

# Protocol

This trial protocol has been provided by the authors to give readers additional information about their work.

Protocol for: Doria A, Galecki AT, Spino C, et al. Serum urate lowering with allopurinol and kidney function in type 1 diabetes. *N Engl J Med* 2020;382:2493-503. DOI: 10.1056/NEJMoa1916624

Doria A, Galecki A, Spino C, et al. Serum Urate Lowering with Allopurinol and Kidney Function in Type 1 Diabetes

This supplement contains the following items

1. Original protocol (v6 dated December 17, 2013; this was the first protocol version under which participants were recruited for the pivotal trial; previous versions were for designing purposes only).
2. Final protocol (v10 dated March 6, 2018).
3. List of all protocol changes from original to final version.
4. Original statistical analysis plan (dated February 22, 2017).
5. Final statistical analysis plan (dated August 3, 2019).
6. List of all statistical analysis plan changes from original to final version.



**STUDY PROTOCOL**

**'A multicenter clinical trial of allopurinol to prevent GFR loss in type 1 diabetes'**

**Version 6.0**

**December 17, 2013**

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- National Institute of Diabetes and Digestive and Kidney Diseases
- Juvenile Diabetes Research Foundation (JDRF)

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**PROTOCOL SUMMARY**

Study Title	A multicenter clinical trial of allopurinol to prevent GFR loss in type 1 diabetes
Study Phase	Phase 3
Objectives	To determine whether lowering serum uric acid by means of allopurinol early in the course of kidney disease may be effective in preventing or slowing the decline of renal function in T1D patients.
Study Design	Multicenter, double-blind, placebo-controlled, parallel-group randomized clinical trial.
Participating Centers	Joslin Diabetes Center (Boston), University of Minnesota (Minneapolis), University of Colorado (Denver), University of Michigan (Ann Arbor), University of Toronto (Toronto), Northwestern University (Chicago), Albert Einstein College of Medicine (New York), Steno Diabetes Center (Copenhagen, Denmark).
Subject Population	<p>480 T1D subjects.</p> <p><u>Inclusion criteria:</u> 1. Male or female subjects with T1D diagnosed before age 35 and continuously treated with insulin within one year from diagnosis; 2. Duration of T1D <math>\geq</math> 8 years. 3. Age 18-65 years. 4. History or presence of microalbuminuria or moderate macroalbuminuria (at least two out of three consecutive urinary albumin excretion rates [AERs] or albumin creatinine ratios [ACRs] taken at any time before screening or at screening in the 30-2500 mg/24 hr (20-1667 <math>\mu</math>g/min) or 30-2500 mg/g range, respectively, if not on RASB agents, or in the 18-2500 mg/24 hr (12-1667 <math>\mu</math>g/min) or 18-2500 mg/g range, respectively, if on RASB agents); if the evidence of micro-/macroalbuminuria dates back to more than two years before screening assessment, evidence of ongoing renal function decline (GFR loss <math>\geq</math>3 ml/min/1.73 m<sup>2</sup>/year) is required. 5. Estimated GFR (eGFR) based on serum creatinine between 45 and 99.9 ml/min/1.73 m<sup>2</sup> at screening, with the upper limit being decreased by 1 ml/min/1.73 m<sup>2</sup> for each year over age 60. 6. Serum UA (UA) <math>\geq</math> 4.5 mg/dl at screening. OR 7. Being an active participant in the PERL Pilot Study. <u>Exclusion criteria:</u> 1. History of gout or xanthinuria or other indications for uric acid lowering therapy such as cancer chemotherapy. 2. Recurrent renal calculi. 3. Use of urate-lowering agents within 3 months before screening. 4. Current use of azathioprine, 6-mercaptopurine, didanosine, warfarin, tamoxifen, amoxicillin/ampicillin, or other drugs interacting with allopurinol. 5. Known allergy to xanthine-oxidase inhibitors or iodine containing substances. 6. HLA B*58:01 positivity (tested before randomization). 7. Renal transplant. 8. Non-diabetic kidney disease. 9. SBP&gt;160 or DBP &gt;100 mmHg at screening or SBP&gt;140 or DBP&gt;90 mmHg at the end of the run-in period. 10. Cancer treatment within two years before screening. 11. History of clinically significant hepatic disease including hepatitis B or C and/or persistently elevated serum liver enzymes and/or HBV/HCV positivity at screening. 12. History of acquired immune deficiency syndrome or human immunodeficiency virus (HIV) infection. 13. Hemoglobin concentration &lt;11 g/dL (males), &lt;10 g/dL (females) at screening. 14. Platelet count &lt;100,000/mm<sup>3</sup> at screening. 15. History of alcohol or drug abuse in the past 6 months. 16. Blood donation in the 3 months before screening. 17. Breastfeeding or pregnancy or unwillingness to be on contraception throughout the trial. 18. Poor mental function or any other reason to expect patient difficulty in complying with the requirements of the study. 19. Serious pre-existing medical problems other than diabetes, e.g. congestive heart failure, pulmonary insufficiency.</p>
Study Duration	9-week run-in period, during which RAS inhibition with ramipril 10 mg or acceptable alternative will be introduced and BP normalized, if elevated above 130/80 mmHg, followed by a 3-year treatment period and then by a 2-month wash-out period.

Study Treatment, Dosage, and Route of Administration	After the run-in period, eligible subjects will be randomized in a 1 to 1 ratio to receive placebo or oral allopurinol at a dose of 100 mg per day for 4 weeks and then at a dose ranging from 200 to 400 mg per day depending on kidney function.
Efficacy Assessments	<u>Primary outcome measure:</u> GFR at the end of the 2-month wash-out period following the 3-year treatment period, measured by the plasma clearance of non-radioactive iohexol (iGFR) and adjusted for the iGFR at baseline. <u>Secondary outcome measures:</u> 1. iGFR at 4 months after randomization adjusted for the iGFR at baseline. 2. iGFR the end of the 3-yr treatment period (before the washout period) adjusted for the iGFR at baseline. 3. iGFR time trajectory estimated from periodical iGFR measurements. 4. eGFR time trajectory estimated from quarterly serum creatinine and cystatin C measurements (eGFR). 5. Time to serum creatinine doubling or end stage renal disease (ESRD). 6. AER at the end of the 2-month wash-out following the 3-yr treatment period, adjusted for the AER at baseline. 7. AER at the end of the 3-yr treatment period, adjusted for the AER at baseline. 8. Time to fatal or non-fatal cardiovascular events.
Safety Assessment	Examination for skin rash, measurements of liver enzymes, serum creatinine, and CBC, carried out 1 month after randomization and every 3 months thereafter.
Statistical Methods	Data will be analyzed according to an intention-to-treat approach. Differences between treatment arms in the primary outcome will be tested for significance by means of a linear model with correlated errors. Intervention effects on other secondary outcomes will be tested by mixed-effect models (GFR time trajectory), ANCOVA (AER), and survival analysis (time to serum creatinine doubling/ESRD and CVD events).
Date of protocol	December 17, 2013

**ABBREVIATIONS**

**AE:** Adverse Event

**ACE:** Angiotensin Converting Enzyme

**ACR:** Albumin Creatinine Ratio

**AER:** Albumin Excretion Rate

**ALT:** Alanine Transaminase

**ARB:** Angiotensin Receptor Blocker

**CBC:** Complete Blood Count

**CKD:** Chronic Kidney Disease

**CRF:** Case Report Form

**CVD:** Cardiovascular Disease

**DBP:** Diastolic Blood Pressure

**DCC:** Data Coordinating Center

**DMC:** Drug Monitoring Committee

**DN:** Diabetic Nephropathy

**DSMB:** Data and Safety Monitoring Board

**ESRD:** End Stage Renal Disease

**GFR:** Glomerular Filtration Rate

**ITT:** Intention to Treat

**HbA1c:** Glycated Hemoglobin A1C

**HBV:** Hepatitis B Virus

**HCV:** Hepatitis C Virus

**HIV:** Human Immunodeficiency Virus

**IRB:** Institutional Review Board

**MAP:** Mean Arterial Pressure

**NO:** Nitric Oxide

**PERL:** Preventing Early Renal Function Loss in Diabetes Consortium

**RAS:** Renin Angiotensin System

**RASB:** Renin Angiotensin System Blocker

**SAE:** Severe Adverse Event

**SBP:** Systolic Blood Pressure

**SC:** Steering Committee

**SOP:** Standard Operating Procedure

**T1D:** Type 1 Diabetes

**UA:** Uric Acid

## 1. INTRODUCTION

Diabetic nephropathy (DN) is the long-term complication of T1D that imposes the highest social and economic burden. After 40 years of diabetes, about one in three patients with T1D has developed kidney abnormalities, which frequently progress to end stage renal disease (ESRD).<sup>1</sup> Despite improvements during the past 20 years in glycemic and blood pressure control, and the introduction of 'renoprotective' drugs such as renin-angiotensin system (RAS) blockers, the overall incidence of DN is not declining.<sup>2-4</sup> Thus, DN remains one of the most important causes of excess morbidity and mortality in patients with diabetes mellitus, and novel therapies to complement and increase the therapeutic effects of glycemic control and RAS inhibition are urgently needed.

DN has been traditionally viewed as a multi-stage process, in which an initial clinical phase characterized by increased urinary excretion of small amounts of albumin (microalbuminuria) is followed by excretion of larger amounts of proteins (overt proteinuria), which then ushers in progressive decline in renal function ultimately leading to end-stage renal disease (ESRD).<sup>1</sup> This paradigm, however, is changing with the demonstration in prospective studies that, in a substantial proportion of T1D patients, renal function starts to decline before the onset of overt proteinuria.<sup>5-7</sup> These findings indicate that T1D patients should be screened for GFR loss when albumin excretion rate (AER) is still in the microalbuminuria range, and that interventions aimed at preventing ESRD should be started at these earlier stages. The earlier the rate of GFR loss is reduced through appropriate interventions, the longer will be the delay of ESRD.

Mounting evidence from epidemiological studies indicates that serum UA levels are strong risk factors for the development of chronic kidney disease and loss of kidney function among persons with T1D. Prospective data from the Second Joslin Kidney Study (JKS) identified elevated baseline serum UA as one of the strongest independent predictors of early GFR loss among T1D persons with microalbuminuria and normal renal function at baseline.<sup>8</sup> The unadjusted odds ratio of developing increased GFR loss was 1.5 (95% CI 1.3-1.9,  $p=0.0002$ ) for each mg/dl increase in serum UA. This translates into a ~2.4-fold increase in the risk of early GFR loss for UA levels  $\geq 4.5$  mg/dl as compared to UA levels  $<4.5$  mg/dl. The magnitude of this effect did not significantly change after adjustment for urinary AER, gender, HbA1c, or, importantly, baseline GFR. The U. of Colorado group also found that serum UA predicted the transition from normoalbuminuria to micro- or macro-albuminuria as well as the progression of subclinical atherosclerosis in the CACTI study.<sup>9,10</sup> As in the JKS, the effect of UA was not influenced by adjustment for other baseline variables. An association between UA and development of persistent macroalbuminuria has also been reported by the Steno group. It is very important to note that, in that study, the UA levels shortly after the onset of T1D was a significant independent predictor of macroalbuminuria 18 years later (hazard ratio 1.90 per mg/dl increase in UA level;  $p=0.04$ )<sup>11</sup>, this suggestive of a pathogenetic role.

