# Report on "*Proof of uniform convergence for a cell-centered AP discretization of the hyperbolic heat equation on general meshes*", submitted by

E. Franck, B. Despŕes. Ch. Buet, Th. Leroy to `Math of Comput.`

## 1 General comments

We are given a very technical follow-up paper to the authors' previous work (cf. Numer. Math, ref. [5]) devoted to AP schemes (see the interesting distinction between "uniformly AP" and "AP" in the introduction) approximating the so–called "$P_1$ model" on 2D unstructured grids. In particular, the focus is now on the (big) issue of precisely quantifying the convergence rates as both the stiffness $\varepsilon$ and the grid size $h$ tend to zero; the most interesting regime for practical applications being the (stiff) one for which $\varepsilon \ll h$. Accordingly,

- like in [5], the 1D case is still used, in §2, as a preliminary guideline to present and set up the general methodology on nonuniform grids,
- and in §3, the full 2D case is addressed for "finite-volumes nodal schemes" (FVNS) which the authors advocate for this type of applications.

Clearly, **the result established by the 4 authors is very significant, for both the rigorous theory of multi-D AP schemes (a concept which is unfortunately often fuzzy and ill-defined) and the applications to e.g., radiative transfer computations on 2D distorted meshes**.

This being said, some features hamper my will to recommend an immediate publication of the present manuscript: the most salient one being its poor presentation, between a poor English, an excessive use of "constant $C$" (which seem to often depend on time $T$, so it is meaningful to let it explicit for numerical purposes) and the choice of not using LaTex's commands `appendix`, `itemize` and `enumerate` which allow to streamline proofs and ease the reader's task.

## 2 A massive overuse of "Constant $C$" and the issue of the estimates' time-dependence

Using the Landau's notation $\mathcal{O}(1)$ or $C$ is perfectly licit when it comes to streamline the expression of intricate estimates. However, this shouldn't be a rhetoric trick to hide a dependence in time, or with respect to other meaningful parameters, under the carpet:

- Page 7, Proposition 2.2: as far as I understand, the proof yields $C = T$. So there's no need for this first constant. Agree ? Same for Prop. 3.1 ?
- The former comment may seem anecdotal, but it reveals (at least) 2 interesting things:

1. assuming $p$ smooth, the approximation (on continuous solutions) is reliable mainly for $t \ll \frac{1}{\varepsilon}$.
2. all the remaining Propositions (pages 11–14) ask for the Gronwall lemma in their proofs: hence we obtain bounds growing exponentially in time, suggesting that numerical approximations (discrete solutions) are now reliable mostly for $t \ll \log(\frac{1}{\varepsilon})$.

Hence I think that the authors shouldn't try to hide this apparent discrepancy under the carpet, because it may be checked numerically rather easily: it suffices to run the code which produced the Figure 6 several times, and scrutinize whether we see a growth in time which is linear, polynomial or exponential. Moreover, this will give valuable information on the sharpness of the error bounds rigorously proven.

- Page 8, the estimate (27): the proof is quite easy, so why not displaying it in order to wipe off the $C$ ? It suffices to set up a classical computation, from e.g. DiPerna,

$$\frac{1}{2}\frac{d}{dt}\int_R p^2.dx + \frac{1}{\sigma}\int_R |\partial_x p|^2.dx = 0 \;\Rightarrow\; \|p(t,\cdot)\|_2 \leq \|p_0\|_2,$$

which yields, without introducing any $C$,

$$\int_0^T \int_R |\partial_x p|^2.dx.dt \leq \frac{\sigma}{2}\|p_0\|_2^2.$$

- Page 11, proof of (32)–(33):

1. "one can find a constant $C \geq 0$ such that the next term [...]". Is this $C$ the one of the grid introduced in Hypothesis 2.1 ?
2. "The last term in (28) [...]": seems like it's (29) instead.
3. "using a Cauchy-Schwarz inequality [...]": in my opinion, one should invoke instead Jensen's inequality. Indeed, let's denote by $Pu = \frac{1}{|C_j|}\int_{C_j} u(x).dx$, the projector on piecewise-constant functions,

$$\sum_j \Big| Pu^\epsilon \cdot |C_j| \Big|^2 = \sum_j |C_j|^2 \cdot |Pu^\epsilon|^2 \leq \sum_j |C_j|^2 \cdot P(|u^\epsilon|^2) \leq h \cdot \|u^\epsilon\|_2^2,$$

and the first $\leq$ comes from Jensen thanks to $\eta(Pu) \leq P(\eta(u))$ for any convex function $\eta$, the second comes from Hypothesis 2.1.

