Response to Reviewer 1: Our comments are provided in blue. Text modifications are provided in green.

This manuscript delves into the utilization of the Accelerated Pseudo-Transient (APT) method for tackling quasi-static elastic, viscoelastic, and coupled hydro-mechanical problems. The study not only derives but also rigorously tests the numerical APT formulations tailored for these specific problem sets. Introducing novel dimensionless parameters (St and I1, I2) for the APT method in the context of elastic and coupled poroelastic equations marks a notable advancement. The manuscript showcases the efficacy and adaptability of the proposed APT method through high-resolution 2D and 3D nonlinear modeling results. These simulations vividly illustrate the method's flexibility and efficiency in handling complex geoscience scenarios. This contribution of the APT method to the modeling of realistic geoscience problems is significant and warrants publication in GMD.

While recognizing the manuscript's importance, I acknowledge that certain sections suffer from unclear or confusing descriptions, likely stemming from the writing style and flow. Therefore, I recommend substantial revisions to enhance clarity and coherence throughout the manuscript. This includes addressing the major modifications outlined and attending to various smaller edits that may be necessary for improved readability and comprehension.

Thank you for your thorough and constructive feedback on our manuscript. We are very grateful for your recognition of the significance of our work, especially the introduction of novel dimensionless parameters (St and I1, I2) in the context of elastic and coupled poroelastic equations, as well as the validation of the Accelerated Pseudo-Transient (APT) method for complex geoscience problems.

We fully acknowledge your concerns regarding the clarity and flow of certain sections, and we take this feedback seriously. We are committed to revising the manuscript to enhance both clarity and coherence. We will carefully address the major modifications you've highlighted and ensure that the smaller edits necessary for improved readability are also attended to.

Our revisions will focus on refining the descriptions and improving the overall writing style to ensure that the important concepts, methodologies, and results are communicated more effectively. Once completed, we are confident that the manuscript will better reflect the rigor and importance of the work, as well as meet the high standards of *GMD*.

Thank you again for your valuable insights.

Sincerely,
Yury Alkhimenkov and Yury Podladchikov

Below are the comments and edits from my sides, with bold text for the major ones.

Line 90-95. and 100-105   This description of 1st order and accelerated PT method is not clear or correct.   converges to 0, suggest vx to 0. It does not make sense.   For APT, you should involve 2nd derivative of Vx like in Eq.6) and Eq.7) of Rass 2022, since you cite it. But it is not clearly stated.   Correct this!

We appreciate the reviewer's comments regarding the different versions of the pseudo-transient method. This section is rewritten and corrected. Correct references were added.

Line 225:   "Naïve" does not sound good here!   " that there are minimal modifications to the original formulation of" is not a good description for this scheme.   I think "Elegant APT scheme " has even smaller modifications (only refine G).   Clarify this!

We agree with the reviewer's suggestion and have revised the text accordingly. We have retained the phrase "APT scheme" in the main text while removing the word "elegant."

In fact, I think "Naïve APT scheme" part can be removed. It is just complicated but not naïve! It added only confusion to your description. It is a natural transition from the scheme of elastic equation to the viscoelastic equation (Eq. 26).

We have removed the title "Naïve scheme" and added further explanation regarding its potential applications to Appendix. Additionally, we have removed the term "Naïve" to maintain a more formal scientific tone.

Line 340: As I wrote above, the formulation in 4.14 is needed in 4.1.3. Perhaps you can do some adjustment.

We understand the reviewer's concerns regarding Section 4.1.3. The reason we initially provided the formulation only in Section 4.1.4 is that the decoupled equations yield simpler roots that fit more neatly on the page. In contrast, the full equations for the roots are much more complex, with numerous additional cross terms, which is why we included Maple routines to validate this part.

In response to the reviewer's comments, we have moved Section 4.1.4 earlier, making it the new Section 4.1.3, and added more explanation to clarify the equations. Additionally, we have improved the Maple files to better address the reviewer's concerns. As a result, Section 4.1 has been significantly improved in the current version of the manuscript.

Line 482. I am not convinced about the sensitivity of optimized numerical parameters on boundary conditions from your example. You need more tests to convince people.

We agree with the reviewer that additional examples are necessary to demonstrate that optimized numerical parameters depend on boundary conditions. After extensive investigation, we have updated our statement to: "There is only moderate sensitivity of optimal numerical parameters with respect to boundary conditions." We found that the sensitivity to initial conditions and non-linearities is much more pronounced. We have improved the explanation in this section to reflect these findings.

