admits to some periods of increased issue for some emperors.<sup>45</sup> Likewise, he deduces that some emperors issued only intermittently.<sup>46</sup> But Füeg's mathematics rely on a steady and ceaseless output of coin that would, apparently, change immediately when a new emperor succeeded to exactly the rate he would need until he died. The artificiality of these premises is self-evident, but the dates are still given in the catalogue, even if in italics. Sometimes they are even used to make quite substantial adjustments to the coinage history. It is on the basis of such distinctions, for example, that he assigns a full seventh of the difficult Corpus of Coin Finds to the short reign of Romanus II rather than that ruler's co-reign with his father Constantine VII, or takes ten years off the date range of an intriguing find in the River Reno.<sup>47</sup>

However, such implications are also sometimes disregarded where they could have been indicative. Füeg dates an issue of coins of Constantine VI and Irene (780–97), on which the empress was depicted but not named, to the period of her political exclusion between late 790 and early 792.<sup>48</sup> Füeg knows of only two such coins that exist to fill that 14-month period, however, as opposed to 90 for the 36 months before and 155 for the 67 months from then to Constantine's blinding and death. It seems unlikely that these were the only coins of those years, if they belong then at all, and his overall chronology for the reign is therefore called into question.<sup>49</sup> Deductions like these can only be rejected, but finding all the incorrect ones is a lengthy endeavour for the reader.

### Obverse and Reverse

Whatever their problems, however, Füeg's deductions can expose problems in the wider assumptions of the discipline. To pick up threads already teased out, one may

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<sup>45</sup> F2014, pp. 129–32.

<sup>46</sup> F2007, pp. 18 and 174, in the first case because he observes too few dies to fill the years in question but in the second case because of several groups of coins of Alexander, Constantine VII and Romanus I with internal die-links but no links between groups, suggesting gaps between their production.

<sup>47</sup> F2007, pp. 91–7 and 112.

<sup>48</sup> F2007, pp. 20–1.

<sup>49</sup> F2007, p. 167.



see this happening when his die-arithmetic and iconography combine at the point of comparing counts of obverse and reverse dies. The distinction between obverse and reverse is assumed by many to be purely mechanical, that is the obverse die is the lower, anvil die. This goes with another assumption that the anvil die, further from the hammer, is the position of honour, so that the obverse is also always the side denoting the issuing authority.<sup>50</sup> Unfortunately, in the Byzantine world these two models can conflict: when Christ or the Virgin are on the coin with the emperor, is their side naturally then the obverse, even though the emperor's portrayal remains unchanged? What about when an emperor occupies each side? Great significance has been extracted from such considerations. Grierson spent several sentences of one monograph teasing out the changing balance of power between the Empress Irene and her son the Emperor Constantine VI based on how the run of the legend from side to side of the coin and the location of officina marks combined with the portraits on their joint coinage changed from obverse to reverse.<sup>51</sup> Füeg's die counts, however, allow him to distinguish obverse and reverse not just on the basis of design but on the basis that reverse dies wear out more quickly than obverse dies. Where an issue shows noticeably more frequent changes of die on one side than on the other, therefore, it is reasonable to suppose that the former side is what we might call the 'mechanical reverse'. Unfortunately this does not always correspond with the 'iconographic reverse'.<sup>52</sup> Thus, in his first volume Füeg notes four cases where obverse and reverse faces, as determined by iconography, are switched between striking positions.<sup>53</sup> Three more of these switches are noted in the second volume, where it is the Virgin who was moved from one face to the other.<sup>54</sup> This choice could apparently be very precise: on tetartera of Leo VI (886–912), Constantine IX (1042–55) and Constantine X (1059–67), Füeg's observations suggest that the Virgin, when depicted without a Christ medallion held before her, was consistently placed on the upper die, whereas in

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<sup>50</sup> E.g. P. Grierson, *Numismatics*, p. 86, or indeed Grierson, *Byzantine Coins*, pp. 27–8.

<sup>51</sup> Grierson, *Byzantine Coins*, p. 158; cf. F2007, pp. 22–3, and now K. Kotsis, 'Defining female authority in eighth-century Byzantium: the numismatic images of the Empress Irene (797–802)', *Journal of Late Antiquity* 5 (2012), pp. 185–215.

<sup>52</sup> Sometimes, also, no real disparity between die counts is observable (F2007, pp. 27 and 77).

<sup>53</sup> F2007, p. 45.

<sup>54</sup> F2014, pp. 75, 79 and 80, although Füeg's illustrations do not reflect the altered position.

those series that depicted her with a medallion she was placed on the lower one.<sup>55</sup> Since other issuers of similar coins did not make this distinction, one may hesitate to call this a policy as opposed to a particular mint-master's sensibility (though that would be valuable to show), but that it was done at all is intriguing.

