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Commentary on *Hydrogeologic cross-section through Sunshine Valley, Taos County, New Mexico,* by W. K. Summers and L. L. Hargis

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W. K. Summers and L. L. Hargis take issue (Summers and Hargis, 1984) with two interpretations given in my 1959 report on the hydrogeology of Sunshine Valley, New Mexico (Winograd, 1959). These interpretations are with regard to: a) the extent of perched water beneath Sunshine Valley; and b) the presence of lakebeds beneath a portion (T. 30 N., R. 12 E.) of the valley. I will demonstrate that their first criticism came about from a cursory reading of my report. With regard to the second criticism, I will concede that some of my evidence for lakebeds in the eastern half of the T. 30 N., R. 12 E. is equivocal, but strong evidence exists to warrant continued belief in the presence of lakebeds beneath the western half of this township.

Extent of perched ground water beneath Sunshine Valley

As a result of focusing on a single illustration, coupled with a cursory reading of my report, Summers and Hargis first misinterpret my findings on the occurrence of perched water, and then they proceed to attack their misrepresentation as incorrect.

The misrepresentation (undoubtedly unintentional) of my work begins with Summers and Hargis' figure 3, which is labeled "Winograd's cross section." However, the key words "perched water table" and "regional water table" (which extend across the center of the cross section they published) *do not appear* on my original illustration (Winograd, 1959, plate 3)! Somehow, Summers and Hargis got the notion that I was reporting the occurrence of perched water throughout the valley, with an unsaturated zone separating "two water tables." They state on page 247: "As the figure shows, he recognized two water tables; one he labeled as 'Water table in alluvial sediments. Semiperched where underlain by saturated andesite–basalt. Perched in vicinity of test hole 30.12.21.111'. The other he labeled as 'Water table in andesite–basalt. Solid line indicates water table; dashes indicate piezometric surface of water confined in andesite–basalt under subnormal artesian pressure.' " On page 248 they state: "Winograd believed that beneath the water table in the alluvium are rocks in which the degree of saturation is less than 100%." And, on page 245 they "concluded that perched conditions are much less extensive than Winograd thought" (Summers and Hargis, 1984).

In my paper is a lengthy interpretive chapter (longer, in fact, than the entire Summers and Hargis text) entitled, "Relation of water in alluvial sediments to water in andesite–basalt" (Winograd, 1959, pp. 28–30). Although I cannot repeat its contents here, in this chapter three key terms, "semiperched" (which is *not* synonymous with perched), "negative confining stratum," and "subnormal pressure surface" are carefully defined and also illustrated (Winograd, 1959, fig. 9, p. 25). These terms are used repeatedly throughout my text, as well as on key illustrations including plate 3. An understanding of the meaning of these words—particularly the difference between perched and semiperched—is essential for a correct interpretation of my hydrogeologic arguments regarding the relationship of ground water in the alluvial sediments to water in the andesite–basalt lavas (hereafter called lavas). Unfortunately, Summers and Hargis apparently did not understand these key terms. More to the point, I stated *twice* in this chapter that no unsaturated rock separates ground water in the alluvial sediments from that in the underlying lavas. For example, on page 29 of my paper, I stated: "The upper body of water within the alluvial sediments, illustrated in figure 9C, is considered to be semiperched with respect to the lower body within the lava. *No unsaturated rock is present between them and each is considered part of the same zone of saturation*" (italics added). I also stated on the same page: "No zone of aeration exists between the upper and lower saturated zones." I can only conclude that Summers and Hargis may not have read this chapter. In only one very restricted area, namely at the western edge of my cross section, specifically west of well 30.12.16.433, did I state that perched water existed. While I will acknowledge that the term "semiperched" (an admittedly awkward term, which was, however, acceptable usage in the mid-1950's) can be confused with "perched," this is hardly an excuse for Summers and Hargis not carefully examining my text to see whether "semiperched" was defined and how it differed from "perched."

Briefly, I did not recognize "two water tables" or extensive perched water in Sunshine Valley, and, therefore, Summers and Hargis' major criticism is meaningless. I did, however, recognize extensive *semiperched* water in the alluvial sediments where they overlie saturated lavas; no unsaturated zone separates the alluvial sediments from the lavas.