Line 6; replace "manuscript" with "study".

Done!

study

Line80-85 Eq. 4) I recommend to write it to σ_{xx}- σ_{xx} _old//dt to clarify. The it is similar to the 1st ord PT method case in rass 2022. Clarify that you aim to solve one transient step for this time-dependent problem. Otherwise, it is quite confusing!

We have modified this section and emphasized in the manuscript that the PT method can be used to solve elasticity equations for calculating effective elastic properties. In this case, σxx _old is zero, allowing us to solve the simplified equations. However, in response to the reviewer's request, we have further improved the explanation in this section to provide greater clarity. We removed the reference to Rass et al since in our study we have elasticity (elliptic) equation and Rass et al did not present this.

Line 90-95 100-105. Eq.5) and 6) the same as Eq.4)! Do it like in Eq.7): do time (real physic) discretization of σ_{xx}.

We have t with tilde (which correspond to the pseudo-time) and t without tilde (which correspond to the physical time). The way we present equations are the standard in geophysical (mechanical, math) literature for time partial derivatives.

Line 104. To avoid confusion: " Propagating waves in pseudo physical space."

We improved the wording in the manuscript.

(i) Inertial terms are added into the constitutive relations, (ii) Inertial terms are responsible for wave propagation in pseudo physical time and space (i.e., hyperbolic) and viscous terms (treated as a Maxwell rheology) are the physical quantities.

Line 109: "into the equation stress" is not clear! Remove "stress"?

We improved the explanation.

Inertial terms are added into the constitutive relations

Better description is need for “(ii) these terms are treated as a Maxwell rheology (a viscous damper)”. As I understand, Eq.7a) use a maxwell model of rheology , the item $\sigma_{xx}/\Delta t$ as a viscous part; while the pseudo item is the elastic part.

The reviewer is completely correct and we improved the explanation.

Here we report a modification of the APT method. The solution of the quasi-static elasticity equations can be achieved in two steps. (i) Inertial terms are added into the equations constitutive relations, (ii) Inertial terms are responsible for wave propagation in pseudo physical time and space (i.e., hyperbolic) and viscous terms (treated as a Maxwell rheology) ate the physical quantities.

Line 112: What is the reason to choose =H? Is there a better choice? You said is to be determined. Perhaps would also has an optimal choice.

The reason for our approach is simplicity. This equation involves only one numerical parameter, \(St\), while the other parameters are dependent. If we used a different value for \(\tilde{H}\) instead of \(H\), we would need to modify the entire numerical scheme and adjust the \(St\) value, without any improvement in convergence, as we are constrained by the CFL condition and the single numerical parameter \(St\). There is only one degree pf freedom = one parameter.

Line 115: It is not good to say Eq.7) can be simplified to Eq.8), which could change the equation. But I know σ_{xx_old} as a constant can be ignore for the derivation process. Please write better description for it.

The reviewer is completely correct and we improved the explanation.

For the analysis of the system of equations \eqref{dve_14} we can omit $\hat{ \sigma}$ since the stress $\hat{ \sigma}$ does not change inside the loop over ``pseudo" time $\widetilde{t}$:

Line 142: How about “Instead, the following combinations are needed for the numerical implementation of the APT algorithm.”?

The reviewer is completely correct and we improved the explanation.

Instead, the following combinations are needed for the numerical implementation of the APT algorithm

Line 145-146. Notice “f” is already use as the function name before you write “f is the frequency”

The reviewer is completely correct and we modified the variable name.

\begin{equation}\label{eq_111111}
F(\widetilde{t}, x) = \exp{ \left[\dfrac{(\gamma \, \widetilde{V}_p\, \widetilde{t} + \pi \,\omega \,x \,i)}{L_x} \right]},
\end{equation}

Line 157.   Is “minimum” suitable here ? Line 158 “This minimum reaches maximal value” is confusing…

The reviewer is completely correct and we improved the explanation.

The real parts of the roots γ_1 and γ_2 control the exponential decay rate of the solution \citep{rass2022assessing}, therefore, we are interested in the minimum of these values. This minimum reaches its value when the discriminant is zero:

Line 186.   Fig. 1 show that damping scheme 2 generate different stress with scheme 1.   Why? You did not talk about it in section 2.3.4

In response to the reviewer’s request, we updated the explanation of this section and removed scheme 1 from the main text. The reason for different stress was that scheme 1 (in the previous notation) was not fully correct.