All this evidence of choices, presumably made by individual superintendents or workers within the mint, should prevent us from being so ready to assign political significance to the choice of obverse and reverse imagery, or from assuming that those involved with Byzantine coins saw this distinction in the way that we have constructed. Indeed, in the one case where a Byzantine source appears to make this distinction, it disagrees with ours by describing the Virgin, rather than the emperor, as being on the 'back'!<sup>56</sup> Our understanding of what makes 'obverse' and 'reverse' on a coin is, after all, a model, rather than a constant that we know applied at other times too. Füeg's work encourages us to question it simply because of the difficulty he experiences making it fit his data.

#### The Dissemination of Messages on Coins

Here and elsewhere Füeg's work provokes an exercise of unintended revisionism, forcing us to confront our own assumptions. He arguably shows, for example, that the portraiture of dated seals cannot be used to date coins, although he is attempting to demonstrate the opposite.<sup>57</sup> He sees responses in coin iconography to matters of the highest level: an imperial coin portrait in which the emperor's right forefinger is extended toward his face, a gesture that Füeg repeatedly calls 'suggestive' without explanation, is for him associated with the imperial coronation of Charlemagne (emperor 800–14).<sup>58</sup> But he has no evidence for this, nor is it clear how such a response would be understood. Since the portrayal begins with Emperor Nikephoros I (802–11), one could argue with equal justification that by having him point to his beard the die-cutters registered the return of a man to the imperial throne after

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<sup>55</sup> F2014, p. 30.

<sup>56</sup> George Pachymeres, *Relations historiques*, ed. A. Failler and trans. V. Laurent, *Corpus fontium historiae byzantinae* 24 (Paris, 1984), p. 540, cit. by Morriison, 'Précis', p. 9. The Greek word used is 'opisthen'.

<sup>57</sup> F2007, p. 12; cf. *ibid.* p. 19.

<sup>58</sup> *Ibid.* pp. 118 and 122 ('suggestive') and 124 (Charlemagne).

Empress Irene's fall. Space precludes addition to this list of points, but one could go on.

While the detection of political significance in upraised fingers or the number of waves in the imperial haircut may be misplaced, Füeg's addition to the body of identified icons on the coinage, mentioned above, implies that there were still visual references on the coins that some part of the coin using public was expected to recognise. Of course, the idea is well established in the literature that coins were used by their issuers to send messages and so-called propaganda to their users, not least because these messages are sometimes still evident to us.<sup>59</sup> Füeg himself propounds this point of view, saying, 'In the event of a change of government, new money with the name of the new emperors was quickly and circumspectly distributed around the empire in order to familiarize the notables – with imperial emphasis – as to who was ruler in Constantinople.'<sup>60</sup>

But how quickly? Füeg immediately goes on to say that nomismata are known which, from overstriking, can be shown to have circulated for sixty or even eighty years.<sup>61</sup> The implications of this are worth drawing out. Füeg says that 'no recalls of coins are known', and while he is not quite right about this, it is broadly true that such circulation patterns show the rarity of anything like total recoinage or demonetisation of older coins (as distinct from their progressive consumption, destruction and reconstitution in the tax cycle).<sup>62</sup> Füeg cites, indeed, as have others, a Novel of Leo VI

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<sup>59</sup> Albeit more from the early Roman world than the later one: see Crawford, 'Imperial coin types', or more recently, B. Levick, 'Messages on the Roman coinage: types and inscriptions', in G.M. Paul & M. Ierardi (eds), *Roman Coins and Public Life under the Empire: E. Togo Salmon Papers II* (Ann Arbor, MI, 1999), pp. 41–60. For a Byzantinist's view see Morriison, 'Authority and Kharaktèr', or the summary at Morriison, 'Précis', pp. 57-59. The term 'propaganda' seems first to have been deployed of ancient coins by A. Alföldi, 'The Main Aspects of Political Propaganda on the Coinage of the Roman Republic', in R.A.G. Carson & C.H.V. Sutherland (eds), *Essays in Roman Coinage Presented to Harold Mattingly* (London 1956), pp. 63–95.

<sup>60</sup> F2007, pp. 152–3, quote p. 152.

<sup>61</sup> F2007, p. 153.

<sup>62</sup> Hendy, *Studies*, p. 319, for known episodes of demonetisation, including Leo VI as mentioned below. For the consumption of coin in the tax cycle, see Hendy, *Studies*, pp. 284–303, or J. Banaji, 'The circulation of gold as an index of prosperity in the Central and Eastern Mediterranean in Late Antiquity', in C.E. King and D.G. Wigg (eds), *Coin Finds and Coin Use in the Roman World: the*

demanding that coins of previous emperors be accepted.<sup>63</sup> What this means is that any imperial issue of coins would initially have all but vanished in circulation amid a mass of older money.<sup>64</sup> Füeg assumes that Byzantine coin production was primarily to replace wastage, and certainly it seems unlikely that the state would have over-spent its closely-guarded gold simply to make a propaganda point slightly faster.<sup>65</sup> Now, we do not have good, or even bad, figures for wastage of nomismata but even if they were as high as 2%, higher than most historically documentable coin wastage rates, it would then have taken about thirty-five years of continuous issue for a new coinage to become even a majority in circulation.<sup>66</sup> Of the emperors covered by Füeg's work, only Constantine VII and Basil II would have reached that point, and not all of Constantine's coinage was in his own name. The short-lived Michael I (811–13), meanwhile, would have had only a 6% random chance of acquiring one of his own coins from circulation on the day of his death.