The prospective nature of these findings and their robustness after adjustment for potential confounders strongly support the concept that moderately elevated serum UA has a pathogenetic role in DN development and in the deterioration of kidney function observed in T1D. Consistent with this hypothesis, hyperuricemia has predicted chronic renal failure in population-based studies<sup>12-14</sup> and mild UA elevation has been shown to cause renal disease in animal models.<sup>15,16</sup> Alterations of nitric oxide (NO) pathways and induction of pro-inflammatory cytokines<sup>17,18</sup>, and increased oxidative stress resulting from the generation of UA by xanthine oxidase<sup>19,20</sup> have been proposed as being responsible for these effects. Two small clinical trials have recently provided proof of concept data for translating these findings into a novel intervention by showing that the urate-lowering agent allopurinol was effective in slowing the progression of non-diabetic renal disease among hyperuricemic as well as normouricemic individuals with moderately reduced GFR.<sup>21,22</sup> A beneficial effect of urate-lowering drugs on the progression of kidney disease has also been observed in animal models.<sup>23</sup> These findings, along with the observational data discussed above, strongly suggest that **lowering serum UA levels may prevent or slow the loss of kidney function among diabetic subjects.**

To test this hypothesis, we have established a consortium of investigators from academic centers where large rosters of T1D patients are available along with long-standing expertise in the study of diabetic complications, especially DN, and in DN clinical trials. Included in this initiative are the Joslin Diabetes Center, the Universities of Minnesota, Colorado, Toronto, , and Michigan, Northwestern University, Albert Einstein College of Medicine, and the Steno Diabetes Center in Copenhagen, Denmark. The Consortium, led by Dr. Alessandro Doria from the Joslin Kidney Study, and by Dr. Michael Mauer, who recently led the Renin Angiotensin System Study (RASS) clinical trial, has been named PERL (**P**reventing **E**arly **R**enal Function **L**oss in Diabetes) to emphasize the Consortium's focus on intervening early in the course of kidney disease, when renal damage is most likely to be able to be arrested or reversed and interventions are more likely to be effective.

PERL has designed the present 3-year clinical trial to test whether the uric acid lowering drug allopurinol can preserve kidney function among type 1 diabetic patients. In preparation for this trial, the Consortium has been conducting a pilot study to determine the study feasibility and establish study procedures. Funded by JDRF (JDRF file # 17-2012-377), the pilot study has a comparable design as the pivotal trial, but a smaller size and shorter duration. As of December 1, 2013, a total of 28 subjects have been randomized to allopurinol or placebo, with another 4 in the run-in period. Upon activation of the present trial, pilot participants will be re-consented and rolled-over to the present study at a time point corresponding to their next scheduled visit. The first 7 visits have identical timing in the two protocols. Visit schedules slightly differ after that, but given the current follow-up status, it will be possible to transfer all pilot participants to the pivotal trial before the timing diverges.

## 2. STUDY OBJECTIVE

To determine whether lowering serum UA by means of oral allopurinol is effective in preventing or slowing decline of renal function in T1D patients with microalbuminuria or moderate macroalbuminuria who still have only mildly or moderately impaired kidney function.

## 3. STUDY DESIGN

The study will be a multi-center, double-blind, placebo-controlled, parallel-group randomized clinical trial including a total of 480 patients with type 1 diabetes (T1D) who are at high risk for GFR loss because of increased albuminuria and a relatively high serum UA ( $\geq 4.5$  mg/dl), but have only mildly or moderately decreased renal function.

## 4. PARTICIPATING CENTERS

The study will involve eight centers that are part of the PERL Consortium:

- Joslin Diabetes Center (Boston)
- University of Minnesota (Minneapolis)
- University of Colorado (Barbara Davis Center for Childhood Diabetes, Denver)
- University of Michigan (Ann Arbor)
- University of Toronto (Toronto)
- Northwestern University (Chicago)
- Albert Einstein College of Medicine (New York)
- Steno Diabetes Center (Copenhagen, Denmark)

## 5. SUBJECT SELECTION

### 5.1. Inclusion Criteria

- Male or female T1D patients between 18 and 65 years of age, inclusive.
- T1D diagnosed before age 35 and continuously treated with insulin within one year from diagnosis. If the onset was between ages 31 and 35, body mass index (BMI) will be required to be  $< 26$  kg/m<sup>2</sup> at the time of diagnosis;
- Duration of T1D  $\geq 8$  years;
- History or presence of microalbuminuria or moderate macroalbuminuria (at least two out of three consecutive urinary albumin excretion rates [AERs] or albumin creatinine ratios [ACRs] taken at any time before screening or at screening in the 30-2500 mg/24 hr (20-1667  $\mu$ g/min) or 30-2500 mg/g range, respectively, if not on RASB agents, or in the 18-2500 mg/24 hr (12-1667  $\mu$ g/min) or 18-2500 mg/g range, respectively, if on RASB. If the evidence of micro-/macroalbuminuria dates back to more than two years before the screening assessment and the participant is currently normoalbuminuric, evidence of ongoing renal function decline (GFR loss  $\geq 3$  ml/min/1.73 m<sup>2</sup>/year) is required. This evidence should be based on the eGFR (CKD-EPI) slope estimated from at least 3 serum creatinine measurements (including the one at screening assessment) spanning a period of 3 years or longer.
- Estimated GFR (eGFR, based on serum creatinine and the CKD-EPI equation) between 45 and 99.9 ml/min/1.73 m<sup>2</sup>, inclusive, at the screening visit. The upper limit should be decreased by 1 ml/min/1.73 m<sup>2</sup> for each year over age 60.
- Serum UA  $\geq 4.5$  mg/dl at the screening visit.
- Willing to comply with schedule of events and protocol requirements, including written informed consent.

OR

- Being an active participant in the PERL Pilot Study.



**5.2. Exclusion Criteria**

- History of gout requiring allopurinol therapy or xanthinuria or other indications for uric acid lowering therapy such as cancer chemotherapy or extremely high serum uric acid values (>12 mg/dl).
- Recurrent renal calculi (history of more than one episode).
- Use of urate-lowering agents within 3 months before screening.
- Current use of azathioprine, 6-mercaptopurine, didanosine, warfarin, tamoxifen, amoxicillin/ampicillin, or other drugs interacting with allopurinol.
- Known allergy to xanthine-oxidase inhibitors or iodine containing substances.
- HLA B\*58:01 genotype (determined prior to randomization) indicating increased risk of Stevens-Johnson syndrome in response to allopurinol.
- Renal transplant.
- Non-diabetic kidney disease as indicated by medical history and/or laboratory findings.
- SBP>160 or DBP >100 mmHg at screening or SBP>140 or DBP>90 mmHg at the end of the run-in period.
- Cancer treatment within two years before screening.
- History of clinically significant hepatic disease including hepatitis B or C and/or ALT (SGPT) >2.50 x ULN at screening and/or HBV/HCV antibody positivity.
- History of acquired immune deficiency syndrome or human immunodeficiency virus (HIV) infection.
- Hemoglobin concentration <11 g/dL (males), <10 g/dL (females) at screening.
- Platelet count <100,000/mm<sup>3</sup> at screening.
- Ongoing alcohol or drug abuse or history of treatment for these conditions in the past 6 months.
- Blood donation in the 3 months before screening (subjects become eligible once 3 months have elapsed since the last donation).
- Breastfeeding or pregnancy or unwillingness to be on contraception if still fertile.
- Poor mental function or any other reason to expect patient difficulty in complying with the requirements of the study.
- Serious pre-existing medical problems other than diabetes, e.g. congestive heart failure, pulmonary insufficiency.

**5.3. Prohibited Medications and Restrictions**

- Allopurinol and other urate lowering agents (e.g., probenecid, rasburicase rys)
- Herbal supplements that may have urate lowering actions (e.g., Devil's Claw or Harpagophytum procumbens, Indigenous cinnamon or Cinnamomum osmophloeum, Skunkvine or Paederia scandens or Paederia foetida)
- Azathioprine
- 6-Mercaptopurine
- Didanosine
- Warfarin
- Tamoxifen
- Amoxicillin/ampicillin
- Any other drug for which there is evidence of interaction with allopurinol
- Dual RASB therapy (i.e., another RASB medication in addition to that already in use)
- Non-RASB antihypertensives that are not listed in the PERL approved menu of antihypertensive drugs, unless these were in use before joining the study.

**5.4. Randomization Procedures**

After the run-in period (described in Section 8.3), participants will be randomized in a 1 to 1 ratio to receive either oral allopurinol or placebo. Randomization will be stratified by center, uric acid ( $\leq 6.0$  vs.  $> 6.0$  mg/dl), and HbA1c ( $\leq 7.8$  vs.  $> 7.8\%$ ). Randomization will be performed using permuted blocks, with a block size that is known only to the DCC. After a participant has been randomized, the clinical site will send a study medication request to the research pharmacy, including the participant's address, so that the study medication can be directly mailed to the participant. Clinical sites will not have access to the treatment assignment (see 6.2., Blinding Procedures). This will be directly communicated or made electronically available to the pharmacy by the DCC.

## 5.5. Discontinuation of study drug

### 5.5.1. Reasons for discontinuation

The study drug will be *temporarily* discontinued if a participant:

- Has clinically significant persistent changes from baseline based on laboratory safety assessment results (the response to discontinuation will be monitored to assess whether the drug can be re-instituted, see next paragraph on permanent discontinuations).
- Requires treatment with allopurinol or medications that make allopurinol contraindicated (see 5.5.2 and 9.5).
- Becomes pregnant or breastfeeding (see 5.5.2)

The study drug will be *permanently* discontinued if a participant:

- Experiences an SAE related to the study drug or an intolerable AE such as a persistent allergy or rash.
- Has clinically significant persistent changes from baseline based on laboratory safety assessment results which do not respond to temporary 2-week discontinuation of study drug and re-institution of drug at ½ of the initial dose.
- Develops end-stage renal disease (eGFR  $\leq 15$  ml/min/1.73 m<sup>2</sup>, institution of chronic dialysis treatment or kidney transplantation) or iGFR decreases by 50% from one measurement to the next or serum creatinine levels double over any 12 month interval in the post-randomization period.

### 5.5.2. Handling of study drug discontinuation

- Date and reason for drug discontinuation will be recorded on the relevant Case Report Form.
- All study discontinuations decided by a clinical site will have to be reviewed and approved by the Drug Monitoring Committee within 10 days from their start.
- If the study drug is discontinued due to treatment with medications that make allopurinol contraindicated (e.g. amoxicillin/ampicillin) or due to pregnancy/breastfeeding, the possibility of resuming the study drug will be evaluated by the Drug Monitoring Committee once those medications have been discontinued or pregnancy/breastfeeding has ended.
- If the study drug is temporarily discontinued and then re-instated, the end-date of the intervention will remain the same as if the study drug had not been discontinued. All visits will be carried out as scheduled while the study drug is temporarily discontinued.
- Unless a participant withdraws consent, all participants that are permanently discontinued from study drug or who discontinue study medication on their own will be followed for the full study period (i.e., 164 weeks, including the washout period) and all data will be collected as scheduled. Major attempts will be made to schedule an end-of-study assessment for all participants who are lost to follow-up during the course of the study.

