

ZEITSCHRIFT
FÜR
PAPYROLOGIE UND EPIGRAPHIK

herausgegeben

von

*Werner Eck, Helmut Engelmann, Dieter Hagedorn, Rudolf Kassel
Ludwig Koenen, Wolfgang Dieter Lebek und Reinhold Merkelbach*



BAND 69

1987

DR. RUDOLF HABELT GMBH · BONN

CONVERSION AND ONOMASTICS: A REPLY

In *ZPE* 62 (1986) 173-81, Ewa Wipszycka has presented an evaluation in some detail of a number of points central to the thesis of my article in *BASP* 19 (1982) 105-24 on "Religious Conversion and Onomastic Change in Early Byzantine Egypt." Drawing on her long experience with Byzantine Egypt and her own distinguished work on the subject, she accepts, in essential, the first part of my argument (that an examination of nomenclature shows the rate of Christianization of the country), while rejecting the second (that one must multiply the number of persons in a given generation with Christian names by about 1.5 to get an approximation of the number of Christians). Neither point seems to me quite correct: that is, I think my analysis of the chronological distribution of Christian names needs a correction more important than any Wipszycka suggests; but her extremely interesting and valuable arguments against the second part of the argument seem to me actually to strengthen it. Nonetheless, one can only repeat that the quality of the results remains limited by the data at our disposition, and they have numerous drawbacks for this kind of investigation.¹

1. Names and Dates

Wipszycka devotes pp. 173-78 of her article to a discussion of the foundations of the data. She expresses some doubts about various of the names which I identified as Christian and about the statistical validity of the material from *P.Cair.Isid.*, along with doubt about the date of the Hermopolite Land Registers (not herself expressing an opinion, simply noting that that date was controversial). In the face of these reservations, however, she concludes, "Malgré tous les doutes que j'ai exprimés ci-dessus, je pense que le rythme des changements a été établi avec certitude et doit correspondre au rythme des conversions."

It is certainly true that gray areas exist in the identification of names as Christian or pagan, and throughout the original article I tried to underline the uncertainties that the method used could not eliminate.² A great deal of onomastic work could usefully clarify some of the

¹ "Je soutiens seulement qu'il faut garder la prudence à l'égard des résultats," says Wipszycka (p. 175); my article certainly intended to make that point at several stages.

² She reproaches me (p. 177) for the fact that "dès qu'il s'est mis à calculer, ses doutes ont dû s'estomper." It is not clear just how such nuances are to be preserved in calculations!

names in dispute, but some uncertainty must remain.³ The numerically most significant class to which Wipszycka objects is no. 2, names formed from the Egyptian root ntr ('god'). She says, "Des deux noms cités par Bagnall à titre d'exemples, Papnouthios et Pinoution, le premier apparaît au IIIe siècle, mais ne devient fréquent qu'ensuite, le second est bien attesté pour les siècles antérieurs à la christianisation de l'Égypte." For Papnouthis, this observation seems decisive: The growth in the use of the name corresponds exactly to the Christianization of Egypt.⁴ For Pinoution, I do not know just what Wipszycka means, for I have found no examples prior to the third century, and the vast majority are fourth century and later, just as with Papnouthis.⁵ It may well be that some monotheistically inclined pagans used such names, as Wipszycka thinks, but the distribution of the name indicates unmistakably a Christian connotation. Much the same points may be made about most of the abstract and adjectival names like Eulogios. In all of these cases, it is certainly true that the method may misclassify a given individual; but that misclassification is (as Wipszycka admits) unlikely to be statistically significant. Vague appeals to the "changement de mentalité religieuse à l'époque impériale" do not change that fact.⁶

Wipszycka's objection to my using a date in the 340's or somewhat later for the Hermopolite land registers (in *P.Herm.Landl.*) seems to me completely unjustified, especially now in the light of Wilfried van Gucht's decisive demonstration in his paper at the Naples Congress⁷ that the bishops in the registers can be identified and that the registers must on the basis of these identifications postdate 346/7 (though probably not by too much). Instead of

³ Wipszycka (p.175) objects to Apollos, which "ne peut pas être considéré automatiquement comme chrétien." I did not claim that; cf. my p. 111: "Apollos, for example, is a good pagan name." As to the saints, I described them as "the most difficult category to evaluate." Wipszycka herself admits, however, that such items probably do not affect the results very much; at the most, they change the degree to which the coefficient discussed below must be raised or lowered.