These are constructed figures but they make a point: if the nomisma was a propaganda medium, it cannot have functioned through regular circulation. The only way for an emperor or his subordinate to ensure that a group of persons saw the newest and most relevant coin issues would have been to supply them direct. Distribution through money-changers would not have been sufficient for this, since there was more gold in circulation than the empire took in tax in each indiction. Not enough could have been gathered at once to buy up all the old coins, even without the ongoing needs of imperial expenditure.<sup>67</sup> Activities which required new coin must

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Thirteenth Oxford Symposium on Coinage and Monetary History 25.–27.3.1993, SFMA 10 (Berlin 1996), pp. 41–53.

<sup>63</sup> F2007 p. 164; cf. Hendy, *Studies*, pp. 302–3, where the significant part of the Novel is given in translation.

<sup>64</sup> Noted also, in passing merely, by Crawford, 'Imperial coin types', p. 58.

<sup>65</sup> F2007, p. 153.

<sup>66</sup> For a gathering of data from all periods on coin wastage rates, see T.R. Volk, 'Mint output and coin hoards', in G. Depeyrot, T. Hackens and G. Moucharte (eds), *Rythmes de la production monétaire de l'antiquité à nous jours*, *Numismatica Lovaniensia* 7 (Louvain-la-Neuve, 1987), pp. 141–221, although this is not to endorse his conclusions; cf. Morrisson, Barrandon and Poirier, 'Monnaie d'or', p. 151.

<sup>67</sup> This logic does not, thankfully, require a reconstruction of the imperial budget as in Hendy, *Studies*, pp. 157–220, just an acceptance that there was more gold coin in the empire than the state held at any one time.

therefore have been small-scale and localised. One thinks immediately of donatives or the annual payments of *rhogai*, but even there, according to our best account, the moneys in question were delivered in sacks and presumably not examined individually on the spot by the fortunate, if overloaded, recipients.<sup>68</sup> More likely contexts for such use of coin are the various largesses and targeted gifts that emperors made; if one was to shower gold upon the people or suddenly enrich a well-chosen noble or churchman, one might well wish to be evident in the material as well as the circumstance of the act.<sup>69</sup> If that is correct the iconography and messages on the coinage were being delivered to very specific audiences, and we could get much more out of the intent of such delivery than simply to describe it as empire-wide propaganda, in which role it would necessarily have been diffuse and uncontrolled.<sup>70</sup>

#### Demonetisation and Withdrawal of Coin

We have referred to a Novel of Emperor Leo VI which enjoined the acceptance of the coinage of previous emperors.<sup>71</sup> As Füg says, this must be connected with the very small numbers of gold coins that Leo himself seems to have issued. This is illustrated starkly by Füg's tables; he knows of 40 coins of Leo's sole reign of 22 years compared to 129 of the four years he shared with his son Constantine VII.<sup>72</sup> For Füg this can be simply explained: 'One can understand the edict as Leo's appreciation that the treasury reserves including coins from earlier reigns were sufficient, and that enough *solidi* were in circulation to uphold the state's economy.'<sup>73</sup> This makes sense

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<sup>68</sup> On the *rhogai* and their distribution, attested principally through Liudprand of Cremona's *Antapodosis*, see Hendy, *Studies*, pp. 187–92, with the relevant passage quoted in extenso.

<sup>69</sup> *Ibid.* pp. 192–201. Note however that John Cantacuzenes envisaged emperors handing out even such largesses in bags: C. Morrisson, 'La découverte des trésors à l'époque byzantine : théorie et pratique de l'εὔρεσις θησαυρου', in *Mélanges Paul Lemerle, Travaux et Mémoires du Centre de recherche d'histoire et civilisation de Byzance* 8 (Paris, 1981), pp. 321–43, repr. in Morrisson, *Monnaie et finances à Byzance*, no. VII, p. 322 n. 13, citing Cantacuzenes I.41. Perhaps the beleaguered Palaeologan could not by then guarantee that the coin inside would in fact be his.

<sup>70</sup> Cf. A. Marzano, 'Trajanic building projects on base-metal denominations and audience targeting', *PBSR* 77 (2009), pp. 125–58; as yet I know of no such work for the Byzantine Empire.

<sup>71</sup> See pp. 18–19 and n. 62 above.

<sup>72</sup> F2014, p. 138, updating F2007, p. 168.