### 5.5.3. Replacements

Participants that withdraw consent from the study during the Run-in period (i.e., before randomization) or do not qualify for study continuation at the end of the Run-in period will be replaced until the target number of randomized study participants is reached. Participants that withdraw consent from the study or discontinue the study drug after randomization will not be replaced.

### 5.5.4. Termination of Study

Premature termination of this clinical trial may occur because of a regulatory authority decision, drug safety problems as determined by the Data Safety Monitoring Board (DSMB), or at the discretion of the sponsor (NIDDK).

## 6. STUDY TREATMENTS

### 6.1. Study Drug Description, Dosage, Administration, and Accountability

#### 6.1.1. Description

Eligible study subjects who agree to participate in the study will all be randomized to receive placebo or allopurinol – a serum UA lowering medication that has been on the market since 1964 as the main drug for the therapy of symptomatic hyperuricemia and for the prophylaxis of gout in cancer patients receiving chemotherapy. Allopurinol is an inhibitor of xanthine oxidase, which is responsible for the conversion of hypoxanthine to xanthine and of xanthine to UA. It is metabolized to the corresponding xanthine analogue, oxypurinol (alloxanthine), which

is also an inhibitor of xanthine oxidase. At the average dosage (300 mg/day), allopurinol causes a 30-40% reduction in serum UA<sup>24-26</sup>, but up to a 60% reduction can be obtained using the maximum dosage of 600 mg.<sup>27</sup> While allopurinol is mostly used in individuals with gout and very high UA levels, several studies have shown that it is also effective at lower UA levels<sup>27-29</sup>.

Because of its rapid oxidation to its active metabolite oxypurinol, allopurinol has a short plasma half-life (~1-2 hrs). However, since oxypurinol has a longer half-life (~15 hrs), effective xanthine oxidase inhibition can be maintained over 24 hrs with a single daily dose of allopurinol. Since both allopurinol and oxypurinol are eliminated through the kidneys, patients with impaired renal function require lower doses than those with normal renal function. A common rule of thumb is to use 75% of the dosage in individuals with eGFR in the 50-90 ml/min range, and 50% of the dosage in individuals in the 10-50 ml/min range.

### 6.1.2. Dosage

After an initial four weeks where all participants randomized to allopurinol will take 100 mg per day, the allopurinol dosage will vary from 200 to 400 mg per day based on eGFR levels. Participants will take 400 mg per day if their eGFR is  $\geq 50$  ml/min/1.73 m<sup>2</sup>, 300 mg per day if their eGFR is in the 25 to  $< 50$  ml/min/1.73 m<sup>2</sup> range, and 200 mg per day if the eGFR is in the 15 to  $< 25$  ml/min/1.73 m<sup>2</sup> range. Allopurinol will be continued at this dosage throughout the study unless the eGFR changes, in which case the dosage will be modified to that appropriate for the new eGFR class.

All participants, whether they are randomized to allopurinol or placebo, will be given four tablets per day to be taken orally following meals, two in the morning and two in the evening. Tablets will be provided in four vials (A, B, C, D). Participants randomized to allopurinol will receive a dosage of 100 mg as a 100 mg tablet (from vial A) plus three placebo tablets (from vials B, C, D), 200 mg as two 100 mg (from vials A and C) and two placebo tablets (from vials B and D), 300 mg as three 100 mg (from vials A, B, C) and one placebo tablet (from vial D), 400 mg as four 100 mg tablets (from vials A, B, C, D). Subjects randomized to placebo will be given four placebo tablets (from vials A, B, C, D).

The dose adjustment will be carried out as follows:

1. At each follow-up visit, a study drug requisition will be sent by the clinical site to the research pharmacy indicating the study ID, name, and address of the participant and the number of days to be covered by the drug supply.
2. At the pharmacy, a clinical pharmacologist will determine the allopurinol dose (ranging from 0 to 400 mg) that should be given at that time according to the study protocol given the participant's treatment assignment and the eGFR estimated from the most recent serum creatinine value (the pharmacy will retrieve this information from the online study database to which it will have access).
3. The research pharmacy will mail the new batch of study medication directly to the study participant.
4. Participants will be instructed to immediately inform the clinical site upon receipt of the new tablets and mail the pill bottles with the tablets remaining from the previous prescription in a provided pre-addressed mailer, to the clinical site for drug accounting and compliance assessment.

### 6.1.3. Compliance and accountability.

Skills will be taught and reinforced at each visit with regard to scheduling and administration of pills at home and while traveling. Methods (e.g. record-keeping) will be taught to help participants monitor tablet usage and enhance compliance. To complement the regular compliance interventions at the scheduled visits, study information and motivational materials (postcards, newsletters, etc.) will be mailed. In addition, at midpoint between clinic visits, participants will be phoned by the clinic staff to review pill-taking. Patients will be provided with random but known numbers of excess medications, providing extras in case of pill loss. Adherence will be monitored by instructing participants to expect extra pills and to mail the pill bottles with the tablets remaining from the previous prescription to the study center upon receipt of a new batch of tablets. The number of extra pills included in each supply of medications will be decided by the pharmacist, who will keep a record of it and will transmit this information to the Study Site. Personnel at the Study Site will enter this information in the appropriate electronic Case Report Form along with the expected number of pills used during the period covered by the supply and the number of unused pills returned by the participant. These data will be used to analyze compliance. If poor adherence is noticed, measures will be taken to increase compliance, such as explaining the purpose of the study again, providing pill reminders, and more frequently contacting the study subject by phone. Participants at each visit will be asked about their perceived compliance and about any difficulties with taking the study medications, but the individualized strategies to improve compliance will not be openly linked to the pill

counts, i.e. participants will not be informed of the results of pill counting. Participants showing poor compliance will not be withdrawn from the study.

## 6.2. Blinding Procedures

Study participants, the investigators and research staff at the Clinical Sites, and the PERL co-PIs (Drs. Doria and Mauer) will be blinded to treatment assignment whereas the Data Coordinating Center Co-Directors and staff and the pharmacy personnel will have access to this information. Serum uric acid values, from which the treatment assignment might be inferred, will not be transmitted to the Clinical Sites by the central or local laboratory and will not be available for viewing in the study database. Should unblinding of a study participant be necessary because of an emergency, the site personnel will login to the password-protected electronic database application that will provide the treatment assignment. Audit procedures will ensure that the name of the individual associated with the login will be communicated to the Data Coordinating Center project manager and Co-Directors. As an additional safety measure, the personnel at the Clinical Sites will be provided with telephone numbers to contact the Data Coordinating Center and/or Pharmacy personnel having access to the treatment assignment on a 24-7 basis. If unblinding occurs, the circumstances that led to it will be reviewed and reported.

## 7. STUDY OUTCOMES

### 7.1. Primary outcome

The primary outcome will be the iGFR at the end of the 2-month wash-out period following the 3-year treatment period, measured by the plasma clearance of non-radioactive iohexol (iGFR) and adjusted for the iGFR at baseline. The rationale of measuring the primary outcome at the end of the wash-out period is to test allopurinol for permanent effects of on the natural history of kidney disease, independent from any transient, hemodynamic effect that the medication may have on GFR. Plasma iohexol clearance has been shown to provide accurate and reproducible GFR measurements.<sup>30,31</sup> It is highly correlated with insulin clearance (the gold standard to measuring GFR)<sup>32</sup> and is a safe, cost-effective method to test hundreds of patients enrolled in multicenter clinical trials.<sup>33</sup> The method consists of injecting a 5 mL bolus of Iohexol (Omnipaque, 300 mg iodine/mL) and drawing blood samples at baseline and 120, 150, 180, 210, and 240 minutes after the injection. Plasma concentrations of iohexol at different time points are measured by HPLC and used to calculate the plasma clearance of iohexol ( $Cl = \text{Dose}/\text{AUC}$ , where AUC is the area under the plasma concentration time curve), which is taken after appropriate body surface area corrections as a measure of GFR.<sup>30,31</sup>

#### 7.1.1. iGFR quality assurance.

It is of the foremost importance that reliable iGFR measurements are obtained. To maximize accuracy and precision, the following procedure will be in place.

1. Personnel performing the iohexol clearance test will undergo a rigorous, standardized training program administered under the DCC supervision through in-person meetings or on-line modules. All staff will have to be study-certified as having undergone the training program in order to perform the tests.
2. Participants will be instructed to discontinue non-steroidal anti-inflammatory drugs (NSAIDs) for at least 3 days and avoid large protein meals for one day prior to the test, since these could influence GFR. They will also be instructed to aim for a fasting glycemia between 90 and 160 mg/dl on the day of the test. Before the test, participants will have a light breakfast at the clinic along with their morning insulin. The insulin dose will be adjusted to keep their blood glucose in the 90-160 mg/dl range. If blood glucose is outside this range right before or during the test, small amounts of intravenous insulin (if blood glucose is too high) or orange juice/milk (if glucose is too low) may be administered to bring blood glucose levels within the desired interval.
3. In the case of extreme deviations from the target blood glucose values, the test may be rescheduled to another day (within 2 weeks). The test will also be postponed in the case of recent febrile illness, diarrhea or vomiting, dehydration, poor fluid intake, recent intake of nephrotoxic drugs such as NSAIDs, urinary tract infection, or a positive pregnancy test.

#### 7.1.2. iGFR quality control

The quality of iGFR results will be monitored by:

1. Systematically checking for deviations from the study protocol, such as deviations from the target blood draws time points during the test or the presence of medical conditions that should have prompted a postponement of the test.

- Calculating the R-square ( $R^2$ ) of the regression between log-iohexol values and time. iGFR tests will be defined as technically acceptable if the  $R^2$  is  $>0.90$ .  $R^2$  calculations will be performed by the DCC using all 5 time points of the iGFR test.

### 7.1.3. Technically unacceptable iGFR measures.

If an iGFR test is deemed to be technically unacceptable according to the above QC criterion ( $R^2 \leq 0.90$ ), or a study protocol deviation is suspected, the following procedures will be followed:

- Source documents related to the test in question will be reviewed to verify whether there was a protocol deviation or a technical error in the iGFR procedure (e.g., presence of contraindications to iGFR, swapping of tubes, wrong collection times, typos, etc.). In the case of an  $R^2 \leq 0.90$ , the iohexol measurements will be repeated by the central laboratory.
- If a technical error is found and the error can be rectified, or the new laboratory measures yield an  $R^2 > 0.90$ , the iGFR value will be recalculated after the appropriate corrections are made. The study site or the central laboratory, as applicable, will be alerted about the error and measures aimed at improving iGFR quality will be implemented.
- If the error is confirmed and cannot be fixed, or no error can be found, the iGFR will be dropped and will be repeated within 4 weeks from when the iGFR results become available.
- If the repeated test is technically unacceptable, or the test cannot be repeated within 4 weeks for logistical reasons, the iGFR value at that time point will be considered as missing for the analysis of the primary outcome. It is therefore critical that every effort be made to obtain this repeat iGFR measure.