- Pages 11–12, in Proposition 2.6, the $C(T)$ is an exponential of the time $T$: it must be indicated and the discrepancy with respect to the linear growth found in Proposition 2.2 should be commented. Moreover, concerning the estimate (35): the statement is correct, except that it would require a little bit of explanations, as it relies on a bootstrap-type argument based on estimate (27), which wasn't stated really correctly (as already seen).

- Pages 20–27: the statements and proofs of Props. 3.4 and 3.5 are A MESS!
  1. When studying the different terms in $\mathcal{E}'(t)$, please use either `itemize` or `enumerate`, and perhaps forward heavy calculations to an Appendix in order to lighten stuff,
  2. Props. 3.6–8 are indeed "technical Lemmas" and should be stated accordingly (cf. comments on the 1D case of Prop. 2.8). Their proofs might be forwarded too.
  3. the 1D comment about Jensen's inequality appears to apply here again, bottom of page 26.
- Page 36, §4: perhaps a bibliographic reference for the solution of the telegraph's equation may be useful to the reader. Moreover, there is a conflict of notation between the $\gamma$ used here and the one invoked in the proofs on pages 25-26-27.
- As said before, there's a question which should be addressed, that is the rate of amplification in time of the errors: the Gronwall's Lemma produces a time-exponential rate, so it's interesting to check out numerically if this feature is a discrepancy of the analysis, or if it holds practically. Such a point was addressed, in a totally different context of scalar balance laws:

  D. Amadori, L. Gosse, *Transient $L^1$ error estimates for well-balanced schemes on non-resonant scalar balance laws*, J. Differential Equations **255** (2013) 469–502.

# 3 Comments and suggestions

- Pages 2–4: with an Introduction that long, it makes sense to insert "subsections" entitled "Precisions on AP discretizations" (right after Remark 1.2), and "Organization of the paper" (right after the proof of Prop. 1.3).
- Page 5: is there a connection between "nodal-based" FV schemes (advocated in the Intro.) and the classical Godunov scheme where 2D Riemann problems are to be solved at each node of the computational grid ? See for instance the elementary case of a non-resonant scalar balance law in,

  L. Gosse, *A two-dimensional version of the Godunov scheme for scalar balance laws.* SIAM J. Numer. Anal. **52** (2014) 626–652.

  If so, the sentence in the Introduction "it strongly questions ..." wouldn't be that surprising. Moreover, on page 27, the authors speak about the "incorporation in the approximate nodal Riemann solver", thus reinforcing this vague impression of similarity with 2D Riemann solvers.

- Page 6: The big footnote is confusing: in my opinion, it would be better to state clearly that, in order to build a discretization able to produce a 2D AP numerical process, the authors choose to renounce to the well-balanced property of the original 1D Gosse-Toscani scheme (except on uniform Cartesian grids) while retaining only its AP character. This explains why the algebraic expressions differ from each other.
- Page 7, Proof of Prop. 2.2, the notation choice $u^\epsilon = -\frac{\epsilon}{\sigma}\partial_x p^\epsilon$ is confusing: according to (13), a notation like $v^\epsilon$ looks better.
- Page 10, last lines, what is the definition of $\mathbf{u}^\epsilon$ ? This notation doesn't appear previously.
- Page 14, the Proposition 2.8 is indeed a (technical) Lemma, and this should be stated accordingly.
- Page 14, end of the 1D section: according to what is displayed numerically in the §4, Figure 6, we see a second-order accuracy (in $h$) for $\varepsilon \simeq h^2$. This appears to be an extension in 2D of an observation made in a paper devoted to "TAHO schemes", see especially §7.1.2 in,

  D. Aregba, M. Briani, R. Natalini, *Time Asymptotic High Order Schemes for Dissipative BGK Hyperbolic Systems*, arXiv:1207.6279.

- Page 15, Proposition 3.1: same comment than for Proposition 2.2.
- Page 15, eqn. (45), there is a "(" missing in the first line.

# 4 Minor issues and typos

1. Page 1, right after eqn. (1), "the $P^0$ finite volume schemes": this creates a confusion of notation with the $P^\varepsilon$ problem, having the $P^0$ problem as a limit $\varepsilon \to 0$. Please use another font here.
2. Page 7, Remark 2.4, "in this work consists in analyzing".
3. Page 8, "quantities related to the diffusion scheme": please correct.
4. Page 11, (17) is not "initial data".
5. Page 14, "Since the scheme being stable" ($\to$ is)
6. Page 16, "the mesh serves" ($\to$ is used) and "one seeS at once [...]".
7. Page 20, Proposition 3.4, "There exists [...]"
8. Page 36: there is again a conflict of notation between the "$\gamma$" there and the one formerly used as a "free parameter" in pages 23–27.

9. Page 38: what is the meaning of "(M and MKT)" in reference [17] ? Moreover, the reference [14] was published in 2002, not in 2000.