<sup>73</sup> F2007, p. 164.

in its own terms but, as Hendy had already shown, the situation was in fact more complex. As the Novel makes clear, rather than legislating for a situation of his making (by not striking coins), Leo claimed that he was repealing an act of earlier rulers: ‘because they begrudged the plenty of their subjects, they made redundant coin bearing the figure of [any] one of their predecessors, and made current only that bearing their own.’ He goes on to say how hard this has made it for people to obtain acceptable coin—as noted above, the mint could not have made up this kind of dearth with any speed—and abrogates the ruling so as to open up circulation once again to all full-value gold coin.<sup>74</sup>

Now, as Hendy rightly noticed, what is referred to here is a demonetisation, and presumably quite a recent one. The emperors in question were plural, it seems, or else the ban went on for more than one reign; if the emperors were concurrent, however, then the obvious candidates are Basil I and his son Constantine, in which case the obvious predecessor is Michael III. Hendy did not go so far, saying only:<sup>75</sup>

Some selective demonetisation, perhaps of the nomismata of a particular emperor, or group of emperors, with their handing over to the authorities being enforced, with or without a reimbursement, in the former case perhaps at a discount, is much more likely to have been involved [than a blanket ban on earlier coinage]. The motivation behind such a demonetisation may conceivably have been either political or religious, or both, iconoclast or iconodule emperors being obvious vehicles of such feelings, but that of a general malevolence towards *imperial subjects seems unlikely....*

Michael III, as the ruler under whom icons were restored and Christ returned to the coinage, might fit into such a frame, although Basil I (867–86) was hardly opposed to those changes. But he had succeeded Michael III by skulduggery and murder, and having his former patron and victim glittering with the Triumph of Orthodoxy in people’s hands may have been unpleasant for him to contemplate.<sup>76</sup> Some such formulation is necessary, in any case, because again Füeg’s tables make a disparity

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<sup>74</sup> Hendy, *Studies*, pp. 302–3, with the translation of the Novel quoted here; the text is given in P. Noaille and A. Dain, *Les nouvelles de Léon VI le Sage* (Paris, 1944), pp. 199–201.

<sup>75</sup> Hendy, *Studies*, p. 303.

<sup>76</sup> G. Ostrogorsky, *History of the Byzantine State*, trans. J. Hussey, 2nd ed. (Oxford, 1968), pp. 232–5.

clear. While Michael III's solidi are not unknown, Füg counts 72 from the 11 years of his sole issue, as opposed to 160 from the prior 12 years in which he issued with Empresses Theodora and (initially) Thecla.<sup>77</sup> There is, it should be admitted, no reference to this numismatic damnatio outwith the Novel of Leo itself, whose interpretation may remain open to question, but Füg's data at least allows a plausible supposition to be offered.

#### Die-Cutters and their Initiative

Here and in other places, therefore, we are faced with occasions when it seems clear that emperors did decide and enact what coinage should depict and what information should be upon it, at least *grosso modo*, but Füg's arguments, and his seriation, often rely on far tinier details of design. Some of these are new and important observations: he is able, for example, to show that, in the reigns of Constantine VIII and Michael IV at least, dies had their lettering cut onto them before the design was added, as the latter can sometimes be found cutting into the legend. This also suggests that the two processes were done by different workers, since as Füg puts it, 'it is hardly likely that a die-cutter would debase his own work'.<sup>78</sup> These are glimpses of mint-working process we did not have before, but Füg's illustrations make it quite possible to observe the phenomenon.<sup>79</sup>

This raises the question of how many hands were at work in the manufacture of Byzantine coin dies. Numismatists are perhaps somewhat obsessed with the figure of the die-cutter, the only person in the process of coin-making whose work we can genuinely identify. It has been argued that die-cutters were moved huge distances

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<sup>77</sup> F2014, p. 138, updating F2007, p. 168. Note that Michael III seems to have issued no copper-alloy coinage in his sole reign; Grierson suggested that he continued to coin in the name of Theophilus (Byzantine Coins, p. 183). If this applied in the case of the gold too, both Füg's and my theories would of course collapse.

<sup>78</sup> F2014, pp. 20 and 23.

<sup>79</sup> *Ibid.*, p. 56. If this is correct, and Grierson was also right (in his 'Coins monétaires et officines à l'époque du bas-Empire', *SM* 11 (1961), pp. 1–8 at pp. 7–8) that officina letters, in the period in which they were used, were not cut until arrival of the die in the relevant officina (whatever that was—see below) then each Byzantine coin die had already been through three craftsman's hands before use. One wonders why this complication was endured.

when late Roman and Byzantine mints were closed so that others might be opened; indeed, it is assumed that the whole mint staff was transferred even though it is only the die-cutters' work that is visible to us.<sup>80</sup> This suggests that such persons were very few in number. But it now seems that the empire did not need very many of them. Füeg's data suggest that for the period covered by his first volume the empire was using up around 13 observable nomisma dies a year. Even with estimates extrapolated as per Carter (see p. x above) this total rises only to 20, somewhat less than one die a fortnight.<sup>81</sup> At that rate, one wonders how there was even work for more than one die-cutter. The total admittedly climbs steeply in the period of the *histamenon*, with the mean die-count per year over the period 967–1067 being 45, extrapolated to 168, and that of Constantine X (1059–67) alone being 95 and 241.<sup>82</sup> Even all those dies, however, could have been manufactured by only two or three reasonably careful men. The idea sometimes found that dies had to be supplied with great urgency, leading to haste in their execution, seems much weaker with this in mind.