⁴ I am not sure that Papnouthis is not found in some spelling before the third century, but at most very rarely. Foraboschi, *Onomasticon*, lists two instances for Papnoutis from the first two centuries; his one example for Papnouthis (*P.Stras.* 194), however, is not II but VI.

⁵ It is possible that Wipszycka was misled by mistakes in Foraboschi's *Onomasticon*, where *SB VI* 9595 is cited as "I-III" (it is actually 7th century), and *P.Stras.* 138 is cited as "II" (it is actually from A.D. 325).

⁶ While the matter does not affect our results, it is surprising to find Wipszycka casting doubt on the extent to which parents were conscious of the meaning of theophoric names (p. 176). She offers no evidence that a speaker of Egyptian would not be aware of the meaning of a name like Petechonsis, and the generalized linkage of the prevalence of such names to local cults certainly seems to indicate that local devotion was involved. The fact that Christians did not change name at baptism (as I myself pointed out) is irrelevant to the method involved here. T. Gregory, *AJP* 107 (1986) 239 seems surprised to discover that converts to Christianity in Greece did not change their names, but there is no reason for such surprise.

⁷ "Some Egyptian Bishops and the Date of P.Landlisten," *Atti del XVII Congresso Internazionale di Papirologia* (Napoli 1984) 1135-40.

the 345 used for convenience in my article, I would incline now to use 350, but that matters little to the argument.⁸

2. Christian Names and Christians

The number of Christians must surely have been larger than the number of people we can identify as Christians on the basis of their names. For the sixth, seventh, and eighth centuries, I established that the number of Christian names must be multiplied by a coefficient of about 1.5 to get the number of Christians; that is, in a virtually wholly Christian population, only about two-thirds of the people had identifiably Christian names. I then applied this coefficient to the fourth-century figures, though in concluding I suggested that the results were probably a bit too high. It is this step in the argument which Wipszycka rejects, and in rejecting it she sets forward the timetable of the conversion of Egypt substantially. This rejection, I must point out, is based on virtually no evidence. It rests, rather, on Wipszycka's belief that the break between the "mentalité" of the Christians of the fourth century and those of later times is so great that an analogical application of a ratio which is valid later is simply impossible. "Nous n'avons aucune possibilité de trouver des critères qui nous permettraient d'établir dans quelle mesure il faut augmenter ou diminuer le coefficient de 1,5, qui ressort des calculs concernant les VIe-VIIIe siècles" (p. 179).

This is simply wrong, for Wipszycka has passed over in silence a critical stage in the argument which tends to confirm the utility of the coefficient in rough terms (which is all that was claimed for it). The attentive reader will recall that the figures given in each body of evidence are derived from the number of persons listed as currently active, "sons" as opposed to "fathers". But there are also persons with names not identifiable as Christian who appear in these texts bearing patronymics which *are* identifiable as Christian. In a period in which rapid conversion to Christianity is going on, it is extremely unlikely that very many of these represent cases where the father was named by Christian parents but himself apostasized before naming his own son. We may reasonably conclude, therefore, that these persons with Christian patronymics are to be added to the number of Christians. In the Abinnaeus archive and in the Hermopolite land registers, we find 12% Christian names among sons, but 18 and 19% Christian names among sons plus fathers. Our coefficient of 1.5 would have predicted 18%. From these figures it is manifest that a coefficient of 1.5 is not chosen from "le domaine de la pure imagination," but rather corresponds reasonably to the evidence. That it was invariable from one population to another, I would not (and did not) claim, but a range around 1.5 is neither unreasonable nor imagined, but rather based on evidence. Moreover,