### 7.2. Secondary outcomes

- Iohexol-clearance GFR at 4 months after randomization.
- Iohexol-clearance GFR at the end of the 3-year treatment period (before the washout).
- Iohexol-clearance GFR time trajectory estimated from periodical iohexol-GFR measurements.
- Estimated GFR (eGFR) time trajectory estimated from quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations.<sup>34,35</sup>
- Time to doubling of baseline serum creatinine value or ESRD (eGFR  $\leq 15$  ml/min/1.73 m<sup>2</sup>, institution of dialysis, kidney transplantation).
- Geometric mean of two AER measurements at the end of the 2-month wash-out period following the 3-year treatment period, adjusted for the mean urinary AER at baseline. Urinary AER will be determined in timed overnight urine collections brought by study participants to regular clinic visits, and expressed in  $\mu\text{g}/\text{minute}$  and as urinary albumin/creatinine ratios.
- Geometric mean of urinary AER during the last three months of the treatment period (Visits 15 and 16), adjusted for the mean urinary AER at baseline.
- Time to fatal or non-fatal cardiovascular events, defined as the composite of CVD death (ICD-10 code I10 to I74.9), myocardial infarction, stroke (ischemic or hemorrhagic), coronary artery bypass grafting, or percutaneous coronary intervention.

## 8. STUDY PROCEDURES

### 8.1. Schedule of Events

The schedule of events that will take place in the proposed study is outlined in Figure 1. Visits will be frequent during the Run-In period and during the first 30 days after randomization in order to escalate the allopurinol dosage and closely monitor the occurrence of AEs. After that, participants will be seen every 12 weeks to monitor their UA levels, renal function, occurrence of AEs, and medication compliance and, if necessary, to perform interventions to improve compliance. Visit 1 will be considered as Time 0 for scheduling Visits 2-5, Visit 5 will be considered as Time 0 for scheduling Visit 6-16, Visit 16 as Time 0 for scheduling Visit 17. The study windows that define when study visits may occur are noted in Figure 1 and differ by type of visit. Visits 2, 3, and 6 will be carried out within 6 business days (before or after) from their scheduled dates; visits 7, 11, 16, and 17 within 2 weeks before and 4 weeks after their scheduled dates; visit 4A (if necessary) within 1 week before and 3 weeks after its scheduled date, and all other visits within 2 weeks (before or after) from their schedule dates. Additional blood or urine samples may be required in between visits if clinically significant changes are observed in

blood or urine measurements that need to be confirmed or otherwise monitored. iGFR measurements may be repeated for medical reasons or technical problems (see 7.1). Safety laboratory tests (CBC, serum creatinine, K<sup>+</sup>, ALT, and pregnancy tests in women) will be performed by local laboratories. Outcome variables (plasma iohexol, serum creatinine and cystatine C, urinary AER), HbA1c, and serum uric acid will be measured by the Central Laboratory at the University of Minnesota, directed by Dr. John Eckfeldt.

Figure 1. Schedule of Events.

Year	1	1	1	1		1	1	1	1	2	2	2	3	3	3	3	3		
Week	-12	-9	-7	-3		0	4	16	32	48	64	80	96	112	128	142	156	164	
VISIT	1	2	3	4	4a	5	6	7	8	9	10	11	12	13	14	15	16	17	
EVENT	Screen	Run-in				RANDO 100 mg	Allopurinol or placebo											Wash-out	
							200-400 mg												EOS
Demographics	X																		
Medical Hx	X																		
Family History																			
Confidence Measure	X																		
Concomitant Meds	X	X	X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Blood Pressure and Measurements	X	X	X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
ECG Report	X			X	(X)			X										X	
Physical Exam		X		(X)	(X)			X				X						X	
Skin Assessment						X	X	X	X	X	X	X	X	X	X	X	X	X	X
Eligibility						X													
Randomization						X													
Family History						X													
RAS and BP Med Log		X	X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
IGFR Procedure				X	(X)			X			X							X	X
PERL Study Drug Prescription						X	X	X	X	X	X	X	X	X	X	X	X		
Exemption Request																			
ACR/AER	X	X		X	(X)			X		X		X		X		X		X	X
Study Drug Compliance							X	X	X	X	X	X	X	X	X	X	X		
Study Drug Exposure							X	X	X	X	X	X	X	X	X	X			
Central Lab																			
ARDL: serum uric acid, serum creat,	X		X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
ARDL: urine ACR/AER	X		X	X	(X)			X		X		X		X		X		X	X
ARDL: HbA1c	X			X	(X)	X		X	X	X	X	X	X	X	X	X	X	X	X
ARDL: HLA B				X	(X)														
ARDL: iGFR				X	(X)			X				X						X	X
ARDL: (HBV) HBsAg and anti-HBcAb; (HCV) anti-HCV; (HIV) anti HIV 1,2	X																		
ARDL: NIDDK Repository: serum, plasma, urine	X		X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
ARDL: Pregnancy test serum HCG			X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Local: Pregnancy test urine dipstick				X				X				X						X	X
Local: ALT, K, CBC, se creatinine, urine Multistix	X		X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Protocol Deviation		X	X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Serious Adverse Event		X	X	X	(X)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Protocol Deviation																			

### 8.2. Screening and Enrollment in the Run-in Period (Visit 1)

Subjects who have a confirmed history of micro- or macroalbuminuria (at least two out of three consecutive urinary AER or ACR in micro- or macroalbuminuria range as defined in Section 5.1) will not need to bring a sample of urine to Visit 1 (if the evidence of micro- or macroalbuminuria dates back to more than two years before screening, evidence of ongoing GFR decline should be gathered, see Section 5.1). Subjects who have incomplete evidence of micro- or macroalbuminuria (one of the last two urinary AER or ACR in the micro- or macroalbuminuria range), or have unknown albuminuria status will be mailed two containers before Visit 1 along with instructions for collecting two samples of urine from their first morning void and bringing it to the visit to confirm the presence of micro- or macro-albuminuria. During **Visit 1**, subjects will undergo the following procedures:

- Obtain written informed consent.
- Collect medical history, prior and concomitant medications, and demographic information.
- Complete Confidence Questionnaire
- Measure weight and height.
- Measure vital signs.
- Perform ECG.
- Perform pregnancy test in women of childbearing potential.
- Collect samples for clinical laboratory assessment.
- Provide a container and instructions for an overnight urine collection to be made immediately before Visit 3 if subject qualifies for the study.
- Upon receipt of laboratory measurements, confirm that inclusion/exclusion criteria are met.

The Screening Visit can be repeated after 8 weeks or longer if the circumstances that led to the exclusion of a participant are deemed to have possibly changed.

### 8.3. Run-in Period (Visits 2, 3, and 4)

Starting at **Visit 2**, eligible subjects who agree to participate in the study will enter a run-in period of 9 weeks. During this visit, subjects will undergo the following procedures:

- Collect concomitant medications
- Measure weight and height.
- Measure vital signs.
- Physical Examination

RAS antagonist treatment will be standardized, and BP, if elevated (>130/80 mm Hg), normalized. Letters will be written to the participants' physicians informing them about the study and notifying them that study physicians will be assuming control of the participants' antihypertensive therapy. The run-in period will start at **Visit 2**. After discontinuing any existing RAS blockers, participants will be prescribed and instructed to start taking 10 mg of ramipril daily or 300 mg of irbesartan daily, if ramipril is contraindicated or has side effects. Participants who are opposed to changing their RAS Blocker therapy will be offered the possibility to continue taking the RAS blocker that they are using after adjusting its dose to make it equivalent to ramipril 10 mg. Participants who have contraindications to RAS blockers (e.g., SBP<100 mmHg, K<sup>+</sup>>5.5 mEq) will still qualify for the study but will not be treated with these drugs, as this would represent standard of care. Participants who are started for the first time on RAS blockers as part of this study will have their serum K<sup>+</sup> and creatinine measured at a local laboratory after 1 week of treatment. RAS blockers will be immediately discontinued in the case of allergic reactions or angioedema. Their dose will be decreased to half if symptomatic hypotension (SBP<100%) or intractable cough develops, followed by their discontinuation. For persistent cough with ramipril or other ACEI, irbesartan will be prescribed in substitution of the ACEI. In the case of hyperkalemia (K<sup>+</sup> >6.0 mEq) or serum creatinine elevation (>30% increase over baseline values), the participant will be asked to immediately obtain a confirmatory lab value at their local lab or clinical site and then discontinue the RAS blocker while awaiting this confirmatory result. If confirmed, the participant will resume RAS blockade at half dose 72 hours later and will have repeat labs one week later. If the problem persists, RAS blockade will be discontinued for the remainder of the trial and BP managed by alternate drugs (see below). If not confirmed, the participant will resume RAS blockade at their usual dose and have a repeat lab check one week later. These same steps will be taken if hyperkalemia develops during the trial. Participants will continue to take any other antihypertensive drug that they may have been taking before study entry (except for other RAS blockers, aldosterone blockers, and renin inhibitors). Participants will be provided with



a blood pressure monitoring device (if they do not already have access to one), will be trained on its use, and will be instructed to periodically monitor their blood pressure at home and to record the results into a BP diary, and to communicate them to study personnel if values are abnormal. If hypotension develops (SBP < 100 or significant lightheadedness), the dosage of non-RAS antagonist antihypertensive drugs will be progressively reduced until discontinuation, followed by a reduction of RAS blockers to half the dose and their discontinuation if the problem persists. If BP is found to be elevated (> 130/80 mm Hg) on three consecutive occasions, the dosage of existing non-RAS antagonists antihypertensive drugs will be maximized, followed, if necessary, by the introduction of antihypertensive drugs of a different class. These will be chosen by the study site physicians from a restricted menu of approved medications at recommended dosage, following the general protocol that was used in RASS<sup>33</sup>. The same menu and protocol will be used to start antihypertensive treatment in participants who have persistently high BP levels and were not on antihypertensive therapy prior to study entry. If the goal of BP < 130/80 is not achieved with these drugs, a Drug Monitoring Committee conference call will be convened to consider the possibility of causes of hypertension other than diabetic nephropathy and discuss alternative therapeutic approaches. BP will continue to be monitored and the anti-hypertensive therapy to be adjusted in a similar way throughout the study.

After 2 weeks of run-in, participants will come in for **Visit 3** during which they will undergo the following procedures:

- Obtain interval medical history.
- Review concomitant medications and AEs
- Review and adjust BP therapy.
- Measure weight and vital signs.
- Collect samples for clinical laboratory assessments as outlined in Figure 1.
- Perform pregnancy test in women of childbearing potential.
- Provide a container and instructions for an overnight urine collection to be made immediately before Visit 4.

After 6 weeks of run-in, participants will come in for **Visit 4** during which they will undergo the following procedures:

- Obtain interval medical history.
- Conduct a physical exam (if deemed to be required by the study physician)
- Review concomitant medications and AEs
- Review and adjust BP therapy.
- Measure height, weight and vital signs.
- Perform ECG.
- Collect samples for clinical laboratory assessments (including HLA B\*5801) as outlined in Figure 1.
- Perform pregnancy test in women of childbearing potential.
- Measure iohexol GFR.

If normal blood pressure control is not achieved at Visit 4, the run-in period may be extended for two more weeks after which participants will be examined as in Visit 4 (Visit 4A). In this event, the GFR measurement scheduled for Visit 4 will be conducted at Visit 4A. Participants whose SBP is > 140 or whose DBP is > 90 mmHg at the end of the run-in period will be discontinued from the study (prior to randomization).