It is of course normal to detect more die-cutters than this at work in a coinage.<sup>83</sup> This suggests at least two things: first, that we overrate our ability to recognise style in this fashion, or secondly, that such engravers also did other work and were called on only occasionally by the mint, perhaps being court craftsmen more usually occupied with the palace's ornamentation, treasures, automata and so on.<sup>84</sup> Füeg clearly favours this option, as he thinks that some die-cutters were practitioners

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<sup>80</sup> Grierson, *Byzantine Coins*, p. 122: Carthage evacuated and reopened in Sardinia; Hendy, *Studies*, p. 381: Ostia's staff, already moved from Carthage by Maxentius, then moved to Arles by Constantine I; London and Ticinum closed to allow the opening of Constantinople and p. 525 n. 383: a Serbian mint, probably supplied with dies from Thessalonica, was apparently unable to fabricate replacements.

<sup>81</sup> Note that at F2014, p. 21 Füeg suggests that in the reign of Romanus III the empire was using up two dozen dies a week. None of his numbers run anywhere near so high; is it possible he has misread his own figures of dies per year? Even this would still only be three dies a week for eight engravers, however; there probably could have been eight engravers in Constantinople at a time.

<sup>82</sup> Such a scale of multiplication amply justifies all Füeg's cautions about the validity of his samples (F2014, pp. 127–8), which again does not put him off using them.

<sup>83</sup> E.g. F2007, pp. 12 and 17 or F2014 p. 26.

<sup>84</sup> Averil Cameron, *The Byzantines* (Malden, MA, 2006), pp. 70–5.



in other media, but one still needs to reconsider the scale of die-cutting operations at Constantinople in the light of his figures.<sup>85</sup>

Such small numbers also add to our understanding of the tiny variations in coin types. Whereas in a large team, as said, this would seem to imply micro communication at an obsessive level to maintain consistency and avoid repetition, if the number of working die-cutters really was quite small, that implies that we are looking not so much at consistent differentiation between craftsmen as individuals distinguishing things among their own output. Why this was necessary we still do not know. Even if pattern coins were kept on file to compare with forgeries, that would not require such frequent changes of marking. Indeed, creating extra varieties of design would only make forgeries harder to distinguish. Here again, therefore, Füeg's observations require us to think harder about what provoked such variations in Byzantine die-cutting. They also remind us how much liberty such persons had and how broad-brushed their orders, from whosoever they came, may have been. Studies on earlier Roman coinages have picked up on the latitude between specification and execution, but it can also be seen here, and deserves more recognition.<sup>86</sup>

#### Officinae and their Ending

One variation that is usually taken to have been mandated by powers other than the die-engraver is the officina mark. In the late Roman and Byzantine periods we recognise that mints were divided into officinae, each of which marked their coins with a signal letter. We do not, admittedly, know what the officinae actually were: teams within the mint, separate workshops and moneyers have all been suggested.<sup>87</sup> Chronologically this barely makes it into Füeg's period: officinae ceased to be marked on Byzantine coins in the reign of Leo III, and thereafter the reverses of

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<sup>85</sup> Die-cutters working in other media: F2007, p. 135.

<sup>86</sup> E.g. P. Bruun, 'The system of the Vota coinages: coordination of issues in the Constantinian empire', *NNÅ* 1956 (1958), 1–21, repr. in Bruun, *Studies in Constantinian Numismatics: papers from 1954 to 1988*, ed. A. Tammisto, *Acta Instituti Romani Finlandiae* 12 (Rome: Institutio Romani Finlandiae, 1991), pp. 27–36, or Bruun, 'The disappearance of Sol from the coins of Constantine', *Arctos* 2 (1958), pp. 15–37, repr. in id., *Studies in Constantinian Numismatics*, pp. 37–48, at p. 38 of the reprint.

<sup>87</sup> Grierson, *Byzantine Coins*, pp. 22–4, summarises debate; see also Grierson, 'Coins monétaires'.

contemporaneous coins were differentiated, if at all, by letters at the end of the reverse legend that we tentatively take to be control marks.<sup>88</sup>

Füeg makes two intriguing observations. First, he points out that the end of officina letters in their traditional form apparently entailed a drop in the number of reverse dies needed by the mint.<sup>89</sup> Secondly, he argues that Leo III introduced indictional dating on his coins and that this is how to understand the final letter of the reverse legend of the post-officina coins, a practice that he sees continuing into the reign of Constantine V. This necessitates both the return to a discredited date of death for Leo and the silent reassignment of one of his types with Constantine to Constantine's own reign.<sup>90</sup> If this latter is correct, it raises the issue of how it could be that Constantine V produced coins at Constantinople even when Artavasdus ruled there. Füeg suggests, indeed, that some craftsmen may have worked for both rulers.<sup>91</sup> It seems more likely that Füeg's reading of indictional dates into the coin legends is mistaken and that the issues are not contemporaneous, however, not least because Füeg's data also seems to show that we are wrong to suppose that officinae operated simultaneously, at least at this time.