⁸ Klaas Worp, cited by Wipszycka for his continued support of an earlier date, has kindly authorized me to state that he now, with the evidence of Van Gucht's article, accepts a date in the late 340's or later for these registers.

these ratios are *minimum* coefficients, because there must have been some Christians who did not have identifiably Christian names and who were not the children of parents with Christian names, as Wipszycka herself points out. We do not have any means of quantifying this additional group, but obviously it can represent only an addition to the baseline furnished by the sons plus fathers figure.

Wipszycka does offer one piece of evidence to demonstrate "le danger que comporte l'application de la méthode proposée par Bagnall." On examination, it suggests, rather, that she has not understood the method. *P.Berl.Bork.*, the list of real property at Panopolis, contains only two Christian-named persons, both named Maria. But, says Wipszycka, we would commit "une faute grossière" if we drew conclusions from the fact that there were only two Christian names among 436 total names, for the same list contains six deacons who bear pagan names. True, but irrelevant: These names are pertinent not for the percentage of Christians in the time of the document (probably 315-320), but for that a generation *earlier*, or around 280-285. And I would not be surprised if in 280-285 the Christian population of Panopolis (later a longlasting pagan stronghold) were less than .5%. The six deacons three decades or so later indicate that the the Christian community had grown rapidly in the meantime, a point in complete accord with the picture of the rate of conversion in the second decade of the fourth century which our statistics indicated.

The same point is exemplified in a list of Melitian bishops in 325, though it is a small sample: Of 29 names, only Peter, Isaac, Amos, Moses, Ephraim, John, and perhaps Pininouthes are Christian, or no more than a quarter of a group which was by definition all Christian.⁹ The bishops mentioned in the paschal letter of 339 offer two Christian names out of eleven men, or maybe four of nineteen counting predecessors listed; in the epilogue to that of 347, of 35 total names, nine are Christian (Munier [see n.9] 9-10). While the samples are all very small, they fall generally in the 20-25% range. What the average age of a bishop was, I do not know (40?), and we are dealing with both newly appointed and superseded bishops. In the 290-310 period, certainly far fewer than this percentage of the general population received Christian names; but it would hardly be surprising that a large share of the church's leadership came from those brought up in the church. In any case, the percentage of Christian names in a list of 325 is completely irrelevant to the number of Christians in 325; it is pertinent only to the number of Christians a generation before.¹⁰ The index of bishops' names from the Council of Ephesus shows that even in the 430's a minority of bishops bore identi-

⁹ H. Munier, *Recueil des listes épiscopales de l'église copte* (Publ. de la Soc. d'Arch. Copte, Textes et Documents, Cairo 1943) 2-3.

¹⁰ A comparison to the list from the Council of 314 at Arles may be interesting; though the manuscripts vary somewhat, no more than 11 of 73 names of bishops, deacons, presbyters, lectors, and exorcists can be classified as Christian (the criteria in Gaul might not be the same as

fiably Christian names.¹¹ Born in the late fourth century for the most part, and mostly (one presumes) in the faith, they suggest to us that if anything the economically better-off classes which supplied most of the bishops were less likely than the ordinary people of our papyri to give distinctively Christian names to their children.

While the example from Panopolis then fails utterly to support Wipszycka's point, it does engage us in a broader generalization which she makes: "Je ne crois pas que l'augmentation du nombre des noms chrétiens résultât directement de la conversion. Il me semble que le changement du style de vie, la naissance de goûts nouveaux a joué un rôle plus important. Les gens avaient tendance à abandonner les noms païens, non pas parce que ceux-ci les gênaient, mais parce que les noms chrétiens leur plaisaient davantage. Ils le faisaient dans la mesure où ils se sentaient libres dans leurs choix onomastiques (souvenons-nous que la coutume imposait aux pères de donner à leurs enfants les noms des grands-parents). Evidemment, ce changements des goûts était une conséquence de la christianisation de la société; mais il s'est produit avec un net retard." No evidence is offered for this statement; but let us suppose for the moment that it is true and explore the consequences.

