

during filling or accidentally entrained air. This could be accompanied by sudden noise, which explains the expression ‘force of air’. Why, then, are these *colliviaria*/towers known only at Aspendos? First, not many *venter* bridges still stand, so we do not know if there were others similar to the bridge at Aspendos. Second, where the inverted siphon was much deeper than at Aspendos, building towers was not feasible. Third, there was a much simpler way to get rid of air from an inverted siphon — by building the *venter* bridge with a slightly ascending incline in the direction of flow. Thus any air would be transported with the stream to the end of the siphon and released there. This arrangement is used in the Beaunant siphon of the Gier aqueduct to Lyon.

Horizontal turns

The towers at Aspendos required much material and labour for their construction. Reinforcing the pipeline at the turning points could solve the problem of sideways inertial thrust much more economically than by building costly towers. The Roman engineers were aware of the danger of disruption of the pipeline at vertical turns (Vitr. 8.6.6). Possibly they knew too of the danger at horizontal turns and knew that this danger could be avoided by relieving the pressure in the pipeline at the turning point by bringing it up to the level of the hydraulic gradient — which thereby necessitated the erection of towers. Probably some building manual, itself the result of trial and error, caused them to solve the problem of sideways inertial thrust by erecting towers at the horizontal turns, rather than by reinforcing the structure, which would certainly have been more economical. In brief, with its substructure and *colliviaria* Aspendos follows Vitruvius’ recommendations for an aqueduct crossing a valley.

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Response to R. Tybout “Roman wall-painting and social significance”

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In his article in this issue, R. Tybout has some harsh words (38-39) for an article by S. Yerkes published in *JRA* 13. T.’s reaction seems to involve not so much a scholarly disagreement as a major misunderstanding of Yerkes’ paper. I would like to offer a brief corrective to his comments.

T. seems under the impression that the identification of Vitruvius’ *monstra* is the main — or only — point of Yerkes’ essay. This is probably because of the title. The paper is a radically reduced presentation of the argument of the author’s master’s thesis which was entitled “Neo-Attic motifs in Roman painting”. And the identification of some of the *sources* for the *monstra* — as motifs in much earlier Roman marble furnishings — is actually the main thrust of her argument. Her original title for the article also included a reference to “neo-Attic motifs” but that was omitted by the editor after it was submitted to *JRA*. T. is dismayed that Yerkes fails to cite the chapter on Vitruvius 7.5.3 ff. in W. Ehrhardt’s monograph *Stilgeschichtliche Untersuchungen* (T. 1, n.1). Now, Yerkes is well aware of this work and cites it in her thesis, together with another paper Ehrhardt published on “Vitruv und die zeitgenössische Wandmalerei” in *KJb* 1991. So she could — and perhaps should — have included Ehrhardt’s work in her notes, as T. suggests; perhaps including fuller references to other past scholarship too. But T. goes on to make the extraordinary claim (38 n.30) that Ehrhardt’s chapter renders Yerkes’ observations “largely superfluous”. I cannot follow T. here, for Ehrhardt’s essay actually has a quite different aim and focus; and the differences are worth underlining.

Ehrhardt starts from the assumption that one can only look for the *monstra* in wall-painting before 22 B.C. (his date for the completion and publication of Vitruvius’ book). In his view, “Any later walls cannot be directly employed for illustrating the passages of Vitruvius under