#### **8.4. Enrollment in the Study and Randomization (Visit 5)**

At the end of the run-in period, eligibility will be re-assessed based on the BP measures obtained at Visits 4 or 4A (if applicable) and HLA-based genetic susceptibility to allopurinol skin reactions<sup>36,37</sup> (tested at Visit 4). Participants who are eligible for randomization based on those measures (SBP ≤ 140 and DBP ≤ 90 mmHg) and a negative HLA B\*5801 test will be asked to come in for Visit 5 during which they will undergo the following procedures:

- Obtain interval medical history.
- Collect family history information.
- Confirm inclusion/exclusion criteria are met.
- Complete Personal Locator Form.
- Review concomitant medications and AEs
- Review and adjust RAS and BP therapy to achieve study goals (see 7.3).
- Measure weight and vital signs.

- Inspect for skin rash.
- Collect samples for clinical laboratory assessments as outlined in Figure 1 and for storage for later biomarker research.
- Perform pregnancy test in women of childbearing potential.
- Randomization to allopurinol or placebo.
- Discuss how the study medication should be taken and its potential side effects.

If the participant is positive for HLA-based genetic susceptibility to allopurinol skin reactions, he/she will be discontinued from the study prior to randomization.

Immediately after Visit 5, participants will be mailed the first batch of study medication by the research pharmacy along with written instructions on how to take it. Participants will be instructed to notify the study personnel by phone and start taking the study medication as soon as they receive it.

#### **8.5. Treatment Period (Visits 6 to 15)**

During the treatment period, the following procedures will be completed at each visit for each participant:

- Obtain interval medical history.
- Review of concomitant medications and AEs.
- Review and adjust RAS and BP therapy
- Measure height (Visits 7 and 11), weight and vital signs.
- Inspect for skin rash.
- Conduct a physical exam (Visits 7 and 11).
- Update Personal Locator (Visits 7 and 11).
- Perform ECG according to the schedule outlined in Figure 1 (Visits 7 and 11).
- Collect samples for clinical laboratory assessments as outlined in Figure 1 and for storage for later biomarker research.
- Perform pregnancy test in women of childbearing potential.
- Measure GFR by means of plasma disappearance of non-radioactive iohexol, iGFR (Visits 7 and 11).
- Provide a container and instructions for an overnight urine collection whenever an AER measurement is scheduled at the following visit.

In the days immediately after each visit, upon completion of serum creatinine measurements, participants will receive a new batch of study medication by mail from the research pharmacy. Upon receipt of the new tablets, participants will be instructed to immediately mail the pill bottles with the tablets remaining from the previous prescription to the study center for drug accounting and compliance assessment (see 6.1.2). A pre-stamped and addressed envelope will be provided to participants for this purpose.

#### **8.6. End of Intervention (Visit 16)**

At the end of the treatment period (Visit 16), the following procedures will be completed for each participant:

- Obtain interval medical history.
- Review of concomitant medications and AEs.
- Review and adjust RAS and BP therapy.
- Collect unused study medication and document compliance.
- Measure height, weight and vital signs.
- Inspect for skin rash.
- Conduct a physical exam.
- Update Personal Locator.
- Perform ECG.
- Collect samples for clinical laboratory assessments as outlined in Figure 1 and for storage for later biomarker research.
- Perform pregnancy test in women of childbearing potential.
- Measure iGFR.
- Provide containers and instructions for 2 overnight urine collections to be made immediately before Visit 17.

Participants will be instructed to stop taking the study medication and to mail the pill bottles with the tablets

remaining from the last prescription to the study center if they did not already bring the unused study medication at the visit. The RAS and BP therapy will be continued as before until the closing visit (Visit 17). The importance of coming back in 8 weeks for the closing visit (Visit 17) will be emphasized.

### **8.7. End of Wash-out Period (Visit 17)**

After the end of the treatment period, participants will enter an 8-week wash-out period at the end of which the following procedures will be completed:

- Obtain medical history.
- Review of concomitant medications and AEs.
- Measure height and weight and vital signs.
- Inspect for skin rash.
- Collect samples for clinical laboratory assessments as outlined in Figure 1 and for storage for later biomarker research.
- Perform pregnancy test in women of childbearing potential.
- Measure iGFR.

### **8.8. RAS blocking and anti-hypertensive therapy after completion of the study**

When the participant completes the study, control of the RAS-blocking and anti-hypertensive therapy will be relinquished to the participants' physicians, who will decide whether or not to continue the therapy established during the study. Participants will continue the anti-hypertensive therapy established during the study until they see their physicians.

### **8.9. Future biomarker studies**

Plasma, serum, and urine specimens and DNA will be stored the Advanced Research and Diagnostics Laboratory at the University of Minnesota and the NIDDK Central Repository for possible future studies of biomarkers of kidney disease in diabetes or other diabetic complications. One 2.5 ml aliquot of plasma, one 2.5 ml aliquot of serum, and one 6 ml aliquot of urine will be stored at each visit, with the exception of Visits 4, 7, 11, 16, and 17 at which three aliquots of plasma, three aliquots of serum, and three aliquots of urine will be banked. Twenty ml of whole blood will be obtained during an early visit when smaller aliquots are obtained. This will be used for white blood cell DNA extraction and subsequent storage. Altogether, the stored plasma and serum aliquots will correspond to about 300-320 ml of blood collected for this purpose over the entire duration of the study. Participants will be allowed to elect to participate in the study while not having any or one or more of these samples stored, if so they choose.

### **8.10. Early Withdrawal**

Unless the participant withdraws consent, all randomized participants will be followed for the full study period (through week 164) and all data will be collected as scheduled.

## **9. SAFETY ASSESSMENTS**

### **9.1. Demographic Data/Medical History**

After collecting a detailed medical history at Visit 1, this information will be updated at each visit through a structured interview, with a special emphasis on skin symptoms and signs such as rash, itching and exfoliation and on pregnancy in females. Participants will be instructed to communicate any change in their health status and intervening hospitalizations to the study coordinator in-between visits. In particular, they will be instructed to discontinue study medication and immediately contact the study coordinator if they develop a skin rash of any kind, swelling of the lips or mouth, arthralgias, and/or jaundice, which may indicate a hypersensitivity reaction to allopurinol. Fever and chills should also be reported but would not require cessation of medication prior to discussion with study personnel.

### **9.2 Skin exam**

At each visit study on and after Visit 5, the skin of study participants will be examined for the presence of any kind of rash. Suspicion of drug allergy or Stevens-Johnson Syndrome SJS would require immediate discontinuation of study medication and dermatologic consultation.

### 9.3. Vital Signs

Blood pressure and heart rate will be recorded at each visit.

### 9.4. Clinical Laboratory Tests

Serum ALT, creatinine and K<sup>+</sup>, and CBC will be monitored and a pregnancy test, if a female of child bearing potential, performed at each visit (except Visit 2). Participants who are started for the first time on RAS blockers as part of this study will have their serum K<sup>+</sup> and creatinine measured at a local laboratory after 1 week of treatment. HbA1c will be measured at Visits 1, 5 and 7, and 8-17. An ECG will be performed at Visits 1, 4, 7, 11, and 16.

### 9.5. Management of Uric Acid Levels

Study participants and study personnel, other than the DCC and the study pharmacists, will be masked as to the uric acid levels obtained during the study. The patients' physicians will receive written requests to refrain from measuring uric acid levels during the time of the patients' participation in the study, except as is mandatory for the patient's wellbeing, e.g., in the treatment of malignancy or diagnosis of a clinical syndrome highly likely to represent gout. If gout is diagnosed, open-label treatment with allopurinol will become indicated. In such case, the study drug will be discontinued but the patient will remain in the study and will continue to be followed as if he/she was taking the study medication. If uric acid lowering for malignancy treatment is required, the patient will receive open-label treatment until such time as return to study drug is deemed clinically reasonable by their physician.

## 10. ADVERSE EVENT REPORTING

### 10.1. Definitions

An Adverse Event (AE) is any untoward medical occurrence in a participant administered an investigational medicinal product regardless of relationship to study treatment. A treatment-emergent AE is an adverse event occurring during the period between the first dose and 30 days after the final dose of the study medication. A Serious Adverse Event (SAE) is any untoward medical occurrence that results in death, is life-threatening, requires hospitalization or prolongation of an existing hospitalization, results in persistent or significant disability, or is a congenital anomaly/birth defect. Important medical events that do not fall into the above categories may also be considered an SAE when, based on medical judgment, such events may jeopardize the patient's safety and require medical/surgical intervention to prevent one of the outcomes listed in the SAE definition. The term SAE is not intended as a measure of severity or intensity.

A Suspected Adverse Reaction is any adverse event for which there is a reasonable possibility that the drug caused the adverse event. For the purposes of IND safety reporting, "reasonable possibility" means there is evidence to suggest a causal relationship between the drug and the adverse event. Suspected adverse reaction implies a lesser degree of certainty about causality than adverse reaction, which means any adverse event caused by a drug. An Unexpected Adverse Event or Unexpected Suspected Adverse Reaction is an adverse event or suspected adverse reaction that is not listed in the investigator brochure or is not listed at the specificity or severity that has been observed; or, if an investigator brochure is not required or available, is not consistent with the risk information described in the general investigational plan or elsewhere in the current application, as amended. For example, under this definition, hepatic necrosis would be unexpected (by virtue of greater severity) if the investigator brochure referred only to elevated hepatic enzymes or hepatitis. Similarly, cerebral thromboembolism and cerebral vasculitis would be unexpected (by virtue of greater specificity) if the investigator brochure listed only cerebral vascular accidents. "Unexpected", as used in this definition, also refers to adverse events or suspected adverse reactions that are mentioned in the investigator brochure as occurring with a class of drugs or as anticipated from the pharmacological properties of the drug, but are not specifically mentioned as occurring with the particular drug under investigation. An Expected Adverse Event or Expected Adverse Reaction is any adverse experience that has been identified in nature or severity in the current investigator brochure and/or protocol.

### 10.2. Adverse Events Reporting

All AEs will be reported on the Adverse Events form that will be completed by the study staff, who are masked as to study treatment assignment, at each regular follow-up visits. This will insure that AEs are ascertained in an unbiased manner using the same standardized methodology for participants in both treatment arms. Forms will include standardized questions relating to specific events of import in diabetic patients on either of the study treatment arms as well as any significantly abnormal physical finding identified on examination and

any significantly abnormal laboratory results obtained on the patient between visits or at the time of the visit. AEs reported or ascertained between clinic visits will be captured and reported at the time of the next schedule visit. Pre-existing conditions (that is, conditions present prior to randomization) will not be considered or recorded as AEs unless the condition worsens in intensity or frequency after randomization. Likewise, continuing AEs will not be reported as AEs at subsequent visits unless they increase in severity or frequency between visits, they results in criteria for a SAEs, and/or they resolve between visits. Each site will be responsible for reporting all AE's to their IRB according to its AE reporting policy and procedures.