This is a controversial claim, and not one that Füeg himself makes. Nonetheless, his contention that the end of officina letters brought about a reduction in observable reverse die numbers seems to be justified in his statistics.<sup>92</sup> Coin production, however, appears to have remained steady. Now, if officinae were parallel workshops and all continued producing, they would have exhausted reverse dies at more or less the same rate as before. If ceasing to mark officina letters on dies instead allowed the use of fewer reverse dies, that implies that officinae were differentiated not in space but in time. Rather than parallel workshops all striking at once under different marks, we must consider a period of activity in which the whole mint would be striking coins with A dies, then a period in which B was used, and so on. The obverse dies would have remained in use throughout, but new reverse dies would be

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<sup>88</sup> F2007, pp. 11–6.

<sup>89</sup> F2007, p. 11.

<sup>90</sup> *Ibid.*, pp. 14–15.

<sup>91</sup> *Ibid.*, p. 15.

<sup>92</sup> *Ibid.*, pp. 11 and 166.

needed at each changeover, by which the period of operation would be identified. The abandonment of that distinction allowed this over-production of dies to stop.

This suggestion may resolve some hitherto intractable problems. For example, in his *Studies in the Byzantine Monetary Economy*, Michael Hendy noted a massive drop in officina numbers at Constantinople, from ten to two, during the Persian invasion of the Empire of the early seventh century. Hendy saw this necessarily as a massive drop in mint output.<sup>93</sup> It was Grierson's impression, however, that mint output under Emperor Heraclius (610–41) remained high, and certainly his nomismata are extremely common, albeit often somewhat careless of both engraving and striking.<sup>94</sup> On the other hand, we are told by the written sources of the time that Heraclius ran very short of gold in the dark days of the mid-620s and had to resort to silver to pay his troops, which is held to have resulted in the hexagram.<sup>95</sup> All this could in fact be reconciled if it be assumed that Heraclius, presumably because of those troops and his desperate campaigning, needed money much faster than emperors normally did.<sup>96</sup> Whatever gold he could gather by the tax cycle would have been struck at maximum urgency, and have been disbursed again in quite a short time. With a mint that ran officinae in parallel, we should then expect them all to be visible so as to maximise output, but they are not. If the officinae worked in series, however, we should expect something like what we see: all available gold struck, hurriedly, before the second officina had finished operating in the mint, and indeed gold still so short that resort had to be made to silver. So here again this version of officina working seems to explain the evidence of the coinage better than the accepted one.

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<sup>93</sup> Hendy, *Studies*, pp. 227–28.

<sup>94</sup> DOC II.1 pp. 5–6; Grierson, *Byzantine Coins*, pp. 85–6. DOC itself makes clear how substantial the surviving numbers of Heraclian nomismata are. One can add the figures from the collection of the Barber Institute of Fine Arts, University of Birmingham, where Heraclius's 388 nomismata are nearly double any other ruler's representation in gold (Constans II is second with 217, Maurice third with 155). These figures are the author's own counts, carried out in August 2015.

<sup>95</sup> E.g. Grierson, *Byzantine Coins*, p. 103, or Hendy, *Studies*, pp. 494–5, the latter with reference and translation of the primary material.

<sup>96</sup> On the tumult of Heraclius's career see J. Howard-Johnston, 'Heraclius' Persian campaigns and the revival of the Eastern Roman Empire, 622–630', *War in History* 6 (1999), pp. 1–45, repr. in Howard-Johnston, *East Rome, Sasanian Persia and the End of Antiquity: Historiographical and Historical Studies*, *Variorum Collected Studies* 848 (Aldershot, 2006), VIII.

There are, admittedly, limits to the possibilities of this reinvention of the officina. Grierson considered the possibility that officinae denoted an issue sequence in 1961 and found it wanting not least because all ten officinae of Constantinople can be observed in the joint coinage of Emperors Justin I and Justinian I, even though their reign together lasted only four months (in mid-527).<sup>97</sup> Such problems for the theory do not arise in later reigns, however, whereas the traditional solution favoured by Grierson does give rise to the difficulties already mentioned. A solution may be that this alteration in striking patterns was one of the many changes brought about in the centralisation of the production of gold coin by the Emperor Heraclius, which would explain why it fits so well in his reign as we have seen.<sup>98</sup> Certainly, the debate cannot be closed here, but that it can be reopened is an example of the kind of significance that can be drawn from data like Füeg's by the critical reader.