The burden of Wipszycka's closing comment is that a growth in the use of Christian names for children will not have taken place right after the parents' conversion, but at some distance in time. We may therefore suppose that a *smaller* percentage of the children of Christian parents in the fourth century will have borne Christian names than in the sixth century, when the habit of giving Christian names in a thoroughly Christianized society was well established. If so, the coefficient to be used in converting the percentage of Christian names to the percentage of Christian children will be *higher* for the fourth century than for the sixth; that is, higher than 1.5. It is simply not logically possible to entertain Wipszycka's view of parents' habits of giving names at the same time as her implied view that the coefficient must have been lower than 1.5.¹²

in Egypt, but this figure errs on the side of inclusion): C. Munier, *Concilia Galliae a.314-a.506* (Corpus Christianorum Lat. 148, 1963) 15-17.

¹¹ *Acta Conciliorum Oecumenicorum I: Concilium Universale Ephesenum*, i.8, 14-25. The criteria for identification seem to me less clear here, but common names from Greek and Roman usage, with no obvious Christian connotations, outnumber identifiably Christian ones by almost two to one; naturally, these are mostly not Egyptian.

¹² Although she denies that there are any criteria for telling if the coefficient was higher or lower than 1.5, she proceeds on (to belief in a slower rate of conversion) as if she had demonstrated that it must have been *lower*.

3. Difficulties

If these objections of Wipszycka's seem insubstantial, a far more serious point which she does not bring up must be raised in connection with *CPR V 26*, the great codex from the Hermopolite village of Skar. For this text I used the date 388 proposed ten years ago by Sijpesteijn and me in an article (*ZPE* 24 [1977] 11-24) shortly after the publication of the text. New evidence and further study, however, have persuaded me that 388 (a date based on the value of gold stated in denarii) is more properly a *terminus post quem* than a firm date,¹³ for it seems that there was no major change in the valuation of gold against the denarius for quite some time after 388.¹⁴ There is, moreover, specific evidence in *CPR V 26* to point to a later date, namely the appearance several times of keratia as subdivisions of the solidus. Our limited evidence suggests that the keration was not introduced as a currency unit (as opposed to a unit of weight) until the early fifth century, the first certain example being in 433.¹⁵ Obviously a date to 433 (like 388, a second indiction) for *CPR V 26* would move a major block of onomastic evidence forward by 45 years. Moreover, even 433 is a *terminus post quem*. Johannes Diethart has published as *CPR IX 43a* an edition of a protocol found on the papyrus out of which the codex was made. While it bears no date, and the official mentioned in it cannot be identified, it has close resemblances to others which Diethart dates to the middle of the fifth century. The argument is not very exact,¹⁶ and the time-lag between protocol and use of the papyrus for the codex is not able to be estimated. Still, a date before 448 is difficult to defend, and a cycle or two later may be more likely.

Now the Skar codex offered a ratio of "sons plus fathers" to sons of only 1.27, lower than that found in the fourth-century archives mentioned above. But the later date for the codex, paradoxically, makes it more probable that the percentage of Christians in this village was in actuality close to 100%. If so, the coefficient of total Christians to Christian sons' names will be not 1.5 but 1.9. It will thus if anything confirm what we discovered earlier:

¹³ I am indebted to Jean Gasco and Klaas Worp for pointing out the arguments in favor of a later date for this papyrus.

¹⁴ See now my *Currency and Inflation in Fourth Century Egypt* (BASP Suppl. 5, Atlanta 1985) 47-48.