### 10.3. Assessment of Causality and Severity

The seriousness of adverse events will be ascertained by the study staff according to the criteria listed in 10.1 and the need for further evaluation, follow-up, or referral. The relationship between study participation and AEs will be determined according to the following criteria:

- A. **Not related** – temporal relationship of the onset of the event, relative to study participation, is not reasonable or another cause can by itself explain the occurrence of the event.
- B. **Possibly related** – temporal relationship of the onset of the event, relative to study participation, is reasonable but the event could have been due to another, equally likely cause.
- C. **Probably related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and the event is more likely explained by the study treatment than by another cause.
- D. **Definitely related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and there is no other cause to explain the event.

### 10.4. Serious Adverse Events Reporting

See Section 15 – Data and Safety Monitoring Plan

## 11. STATISTICAL ANALYSIS

### 11.1. Analysis Population

An intention to treat (ITT) analytical approach will be employed. Accordingly, the population for statistical analysis will consist of all study participants considered in their original randomization group, regardless of treatment discontinuation or loss to follow-up.

### 11.2. Initial Data Analysis

The initial data analysis will be performed to detect any differences in distributions of characteristics measured at baseline, 4, 20, 36, and 38 months (0, 16, 84, 156, and 164 weeks, respectively) between study groups. The number of patients screened, enrolled, and completing the study will be summarized within and across study centers. Measures of central tendency (means, medians) and variability (standard deviations, ranges) will be estimated from the data for continuous variables. Frequency distributions will be provided for categorical data. This preliminary analysis step will provide us with insight into data, distributions of the variables considered, and will allow us to find additional invalid values not detected earlier during data validation.

### 11.3. Primary Efficacy Analysis.

For the primary endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention), we will follow the recommendations by Carpenter et al<sup>38,39</sup> and perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available iGFR measures (including those at baseline, 4, 20, 36, and 38 months [i.e., 0, 16, 84, 156, and 164 weeks, respectively]) as the dependent variable. By conditioning on the baseline iGFR measure we will also effectively use this variable as a covariate. Treatment group, study center, stratifying variables, baseline AER, time, and time by treatment interaction will be included as covariates in the model. Three features make this analytical approach especially attractive:

1. If there is no dropout (a very unlikely case), the estimate of the treatment effect at the end of the 2-month wash-out period following the 3-year intervention and its precision obtained using this approach will be exactly the same as those based on a classical approach employing an analysis of covariance (ANCOVA) model with treatment group, study center, iGFR and AER/ACR measured at baseline included as covariates.
2. If the iGFR measure at the end of the wash-out period is missing, we will be able to efficiently use the information contained in the intermediate iGFR measurements obtained at 4, 20, and 36 months, by virtue of them being correlated with the GFR measurement at washout. Estimate of the treatment effect obtained

this way is valid under the missing at random (MAR) assumption. This is in contrast to the ANCOVA approach, which would lead to the loss of this information and would require a more stringent assumption about the mechanism of data missingness, i.e. a missing completely at random (MCAR) mechanism.

3. The underlying analytical framework allows the use of all post-randomization data and is well suited to investigate the reason for withdrawal, for example to study whether participants having low iGFR values are more likely to withdraw.

Calculations will be performed using SAS PROC/MIXED. Results of the analysis will be expressed in terms of point estimate and its corresponding 95% confidence interval for the treatment effect at the end of the 2-month wash-out period following the 3-year treatment and will be accompanied by the corresponding p value.

#### 11.4. Secondary Efficacy Analyses.

1. The effect of treatment on the iGFR at 4 months after randomization will be evaluated using the same analytical approach employed for the primary outcome.
2. The effect of treatment on the iGFR at the end of the 3-year treatment period (before the washout) will be evaluated using the same analytical approach employed for the primary outcome.
3. The iGFR and eGFR time trajectories, estimated from periodical iGFR measures and quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations<sup>34,35</sup>, respectively, will be analyzed using linear mixed-effects models.<sup>40-42</sup> The main objective of the analysis will be to construct confidence interval for the effect of the intervention over three years of observation (treatment main effect) and investigate whether the effect of the intervention changes with time (time by treatment interaction).
4. Time to serum creatinine doubling or ESRD in the two treatment groups is subject to censoring due to dropouts or reaching the end of study before the participant experiences the event. Survival time will be defined as the time from randomization to the event (the first of serum creatinine doubling from baseline or occurrence of ESRD, defined as  $eGFR \leq 15$  ml/min/1.73 m<sup>2</sup>, hemodialysis, or kidney transplant) or, for participants who did not experienced an event, to the last study visit. Data will be summarized by means of Kaplan-Meier survival curves and by providing the proportions of participants surviving without events at 1, 2, 3 years, and at the end of the wash-out period along with their 95% CIs. Given the potentially small number of events, differences between study groups will be tested by means of the log rank test or by means of simple Cox regression models including a limited number of predictors in addition to treatment group.
5. The effect of treatment on the AER at the end of the wash-out period, based on the geometric mean of two AER measured at this time point and adjusted for the geometric mean of AER at baseline (Visit 3 and 4), will be investigated in a linear regression model framework as in the case of the primary outcome.
6. The effect of treatment on the AER at the end of the treatment period, based on the geometric mean of the AER measures at visit 15 and 16 adjusted for the geometric mean of AER at baseline (Visit 3 and 4) will be investigated as in #5.
7. Time to fatal or non-fatal cardiovascular events will be analyzed as proposed for time to serum creatinine doubling or ESRD.

#### 11.5. Incomplete Data.

Missing values represent a potential source of bias. Efforts will be made to keep all participants in the study. If this is not feasible, at least some information regarding the status at the end of the trial will be obtained. For randomized patients, the number of completing and dropouts will be summarized. This procedure will help to compare characteristics of the participants' groups who drop out from the study with those who completed the study by treatment group, within and across study centers. The models considered in the proposal allow for a missing at random (MAR) mechanism. MAR means that the missing values mechanism can be explained by observed data and does not depend on the *unobserved* values of outcome measures. The differences in distributions between characteristics of the groups may indicate potential sources of bias due to missing values. For instance some patients may dropout from the study due to *unobserved* factors related to the intervention itself. If we suspect such bias is present, the methods discussed in this section, assuming (MAR), are not applicable. We will incorporate plausible missing values mechanism into the model as discussed in Little<sup>43</sup> and investigate how such

mechanism may affect the estimates of treatment effect. To this end, sensitivity analyses will be conducted involving selection and/or pattern-mixture models<sup>44</sup> with an appropriate submodel used to describe dropout.

#### 11.6. Pilot participants.

All pilot participants who were already randomized to allopurinol or placebo during the pilot will be included in the final analysis of the pivotal trial. Those who do not consent to the pivotal trial will be treated as having dropped from the study at a time corresponding to their last pilot visit. Sensitivity analyses will be performed to investigate whether results may be potentially affected by the roll-over of pilot subjects in the pivotal trial.

#### 11.7. Model assumptions and alternative analyses

Model assumptions will be thoroughly checked for individual and systematic departures, using informal, e.g. inspection of residuals, and formal methods such as score test for extra parameter or methods based on likelihood displacement. If individual outliers are detected, their influence will be evaluated using influence diagnostics methods based on comparing estimates from models fitted to data with and without outlying values. Whenever we are not successful in fitting the parametric model (linear or non-linear), then non-parametric analyses and/or transformation of the variables involved in the analysis will be considered. To investigate the potential hemodynamic influence of allopurinol on treatment effect, in addition to the aforementioned analyses, we will consider models including the post-randomization measure of GFR at 4 months as an additional covariate. To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses will be performed by age groups ( $\leq 40$  and  $> 40$  yrs), gender, racial/ethnic group, HbA1c ( $\leq 7.8$  and  $> 7.8\%$ ), serum uric acid ( $\leq 6.0$  and  $> 6.0$  mg/dl), baseline GFR ( $\leq 70$  ml/min and  $> 70$  ml/min/1.73m<sup>2</sup>), and AER ( $\leq 300$  and  $> 300$  mg/24 hr). These additional analyses will be considered as strictly exploratory.

#### 11.8. Safety Analyses

Adverse events will be independently reviewed by an independent data safety monitoring board (DSMB, see Sections 15 and 16). All safety data will be available in data listing in the clinical protocol report. Data will be described in terms of descriptive statistics and presented by treatment group. Presentation will include graphs (scatterplots, boxplots, histograms), measures of central tendency (mean, median) and variability (confidence intervals) for continuous variables and frequency tables for categorical variables.

#### 11.9. Interim Analysis

No formal interim analyses of efficacy to stop for benefit or futility are planned, given the timing of the primary endpoint.

#### 11.10. Sample Size.

Since a variance-covariance matrix for the iGFR measures is not available and this matrix is essential in order to perform formal power calculations for a model with correlated errors, we performed alternative power calculations based on an intent-to-treat analysis within an ANCOVA framework. Specifically, we assumed that the primary hypothesis is tested in the following model:

$$M1: \text{iGFR at washout} = \text{iGFR at baseline} + \text{treatment group}$$

Compared to the model that will be used in the primary analysis, model M1 is simplified in two aspects. First, it does not use information from iGFR values measured at intermediate time points. Second, it does not include covariates such as the stratifying variables (HbA1c and UA) or other GFR predictors such as baseline AER. Both of these aspects may lead to loss of precision of the treatment effect estimate. Consequently, our sample size calculations should be considered as conservative.

The hypothesis being tested, i.e. the effect of treatment on iGFR at washout, corresponds to testing whether the treatment group factor in Model M1 is significant. The choice of the ANCOVA model for the purpose of power calculations is sensible, as residuals from a univariate model involving baseline iGFR as covariate fitted to data from RASS study conform to normal distribution. Sample size calculations were performed based on Cohen<sup>45</sup> and making the following assumptions:

1. Postulated effect on iGFR at washout ( $\Delta$ ) = 3 ml/min/1.73 m<sup>2</sup>. We deem this effect to be clinically meaningful and attainable. It is clinically meaningful because it would translate on average into a 10-year delay in the progression to ESRD. It is attainable because it is smaller than the difference in 3-year GFR that we observed in the JKS between subjects with serum UA  $\geq 4.5$  mg/dl compared to those with levels

below this value. The postulated effect was based on the following changes in GFR levels in the two treatment groups:

- a. Untreated group = 3 ml/min/1.73 m<sup>2</sup> per year. This estimate is based on data from the Joslin Kidney Study (JKS), in which the median GFR loss among 43 subjects meeting the above criteria was 3.1 ml/min/1.73 m<sup>2</sup> per year, with 70% of subjects having a GFR loss >1.5 ml/min/1.73 m<sup>2</sup> per year. Also, among 116 subjects from Steno who met the albuminuria and GFR criteria, but for whom serum uric acid values were not available, the median GFR loss was 3.3 ml/min/1.73 m<sup>2</sup> per year, with 71% of subjects having a GFR loss >1.5 ml/min/1.73 m<sup>2</sup> per year.
  - b. Treated group = 2 ml/min/1.73 m<sup>2</sup> per year. The average GFR loss in the JKS subjects with serum UA <4.5 mg/dl was 1.5 ml/min per year. On this basis, we conservatively assumed that the allopurinol treatment, if effective, would decrease the GFR loss to 2 ml/min per year (a 33% decrease compared to the untreated group).
2. Standard deviation (SD) of residual error = 10.1 ml/min/1.73 m<sup>2</sup>. This was estimated based on the root-mean-squared error from a regression model with eGFR at 3 yrs as the dependent variable and baseline eGFR as the independent variable fitted to data concerning T1D patients from the Joslin Kidney Study meeting the PERL inclusion criteria.