The major misunderstanding outlined above led Summers and Hargis to several subsequent misunderstandings. They state on page 248: "Moreover his water table implies movement of water toward the river through the sediments. However, no springs occur in the wall of the Rio Grande gorge, and we have no reason to believe that water evaporates or transpires from the water table, so the water must be moving downward." Again, these topics are discussed in my paper in the chapters entitled "Movement and discharge" and "Relation of water in alluvial sediments to water in andesite–basalt." *The whole concept of semiperched water revolves about the presence of downward-directed hydraulic gradients and flow!* I also observed the "absence of springs in the box canyons tributary to the Rio Grande Canyon" (Winograd, 1959, p. 33) and considered this additional evidence for downward drainage of ground water from the alluvial sediments into the underlying lavas. In retrospect, it would have been helpful to readers if I had arrows depicting downward and lateral flow on my plate 3; however, such arrows *are* shown on my figure 9!

Are lake deposits present?

I postulated the presence of fine-grained lakebeds in T. 30 N., R. 12 E. (Winograd, 1959, pp. 19–20 and fig. 8). Summers and Hargis do not believe lake deposits exist in this area, though they say "Ponds or playas may have developed locally . . ." (Summers and Hargis, 1984, p. 248).

I agree with Summers and Hargis' statement on page 248 when they state that it will be difficult to "establish criteria for defining the top of the lakebeds," particularly, I will add, utilizing drillers' logs. I also will acknowledge that in carefully re-examining the well logs in my report I question whether a lake (or even a playa) existed beneath the northeastern quarter of T. 30 N. R. 12 E. However, the *geologists'* logs of Corps of Engineers' test holes in the western half of T. 30 N., R. 12 E. (specifically, logs for dam-site test holes 29.12.3.422, 30.12.9.422, 30.12.21.111, and 30.12.33.220 found in Winograd, 1959, table 7) indicate the presence of 40–95 ft of fine-grained sediments directly overlying the uppermost lava flows. Moreover, the documented failure to obtain adequate supplies of irrigation water in sec. 16, T. 30 N., R. 12 E. (Winograd, 1959, pp. 24, 26) provides independent hydraulic evidence for the fine-grained nature of sediments beneath this part of the township.

Whether these fine-grained sediments were deposited in a lake, in a periodically flooded playa covering the western half of T. 30 N., R. 12 E., or in a few local playas, as Summers and Hargis believe, is indeed debatable. And, of course, lake and playa environments are gradational. It is impossible for me to learn from Summers and Hargis' figure 1 which of those wells drilled after completion of my field work in 1955 have yielded subsurface information pertinent to the thickness and extent of fine-grained deposits beneath T. 30 N.,

R. 12 E. I note, however, that although Summers and Hargis' figure 1 shows significantly more wells (and presumably more well logs) east, southeast, and south of T. 30 N., R. 12 E. than I had in 1955, they show very little additional well control in that township!

Accordingly, I stand by my suggestion for *widespread* fine-grained deposits (of either lake or playa origin) in the western half of T. 30 N., R. 12 E. Whether or not a lake (or a widespread playa) existed is of more than academic interest because, as I said in my report, "it appears that adequate supplies of irrigation water cannot be obtained from the alluvial sediments in the western half of T. 30 N., R. 12 E., owing to the presence of lake deposits at shallow depths" (Winograd, 1959, p. 26).

Briefly, the presence of widespread lake (or playa) deposits above the lavas beneath the western half of T. 30 N., R. 12 E., appears likely based on *geologists'* logs of Corps of Engineers' test holes and on unsuccessful test drilling for irrigation water within the sediments. I have much less confidence now than I had in 1959 in arguing for the identification of similar deposits in the eastern half of this township. As I suggested in my report, resolution of this question has significant implications with respect to the development of irrigation water supplies (Winograd, 1959, p. 26).

Miscellaneous points

Summers and Hargis infer that I relied on the presence of lakebeds to cause the perching (actually semiperching) of ground water in the alluvial sediments. In their opening paragraph they state: "Our analysis led us to doubt the lacustrine origin and hydraulic-confining function of the fine-grained sediment . . ." (Summers and Hargis, 1984, p. 245). And at the end of their article they state: "The term 'lake beds' implies that the rocks include clays that are less permeable than the alluvial clays elsewhere in the valley, that the clay beds are more continuous elsewhere, and that as a result they should cause 'perching' of ground water" (Summers and Hargis, 1984, p. 248). However, on page 29 of my report, as well as in my figure 9C, I point out that the upper portions of the lava flows may themselves act as negative confining strata; the presence of lakebeds is not essential, even though they certainly function as negative confining strata when present. As mentioned above, the term "negative confining stratum" is carefully defined in my report as is the term "semiperched."