Fig3. There are two subplots, but there is no description of it, neither in the caption or in the main text.

The reviewer is completely correct and we added the explanation and reference in the main text.

It can be seen that the analytical and numerical results are in excellent agreement (Figure \ref{FigVS1}) that validates the proposed approach.

Line 290.   It would be nice to clairfy the (pseudo) physical meaning of I2.

We improved the explanation of I2. It actually has physical meaning in the framework of poroelasticity.

The physical meaning of I_2 is the following: I_2 controls the behavior of the Biot's slow wave, if $I_2 \ggg 1$ the slow wave behaves as a propagating wave, if $I_2 \lll 1$ the slow wave behaves as a diffusive mode.

Line 300. Need a bit explanation on the choice of numerical parameter K1=K_u G1=Gu.

The reason for our approach is simplicity as in the previous section. We added some explanation into the manuscript.

The reason for setting $\widetilde{K}_1={K}_u$ and $\widetilde{G}_1={G}$ is simplicity, since the 4-th order equation has only two degrees of freedom, a different choice of these parameters would simply re-scale the two final optimal parameters.

Line 299 and 335. How come the optimized St value is St=2*pi and St=2.9? formulation? From Fig.5, I can see you do have a formulation. It would be nice to write it down in the main text or appendix.

The values came from analytical derivations using the Maple file. The resulting values are the roots of the equations. We improved the explanation in the corresponding section. The full equations are very cumbersome and we advise to consult the Maple file for an interested reader.

Fig. 6. The caption is too cumbersome with a lot of repetition. Simplify it!

The reviewer is completely correct and we shortened and improved the explanation.

\caption{Convergence rate in a homogeneous poroelastic medium for different I_2: numerical result as a function of the dimensionless parameter ${\mathrm{St}}$. Panel (a): $I_2 = 100$. Panel (b): $I_2 = 0.01$. Panel (c): $I_2 = 100$. Panel (d) $I_2 = 0.01$.
}

Fig. 6. For the 3D case, the optimized St are 28 for both I2=100 and I2=0.01, while they are different for 1D and 2D. Explain it!

We revised this section and deleted 3D results. We analyze 3D results in the discussion section.

\subsubsection{2D numerical simulations: poroelasticity}

The accuracy of the proposed ${\mathrm{St}}_\text{opt}$ is illustrated numerically in 2D (Figure~\ref{Fig_2Dv1}a-b). It can be seen that the results presented here for 1D need some calibration to be applied to 2D simulations. Note that the numerical parameters are sensitive to boundary and initial conditions, which is explored below. Therefore, some test must be performed for each numerical setup.

Line 400. Without comparison of low resolution, I can not see the thickness of shear band is mesh-independent.

The reviewer is correct and to prove it, we would need even higher resolution for comparison. However, without regularization the localization of shear bands is 1 grid cell. In our implementation we have several grid cells (more than 10) which already proves the mesh-independency of our simulation according to the study by

Resolving strain localization in frictional and time-dependent plasticity: Two-and three-dimensional numerical modeling study using graphical processing units Y Alkhimenkov, L Khakimova, I Utkin, Y Podladchikov

We added the references.

Figure~\ref{Press_2D_HR1} shows the results of the 2D simulation with an ultra-high resolution of $N=10,239^2$ grid cells. The finite thickness of the shear bands confirms that the simulation is mesh-independent as it has been shown by \cite{https://doi.org/10.1029/2023JB028566}.

Line 450.   Here you say St_opt=2*pi*sqrt(3).   It is different with Fig. 6 (28).   A lot is missing. Perhaps you should provide 2D and 3D derivation process. I could not find it in the maple file.

We did not make available the rigorous derivation process in 2D and 3D in Maple for a public. We agree that the values are different from Fig 6 and we modified this section. Indeed, as a first guess St_opt=2*pi*sqrt(3) but his value should be adjusted with respect to boundary and initial conditions and nonlinearities involved. See new discussion section.

Fig.11.   Please put boundary conditions information on the subtitle of b and c. it would made the figure more readable!

Done.

Line 469. "highly sensitive"?   The change is only from 4.63 to 6.0142. It is not very sensitive. You need another example to say it is highly sensitive!

The reviewer is completely correct, and as we mentioned earlier, we have revised this conclusion. The sensitivity is only minor.

Lawrence H.Wang

We would like to thank the reviewer again for valuable comments, which helped us improve the quality of the manuscript.

Sincerely,
Yury Alkhimenkov and Yury Podladchikov