Assuming a two-sided alpha error equal to 0.05, the effective sample size needed to detect the pre-specified treatment effect ( $\Delta = 3 \text{ ml/min/1.73 m}^2$ ) at washout adjusted for baseline iGFR with 80% power is equal to n=180 per group. To take into account the anticipated overall dropout rate (up to 5%/yr or 15% over the entire duration of the study) and drug discontinuation or non-compliance in the treatment group (up to 2%/yr or 6% over the entire duration of the study), and to maintain the desired power of at least 80%, it will be necessary to recruit n=240 subjects per group. In Table 1, we show the power of the proposed sample size for Model M1 under

different dropout and non-compliance scenarios. We also provide the corresponding power for a model (Model M2)

including the two stratifying variables (Hb1Ac and UA) and baseline AER as covariates to illustrate the effect of adding these variables to Model M1. In this analysis, we assumed that adding these covariates reduces the residual

variance by 10%, which corresponds to these covariates explaining merely 4% of the total iGFR variation over and above the variability explained by iGFR at baseline. As shown in Table 1, once these covariates are accounted for, power is expected to exceed the conservative estimates provided by Model M1 and reach almost 90% for 15% dropout and 6% non-compliance rates.

**Table 1. Power to detect treatment effect for two ANCOVA models under different drop-out and non-compliance scenarios.**

Overall Dropout (%)	Non-compliance (%)	Model	
		M1	M2
9	0	.87	.92
12	0	.86	.91
15	0	.85	.90
9	6	.83	.89
12	6	.82	.88
15	6	.80	.87

**12. DATA COLLECTION AND QUALITY ASSURANCE**

Comprehensive data coordinating center (DCC) functions for this clinical trial, including clinical monitoring, database development, web-based data entry and management, as well as the creation and export of study reports for the DSMB will be provided by the University of Michigan Statistical Analysis of Biomedical and Education Research (SABER) group. Housed in the top nationally ranked Department of Biostatistics, SABER, in its 13-year existence, has served as the DCC for over 50 studies, including multiple NIH-sponsored networks.

The DCC will use OpenClinica® (OpenClinica Clinical Trial Software; OpenClinica, LLC, Waltham, MA), a clinical trial software platform for electronic remote (i.e., site-based entry) data capture and clinical data management, as the basis for our custom-designed data entry and management system. We expect that the majority of data will be collected via Case Report Forms (CRFs); however, other data sources, such as laboratory data from the central laboratory, may be used. In these circumstances, the DCC will also utilize electronic data transfer. Protocols for the transfer of data, with careful attention to data integrity, will be written by experienced programmers and stored in the OpenClinica database or data mart.

The DCC has established a set of standard operating procedures (SOPs) governing the processes used to ensure patient privacy and data confidentiality, including the use of anonymous participant IDs on CRFs and in reports. In addition to clinical study databases, the UM DCC has also incorporated MEDdra® [www.meddrasso.com/] and WHODD® [www.who-umc.org/] databases into our systems to have the capacity to



code adverse events and illnesses by body organ system, respectively. OpenClinica® enables compliance with Good Clinical Practice (GCP) and regulatory requirements by providing differentiated user roles and privileges, password and user authentication security, electronic signatures, SSL encryption, and comprehensive auditing to record and monitor access and data changes.

### **12.1. Case Report Forms**

Study information will be collected for each participant by study staff using standardized electronic Case Report Forms (CRFs). CRFs will be developed by the DCC, modeling their formats on the CRFs developed for the RASS clinical trial<sup>33</sup>, to which the study group has access through Dr. Mauer. CRFs will not report information about treatment assignment, in order to maintain blinding of study site. Forms will be stored at a secure location at the clinical sites.

### **12.2. Quality Control and Quality Assurance**

DCC staff will prepare data management and clinical monitoring plans. The clinical monitoring plan will detail procedures to assess accuracy of the database relative to source documents, as well as site adherence to regulatory and study procedures. Emphasis will be placed on the process of consenting subjects, compliance with regulatory requirements and study protocol, values of key endpoints, and identification of SAEs that may not have been reported. The data management plan will describe the front-and back-end edit checks, as well as forms tracking procedures, that will be implemented to ensure timely and high-quality data collection. It will also define the periodic reports that will be shared with site coordinators and PIs that summarize site performance. The clinical monitoring and data management procedures will be consistent with the International Conference on Harmonisation (ICH E6) standards for Good Clinical Practice (GCPs).<sup>46</sup>

#### **12.2.1. Clinical monitoring.**

During trial conduct, the DCC will conduct periodic monitoring visits to ensure that the protocol and GCPs are being followed. The monitors may review source documents to confirm that the data recorded on CRFs is accurate. The investigator and institution will allow DCC monitors direct access to source documents to perform this verification. It is important that the investigator(s) and their relevant personnel are available during the monitoring visits and that sufficient time is devoted to the process.

Clinical monitoring will be conducted through approximately yearly on-site visits by qualified personnel. In the first year, site initiation visits will be conducted to review the protocol, verify that the Site Director and his/her collaborators receive all necessary trial documents for a proper trial conduct, and review the procedures related to CRFs completion and query resolution. Investigators will be instructed about the importance of recording accurate and clean data, avoiding protocol violations, and retaining participants in the study. Follow-up monitoring visits will involve the verification of source documents and reporting of adverse events. Monitors will also verify that an informed consent form is on file for each subject screened, with appropriately dated signatures and all pages present, that all of the inclusion and exclusion criteria were met for each subject enrolled into the study, and that all the withdrawals and dropouts of enrolled participants from the trial are reported and explained in the appropriate CRFs. Accrual and retention rates will also be monitored and if these fall appreciably below the projected levels, attempts will be made to identify the reasons. At the end of each monitoring visit, a clinical monitoring report will be prepared by the DCC Clinical Monitor and sent to the site PI and to a NIDDK representative with recommendations to correct problems and/or improve the trial quality.

#### **12.2.2. Statistical monitoring.**

Clinical monitoring will be complemented by statistical central monitoring, as described by Venet et al.<sup>47</sup>. Such a statistical approach to central monitoring relies on assessing the clinical data for departures from expected patterns (e.g., baseline variables in our randomized trial should be comparable between treatment arms for each site; visit days should be randomly distributed over the week), and assessment of a greater number of data values than those associated with site performance metrics. The statistical monitoring plan will be incorporated into the larger data management plan, and will identify the specific descriptive statistics, graphical presentations, and formal hypothesis tests to be performed and at what frequency. Pattern recognition may require a sufficient number of participants followed for a sufficient amount of time in order to be valid.

#### **12.2.3. Laboratory quality monitoring.**

The Central Laboratory at the U. of Minnesota uses internal quality control methods to assess assay precision and drift as well external quality control (e.g., proficiency testing) to assess accuracy. The laboratory is well versed in both aspects and adheres to rigid performance standards. For all analytes, enough commercial

lyophilized control material is typically purchased prior to beginning the study, so that a single control pool lot can be used throughout. Control pool mean and between-day SD's for each analyte are routinely established at a minimum of two different analyte concentrations on a minimum of one hundred different analytical batches. In the unlikely event that a new control pool lot becomes necessary during the study, both the new and old control pool lots will be run for a minimum of 20 days to establish a target mean for the new pool. The target SD's are not changed if the new and old lots of control material are the same "matrix" (e.g., lyophilized human serum). An in-house control pool is also typically prepared from the exact same specimen type (e.g., 0.5-mL aliquots of pooled serum or EDTA-plasma from several individual donors), kept at -70°C, and incorporated as an internal control in all analytical batches throughout the study. This sort of control is very useful since occasionally the commercial pools of lyophilized serum have matrix effects that make assessment of the true accuracy of the given assay difficult to assess. Accuracy is assessed by comparison of external quality control proficiency testing results. For most of the assays, the laboratory participates in either the College of American Pathologists' Surveys Program (uric acid, creatinine, ALT, glycosylated hemoglobin, urine albumin, urine creatinine) or by sample exchange in a survey program with another laboratory (iohexol). The laboratory is CLIA certified.

### 12.3. Study Record Retention

The source documents will be stored for at least 10 yrs after the study ends.

### 12.4. Data and Biosample Archiving in the NIDDK Central Repository.

In agreement with NIDDK's policy on the sharing of data from large, NIDDK-sponsored, multi-site studies, the data and biosamples collected in the course of the trial will be archived in anonymized form in the NIDDK Central Repository for future distribution to the scientific community. All samples and data transferred to the Repository will be under the custodianship of the NIDDK, although the study's Steering Committee will have proprietary control of and exclusive access to the sample and data for an agreed-upon period of time before these are made available to the wider scientific community.

## 13. PROTECTION OF HUMAN SUBJECTS

### 13.1. Characteristics of the study population

Participants will be patients who have had T1D for 8 years or more and who will be 18-65 years old at entry into the study. We anticipate that there will be approximately an equal number of males and females. There will be no selection criterion based on race, although most patients will be of Western background and European extraction, given the demographics of the cities in which the centers are located and the fact that T1D is 30-40% less common among Blacks and Hispanics than among Whites. Inclusion and exclusion criteria are as noted above. Female patients of child-bearing potential will be included in the study but only if pregnancy is not planned during the time frame of the study. Women who become pregnant after randomization will be discontinued from the study medication until pregnancy and breast feeding are complete; iGFR will not be obtained during or for 6 months after pregnancy is completed. Individuals younger than 18 will not be included since kidney complications are rare before this age. Patients will be tested for HLA-based genetic susceptibility to Stevens-Johnson Syndrome and excluded if this is found.

### 13.2. Sources of research material

- a. Specimens on patients obtained specifically for research purposes.
  1. Renal function studies requiring multiple blood specimens drawn from an indwelling IV over 4-6 hours for measurement of glomerular filtration rate at yearly intervals.
  2. Collection of urine for measurement of urinary albumin and creatinine at 3-6 month intervals.
  3. Blood for measurement of serum UA, creatinine, and liver enzymes, and WBC at quarterly intervals.
- b. Specimens or measures obtained quarterly as component of routine patient care
  1. HbA1c
  2. Blood pressure
  3. Height and weight
- c. Patient and family medical information.

All study participants will be assigned a unique study identifier and in no publications or public presentations will information be available which could identify individual study participants or their families.

### 13.3. Plans for recruitment of subjects and consent procedures.

Potential participants will be sought (1) from among the patients attending the study centers (including the satellite centers) involved in the study and (2) by placing advertisements at other health care facilities and in local newspapers or other media. At each clinical site, potential candidates will be identified and contacted according to the procedures established by the local IRBs in compliance with local laws protecting patient confidentiality. Invitation letters to patients attending the study centers will clearly offer the possibility to opt out of any further contacts with the study. Patients who agree to participate will be screened by means of a telephone or in-person interview to determine whether exclusion criteria apply. Subjects who respond to advertisements will undergo the same screening interview. Subjects who pass this initial screening will be given or mailed an informed consent form and will be invited to come to the clinic for a screening visit (Visit 1) during which a final eligibility determination will be made on the basis of a detailed medical history and laboratory tests. Written consent will be obtained on that occasion from all subjects undergoing the screening visit after explaining again the purpose and procedures of the study. In the initial contact and again at the time of the screening visit, study subjects will be encouraged to ask questions and they will be reassured that they may withdraw from the study at any time. The consent form will again be reviewed and consent affirmed at the visit prior to randomization (Visit 4).