#### Efficiency and the production of concave coins

This all suggests a mint that worked with frantic urgency for quite short periods, and Füeg evokes this with a very human portrayal, indeed, using common sense to unseat the famous but disdainful description of Mesarites: mint-workers would have needed light, air, skill and team-mates to work at full efficiency.<sup>99</sup> Füeg sees efficiency as, a priority in determining how a mint operated and follows Morrisson in explaining the concave coinages from the eleventh to the thirteenth centuries.<sup>100</sup> I am publishing on this subject elsewhere, so will not recapitulate the debate, but suffice it to say that despite prolonged controversy over the reasons for adopting such a peculiar form of coins, opinion has, since the mid-1990s, more or less rested with Morrisson and her colleagues, who argued that the concave fabric was both a symptom of, and eventually a deliberate response to, the broadening and thinning flan of the nomisma and its decreasingly fine alloy, both of which made it less plastic and harder to strike adequately. With the flan so broad that 30% of it was left unstruck by the dies, the unstruck portion would easily bend around the die under the impact of the strike. The

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<sup>97</sup> Grierson, 'Coins monétaires', p. 6.

<sup>98</sup> See Hendy, 'Administrative Basis of the Byzantine Coinage'.

<sup>99</sup> F2014, p. 121; more description of process *ibid.*, pp. 112–13; cf. Morrisson, 'Précis', pp. 11–12!

<sup>100</sup> F2014, pp. 103–24.

response of the mint was to ensure that this at least happened consistently by making the reverse die smaller than the obverse, resulting in the concave fabric.<sup>101</sup>

There are things this answer leaves unexplained, such as why the simpler option was not taken, to make the coins narrower and thicker again, as was done (presumably for different reasons) at Carthage in the seventh century. Füeg recognises this problem, and his suggestion is that Emperor Michael IV wanted his coins to be larger than the competing Muslim dinar and to provide his die-engravers with a canvas suitably large for displaying the splendour of his new dynasty.<sup>102</sup> One wonders in what context nomismata and dinars could be said to have competed. Also, of course, although the flan size of the nomisma increased, die size did not, so Michael IV was not in fact displayed any larger than his predecessors had been. Nonetheless, Füeg is basically happy with the proposition that efficiency of striking under difficult conditions was the primary motivation for the development of concavity.

In fact both because of Füeg's data and because of other factors, this is less likely to be true than Füeg supposes. First, work done by Simon Bendall and David Sellwood, to establish the manufacturing process for these coins, emphasises its great

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<sup>101</sup> C. Morriçon, 'La concavité des monnaies byzantines', *BSFN* 30,6 (1975), pp. 786–8 (cf. P. Lathoumetie, 'Le problème des pièces cupuliformes byzantines reste entier', *Archaeonumis* 17 (1976), pp. 14–17); F. Delamare, P. Montmitonnet and C. Morriçon, 'Une approche mécanique de la frappe des monnaies: Application à l'étude de l'évolution de la forme du solidus byzantin', *RN*<sup>6</sup> 26 (1984), pp. 7–39; id., 'A mechanical approach to coin-striking: application to the study of Byzantine gold solidi', in W.A. Oddy (ed.), *Metallurgy in Numismatics II*, *RNS SP* 19 (London, 1988), pp. 41–53, repr. in Morriçon, *Monnaie*, XIII; Delamare, Montmitonnet and Morriçon, 'L'apparition de la concavité des monnaies d'or frappées au XIe siècle', *RBN* 145 (1999), pp. 249–59; Morriçon, 'Précis', pp. 63–4. Cf. J. Jarrett, 'Why did the Byzantine coinage turn concave? Old suggestions and a new one', in *Atti del 15 Congresso Internazionale di Numismatica, Taormina, 21 – 25 settembre 2015*, ed. Maria Caltabiano and Mariangela Puglisi (Messina, forthcoming).

<sup>102</sup> F2014, pp. 122–4. Morriçon, 'Précis', p. 63 n. 248, working with an unpublished draft of F2014, reads Füeg as attributing these motives to Constantine IX, and initially disagrees, but then allows such factors to explain the apparent public appeal of such coins: 'elle devait inspirer [attachement] au public, par son diameter large et, pourquoi pas, par ses implications symboliques' (this last apparently a reference to M. Labouret, 'Monnaies "scyphates": de l'analogie structurale à l'hypothèse idéologique', *BSFN* 65 (2010), pp. 113–23). Of course, that the coins were popular in this fashion is nowhere attested.