Summers and Hargis ask "What happens to the water table in the basalt at its eastern extremity?" (1984, p. 248), and then they go on to say "The head distribution depicted in Figure 2 shows water moving from the water table at higher altitudes toward the river, taking some of the mystery from Winograd's interpretation of recharge . . ." That water moves generally westward across the valley to discharge points in the Rio Grande is clearly shown by the potentiometric contours on my plate 2 and is discussed at length in my chapter entitled "Movement and discharge" (Winograd, 1959, pp. 32–34). I quote from the summary paragraph of this chapter (p. 34) in an attempt to help solve Summers and Hargis' "mystery."

> By way of summation, ground water within the alluvial sediments moves from the recharge areas along the east side of the valley toward the west. In the zone of interbedded alluvial sediments and lava most of the ground water in the sediments is slowly discharged into the underlying lava. Some discharge occurs also through evapotranspiration in the south-central part of the valley where the water table is very shallow. Ground water within the lava moves from the areas of recharge, which underlie saturated alluvial sediments, toward the Rio Grande, into which it discharges as spring flow.

Summers and Hargis depict the flow system using equipotential lines on a cross section. Certainly, if my work had been done in the 1970's (or even in the 1960's) instead of 1955, I would have used equipotential lines also or perhaps a flow net to depict ground-water movement; such depiction was not in vogue in the mid-1950's. Nevertheless, my text and illustrations clearly show that the flow model I envisioned was just as "dynamic" as Summers and Hargis', and that "In the immediate vicinity of the river the vertical component of hydraulic gradient is directed upward" (Summers and Hargis, 1984, pp. 247–248). Indeed, I devoted still another chapter ("Ground water accretion to the Rio Grande and the Red River") and an illustration (fig. 11) to a discussion of the magnitude of ground-water discharge to the Rio Grande.

With regard to depiction of ground-water movement, it would have been very instructive if Summers and Hargis had elected to draw their cross section along the same line as mine instead of 2–4 miles south of my section. If they had done so they would have had the opportunity to demonstrate a phenomenon frequently illustrated in textbooks (*see*, for example, Freeze and Cherry, 1979, p. 198, fig. 6.4), but rarely documented in the field; namely, the refraction of equipotential lines in a region underlain by aquifers of differing transmissivity. That the transmissivity of the lavas beneath the region of my cross section is significantly larger than that of the overlying alluvial sediments is strongly suggested by an hydraulic gradient in the lavas, which is about one-fifth as large in the alluvial sediments (*see* Winograd, 1959, plate 2), and by the "fact" (attested to, after all, both by Summers and Hargis and Winograd) that all the water in the sediments has to pass through the lavas enroute to the Rio Grande. Thus, equipotential lines connecting *both* aquifers would (at the latitude of my cross section) have to be refracted to reflect the differing transmissivities.

Summary

W. K. Summers and L. L. Hargis have misinterpreted the contents of my paper with regard to the extent of perched ground water beneath Sunshine Valley, New Mexico, and consequently also with regard to the local and regional hydrodynamics of this valley. I can only conclude that they never carefully read several key chapters of my report in which the terms "semiperched," "negative confining stratum," and "subnormal pressure surface" were defined, and where the flow-system dynamics are described in considerable detail! Therefore, their brief "re-interpretation" of ground-water flow, though couched in equipotential terminology, does not really differ from mine; the flow system I depicted in the mid-1950's is just as "dynamic" as that envisioned by Summers and Hargis in 1984! I suspect that the misinterpretation of my work by Summers and Hargis may have arisen by their giving excessive attention to my cross section (plate 3) and insufficient attention to the accompanying text. A complex hydrogeologic section, however carefully designed, cannot stand alone.

With regard to the presence or absence of lakebeds in T. 30 N., R. 12 E., I agree that evidence for the presence of lakebeds (or extensive playa deposits) in the eastern half of this township is weak, but I maintain that the geologic and hydraulic evidence for such deposits in the western half of this township remains strong. Semiperching of water in the alluvial sediments does not necessarily depend, in any case, on the presence of fine-grained sediments, be they lake or playa deposits.

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