¹⁵ See *Currency and Inflation* 10. A text as late as 430 uses fractions of a solidus instead of keratia. The second indiction in *CPR V 26* could indicate 433/4 and thus coincide exactly with the first known use in *P.Amst.* I 53; but, as we shall see, even 433 may be too early.

¹⁶ Diethart argues that *CPR IX 43a* (and 43) are palaeographically similar to 39 and 41-42; 39 has the same official in it as 40, which is datable to 451. Although *CPR V 26* is palaeographically conservative, Diethart favors a date in the second half of the fifth century or later. I am inclined to accept a date around 450 for the protocol in *CPR V 26* (i.e., *IX 43a*), but I take seriously the palaeographical conservatism of the codex and would put it not much later than this date.

Wipszycka's discussion has had the valuable effect of focusing attention on the fact that almost all possible sources of error in the determination of a coefficient to the percentage of Christian names found in the texts will work to make it appear lower than it properly is. A true coefficient lower than 1.4 seems to me extremely improbable, while one higher than 1.7 bumps up against arithmetical limits in some cases but may be true in others. If one takes this range into account along with the probable changes in dating for two of our bodies of evidence discussed above, the following table emerges:¹⁷

Date	% Names of sons	% Names + fathers	% Names x 1.4	% Names x 1.5	% Names x 1.7
278	7	NA	9.8	10.5	11.9
280	9	NA	12.6	13.5	15.3
313	12	18	16.8	18	20.4
315	12	19	16.8	18	20.4
393	33	NA	46.2	50	56.1
428	52	66	72.8	78	88.4

The moving forward of the Skar codex (I use a date of 463 for this purpose) obviously has serious effects. Since a coefficient of 1.7 is probably too low for the Skar codex, we may reasonably suppose that Christians were well more than a majority before the end of the century; but it is hard to be more precise than that. The absence of any usable body of data for persons born in the period between 315 and 393 becomes acute. The main part of the Archive of Papnouthis and Dorotheos (in *P.Oxy.* XLVIII), with an average date perhaps in the area of 365,¹⁸ yields 141 entries, but because only 23 have patronymics, little can be done with them; distinguishing actual individuals is mostly impossible. For what it is worth, six of the 23 have Christian names, four more Christian patronymics: 26.1% of sons, 43.5% of sons plus fathers. For a period (for their birth) around 330, those figures would fit well enough into the table above—but only because we lack other evidence for the period. And the sample is so small as to prevent any confidence in these results.¹⁹

¹⁷ I have omitted here the data from *P.Cair.Isid.* 9, which Wipszycka (p. 174) regards as the one dubious body of evidence. I am by no means persuaded that she is right about the cohesion of Egyptian villages (the Sakaon archive, even more than that of Isidoros, suggests a perpetual state of feuding), but these data are not important to the argument here.

¹⁸ Omitting nos. 3384-3389, which do not pertain to the main period of the two men's work which generated the bulk of the documents.

¹⁹ It is worth pointing out that if patronymics are disregarded and all entries tabulated, the figure for Christians is smaller, 18%. But the entries without patronymic differ radically from those with in other respects, such as the ratio between pagan and unassignable names (43.5%:30.4% with patronymics, 33%:49% without). Another statistical point of which we should not

For the moment, I do not see how to make further progress. The method, I think, is vindicated. But the evidence is less helpful than it seemed. The urgency of fourth-century prosopographies is all the more obvious.²⁰

Columbia University

Roger S. Bagnall

make too much is that in the sample with patronymics, the Christian fathers' names point to a minimum coefficient of 1.67!

²⁰ I have not yet seen any sign of fulfillment of the hope expressed by Ramsay MacMullen (*Christianizing the Roman Empire* [New Haven 1984] 157 n. 41) to offer his own interpretation of the papyrological data with which we are concerned. MacMullen (83) suggests that the numerical balance tipped toward Christianity in Egypt in the 390's. The character of the evidence he cites is hardly compelling.