complexity.<sup>103</sup> They determine, first, that the dies that impressed the design were curved, and Füeg's own diagrams of flan curvature make it clear that this must have been so, with presumably the added difficulty of rendering a design on the die that would appear correctly proportioned.<sup>104</sup> There is some argument about whether the flans had already been bent at this point, but it is at least possible.<sup>105</sup> Secondly, Bendall and Sellwood note that the coins must have been struck twice, with the upper die rocked from side to side to ensure impressing of both halves of the design; the mismatches this caused are extremely easy to spot once they are familiar.<sup>106</sup> Füeg is not aware of this explanation, but he does observe the phenomenon, accounting it as double-striking and seeing it with increasing frequency through the course of the eleventh century, on a full half of observed coins by the time of Emperor Isaac I Komnenos and on most coins by that of Emperor Constantine X.<sup>107</sup> He also believes that there are signs of the edges of the coins being beaten out flat, and indeed, anyone familiar with concave histamena will have noted the thinness and even sharpness of their edges, although those edges would never have been under the impact of a die and should therefore be thicker than the centre.<sup>108</sup>

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<sup>103</sup> S. Bendall and D. Sellwood, 'The method of striking scyphate coins using two obverse dies in the light of an early thirteenth century hoard', *NC*<sup>7</sup> 18 (1978), pp. 93–104; Sellwood, 'The production of flans for Byzantine trachy issues', in D.M. Metcalf and A. Oddy (eds.), *Metallurgy in Numismatics*, *RNS SP* 13 (London, 1980), pp. 174–5; S. Bendall, 'The double striking of late Byzantine scyphate coins', *Celator* 12 (1998), pp. 20–3. See also M.F. Hendy and J.A. Charles, 'The production techniques, silver content and circulation history of the twelfth-century Byzantine trachy', *Archaeometry* 12 (1970), pp. 1–21, repr. in Hendy, *Economy*, VI.

<sup>104</sup> F2014, p. 109.

<sup>105</sup> The argument of Bendall and Sellwood, 'Method', requires the flans already to be curved when struck; cf. F2014, p. 117. M. Labouret, 'Monnaies scyphates' <<http://www.marc-labouret.fr/monnaies-scyphates.html>> [last modified 2 May 2012 as of 17 September 2015], *Annexe amusante*, adduces parallels using former dies made of wood, which is not to endorse Labouret's other conclusions (cf. F2014, p. 124).

<sup>106</sup> For example, Barber Institute of Fine Arts B5451 (histamenon of Michael VII, DOC III.2 Michael VII 2e) and B5499 (histamenon of Nicephorus III, DOC III.2 Nicephorus III 1), among numerous others.

<sup>107</sup> F2014, p. 109. Füeg 2014, p. 153 lists Sellwood, 'Production', and it is cited *ibid.*, p. 108, though the double application of the die which he suggests is not part of Füeg's discussion.

<sup>108</sup> F2014, p. 108.

All of this, the manufacture of curved dies, the beating out of edges, the pre-striking (presumably in that order), the two strikes per coin and the frequent cracking and breakage to which the whole process apparently gave rise massively undermine any argument that the purpose of these changes was to speed up the manufacture of coin. Indeed, Füeg believes that overall production of nomismata declined despite the vast increase in the numbers of dies used in their manufacture.<sup>109</sup> He also registers other problems with the mechanical hypotheses of Delamare and colleagues, noting concave coins of which more than 70% has been struck and whose dies were the same size.<sup>110</sup> This, like the requirement for curved dies, which Füeg observes even on coins of the eleventh century, seems to show that this complicated manufacture process was in use from almost the beginning of concavity, meaning that it was a deliberate decision to produce concave coins, rather than an adaptation to the accidental concavity resulting from increasingly inadequate conventional striking.<sup>111</sup>

## Conclusions

Once again, therefore, there are conclusions to be drawn from Füeg's data that challenge existing wisdom. A critical reading of these two volumes reveals much about the inadequacy of our current understanding on so many of the inner issues of the Byzantine currency. These range from the somewhat abstruse question of concave coins to more universal concerns such as political messaging, stylistic seriation, the initiative and number of die-cutters necessary for the production of so large a state's coinage and the internal organisation and operation of its mints. Just how few of those questions even this speculative summary has been able to attempt to answer, makes it clear just how desirable a wholly new synthesis of Byzantine minting, its processes and its purposes, would be. Whatever the shortcomings of Füeg's work, it provides some of the data that such a synthesis will need and indeed shows in relief the most

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<sup>109</sup> F2014, p. 113 ; cf. Morrisson, 'Précis', again using an unpublished draft of F2014 which she apparently reads as arguing for a massive increase in output, rather than just die use; cf. n. 82 above.

<sup>110</sup> F2014, pp. 108 and 117, cf. Delamare, Montmitonnet and Morrisson, 'Apparition de la concavité', p. 258. Füeg suggests (p. 108) that the true tipping point may have been flans of more than 25 mm width, which depending on die size might amount to something very similar to the 70% rule.

<sup>111</sup> Naturally enough, this is also the line of argument pursued in Jarrett, 'Why did the Byzantine coinage turn concave', where why this might have been desirable is explored.

difficult questions it will need to resolve, and for both data and stimulus the field may well then be grateful.