### 13.4. Potential Risks

#### 13.4.1. Risks associated with screening procedures and blood tests

After participating in the screening tests and procedures, or after the run-in period, subjects may find out that they are not eligible to participate in the study. In that case, they will be told the reasons for their ineligibility and will be given the results of clinically approved tests such as serum UA, serum creatinine, urinary albumin/creatinine ratio, and ALT. The results of the test for genetically increased risk of allopurinol-induced SJS will also be given to their physician, if the subject agrees with this. Thus, they may learn about as yet unknown health problems such as anemia or liver disease, more advanced kidney disease, or the need for allopurinol avoidance. This and/or the exclusion from the study may cause psychological distress. The drawing of blood samples may cause some pain and discomfort and hematoma formation at the site of venipuncture. The total amount of blood taken for the entire study will be about 700 mL (26 mL at Visits 1, 3, 5-6, 8-10, and 12-15; 67 mL at Visits 4, 7, 11, 16, and 17). At the dose used in the study, there are no known risks to the infusion of the substance used for the measurement of renal function other than the very small risk of allergic reactions (<0.5%), diminished by the exclusion of patients with a history of iodine allergy and by having appropriate treatment drugs for allergic reactions on hand.

#### 13.4.2. Risks associated with allopurinol treatment

Allopurinol has been used for several decades for the long-term therapy of symptomatic gout. The risks associated with its use are low and include:

- a. Skin rashes, usually pruritic maculopapular skin eruptions, sometimes scaly or exfoliative, are the most commonly reported adverse effect of allopurinol. Skin reactions were observed in the past in up to 3% of treated patients, but more recent data suggest that their frequency is now less than 1% ([www.drugs.com/pro/allopurinol.html](http://www.drugs.com/pro/allopurinol.html)) perhaps more likely due to changes in the filler compounds rather than the actual drug. Rashes may be followed by more severe hypersensitivity reactions such as exfoliative lesions and the Stevens-Johnson syndrome (erythema multiforme major), which can be fatal. Although such occurrence is very rare, in the order of 1 in 10,000<sup>18</sup>, treatment with allopurinol will be immediately discontinued if a rash develops and will not be reinstated. As noted, those with HLA-based genetic susceptibility to allopurinol-related SJS will be screened out. About 0.7% of Whites and 2-3% of African Americans and Asians are carriers of such genetic susceptibility.
- b. An increased frequency of acute gout attacks has been reported during the early stages of allopurinol administration, possibly resulting from the mobilization of urates from tissue deposits causing fluctuations in serum UA levels. Early studies estimated the risk of such events to be about 6%, but an analysis of current usage suggests that the risk has now decreased to less than 1% ([www.drugs.com/pro/allopurinol.html](http://www.drugs.com/pro/allopurinol.html)). The risk is expected to be even lower in this study population since individuals with a previous history of gout will be excluded and UA levels will be on average lower than in patients usually taking allopurinol for elevated UA levels.

- c. Reversible liver damage as well as asymptomatic rises in liver enzymes has been observed in 1-2% of patients taking allopurinol. Some very rare cases of irreversible liver damage have been observed in the context of the Stevens-Johnson syndrome.
- d. Bone marrow depression has been reported in patients receiving allopurinol, most of whom received concomitant drugs with the potential for causing this reaction. Bone marrow depression has been rarely observed in patients receiving allopurinol alone.
- e. Experience with allopurinol during human pregnancy is limited because women of reproductive age rarely require this treatment. Given this paucity of data, the study will consider it unsafe for the fetus or the mother to receive this drug. Allopurinol has been found in the milk of a mother on this drug and, therefore, will not be taken by nursing mothers.

**13.4.3. Risks associated with RAS blocker treatment.**

Treatment with RAS blockers (either ACE inhibitors such as ramipril or angiotensin receptor blockers such as irbesartan) is currently the standard of care for diabetic individuals who have micro- or macroalbuminuria. The risk associated with the use of these drugs during the trial will not be greater than the risks participants would face outside the trial by being treated with these agents. These risks include allergic reactions, hyperkalemia, hypotension, increased serum creatinine, persistent cough (with ramipril), liver damage, and bone marrow depression. The occurrence of these adverse events will be monitored during the trial.

**13.5. Procedures for protecting against and minimizing potential risks**

**a. General**

The patients are under constant medical supervision. They are told that the data which are collected will be used for scientific report, but they will not be identified in such reports.

**b. Specific**

- 1. Regular pregnancy tests and education regarding fetal risk of the study drug will be provided to female patients of child bearing age.
- 2. Quarterly measures of liver enzyme and white blood cell count will allow for early detection of liver injury or leucopenia potentially representing drug toxicity.
- 3. Quarterly measures of serum creatinine will allow titration of allopurinol in relation to kidney function to avoid excessive dosage of the medication.
- 4. IV's for kidney function studies will be placed by trained skilled clinical research nurses or technicians or by experienced physicians.
- 5. Blood drawing for laboratory studies will be performed by trained skilled phlebotomy personnel at the respective institutions, thus limiting the risk of discomfort or local hematoma formation.
- 6. Participants will be advised not to donate blood throughout the time they are in the study.
- 7. Regarding possible drug toxicity:
  - a. To avoid fetal risks from the study drug, patients planning pregnancies will not be included. Sexually active female patients will be instructed to immediately discontinue study drugs if a menstrual period is missed by more than two weeks and, if found to be pregnant, the study medication will be discontinued and not resumed until pregnancy and nursing are completed. Pregnancy tests will be done quarterly on all women in the study.
  - b. Subjects with known allergy to xanthine-oxidase inhibitors will be excluded from the study. Patients will be instructed to immediately report skin reactions and allergic symptoms and to immediately stop the study medication should these occur. Patients will be given antihistamines for symptom relief. A small supply of antihistamines to be used in such an event will be supplied to each patient. Should an allergic reaction or skin rash occur, the study drug will be permanently discontinued.
  - c. To minimize the risk of gout attacks, subjects with a gout history will be excluded from the study and the allopurinol dosage in those enrolled in the study will be gradually increased over several

weeks. Should a gout attack occur, this will be treated with colchicine or anti-inflammatory agents according to current standards of care by study personnel. Study uric acid levels <2.0 mg/dl will be flagged by the DCC and reported to the appropriate study pharmacist who will initiate a 50% dose reduction in study drug at the next quarterly visit. In order to avoid gout attacks, if uric acid levels exceed 12 mg/dl this will be flagged by the DCC and the center informed and open-label allopurinol will be started and titrated with the goal of bringing and keeping serum uric acid below 7.0 mg/dl. Participants will continue to be followed according to the study protocol and will be analyzed according to their blinded treatment groups.

- d. Primary care physicians will be notified (with the participants' permission) of the patients' participation in the trial, so that they avoid the prescription of drugs interacting with allopurinol or notify the study personnel that treatment with such drugs is necessary.
- e. Participants will be reminded at each visit to immediately notify the study personnel if they start a new drug, so that possible interactions with allopurinol can be identified at once and appropriate precautions can be taken including discontinuation of the study drug.
- f. Subjects taking drugs known to interact with allopurinol in causing bone marrow depression will be excluded from the study. White blood cell counts will be done before the study drugs are prescribed, and quarterly thereafter and the study drug discontinued should evidence of bone marrow depression ( $WBC < 3500/mm^3$ ) develop. The Drug Monitoring Committee will review each case and decide whether a referral to a hematologist is warranted and whether study treatment can be reinstated after blood values have returned to normal. If drugs potentially causing bone marrow depression in combination with allopurinol are begun after entry into the trial, observations for this side effect will be intensified or, if recommended by the Drug Monitoring Committee, study drug may be interrupted.
- g. To minimize the risk of allergic reactions during the iGFR measurement, subjects with a history of iodine allergy will be excluded from the study.
- h. To minimize the risk of liver injury, subjects with clinically significant hepatic disease and/or elevated liver enzymes above 2.5 x the upper limit of normal at the screening visit will be excluded from the study. In those subjects that are enrolled in the study, liver enzyme levels will be monitored at each follow-up visit. If levels are abnormal, the measurement will be repeated and if values are confirmed to be elevated the study drug will be discontinued. The Drug Monitoring Committee will review each case and decide whether a referral to a hepatologist is warranted and whether study treatment can be reinstated after enzyme values have returned to normal on the recommendation of a hepatologist.
- i. To minimize the impact of blood draws, participants with low hemoglobin levels (<11 g/dL in males, <10 g/dl in females) will be excluded. Subjects will be advised not to donate blood while participating in the study and for two months after their participation has ended. If they have just donated blood, their screening for the study will be delayed by 3 months. Hemoglobin levels will be monitored quarterly.
- j. Most of the participants will already be on an ACE inhibitor. Thus, switching them to another drug of the same class (ramipril) should not pose significant risks. In the remaining participants (taking an Angiotensin Receptor Blocker [ARB] or not taking any RAS Blockers), risks will be minimized by not prescribing RAS Blockers to participants who have contraindications to these drugs and by prescribing an ARB (irbesartan) whenever ACE inhibitors are contraindicated. If adverse events develop that are deemed to be related to the use of RAS blockers, the dose of these drugs will be decreased, followed by their discontinuation if the problem persists (see 8.2. Run-in period), thus adhering to current standards of care. If receiving discontinuation of RAS blockade becomes necessary, BP will be managed by alternate drugs as described above.
- k. Blood pressure will be measured quarterly with the goal of maintaining BP <130 mmHg systolic and <85 mmHg diastolic. If elevated a recheck will be performed within 2 weeks and if still elevated additional antihypertensive non-RAS blockers will be added from a limited menu of agents as prescribed in the MOO. Failure to achieve satisfactory BP control within 2 months would lead to a Drug Monitoring Committee conference call. The patient's physicians would be asked to

relinquish BP management to PERL personnel in order to achieve uniformity of goals, but the patient's physicians would be informed of any BP medication changes.

- l. Participants with a decrease in both iGFR (meeting the  $R^2$  criterion described in 7.1.2) and eGFR from one measurement to the following one corresponding to a GFR decline >20% per year will be referred to a nephrologist to investigate the causes of such rapid loss of kidney function. If a decrease of such magnitude is observed for the iGFR but is less than a 20% decline per year for the eGFR, the iGFR measurement will be repeated. If the >20% per year iGFR decrease is confirmed, the participant will be referred to a nephrologist for further evaluation. In this case, the first iGFR value will be used for the analysis of the primary outcome. If the >20% per year iGFR decrease is not confirmed, the participant will not be referred to a nephrologist. The first iGFR test will be reviewed to verify whether medical conditions that should have prompted a test postponement, such as dehydration or recent use of NSAID (see 7.1.1.3), were present. In that case, the repeated iGFR value will be used for the primary outcome analysis. Otherwise, the first iGFR value will be used.
- m. Data monitoring will be performed on a regular basis. Data entry computers will be programmed to flag any parameters outside clinically acceptable ranges.

### **c. Protection of confidentiality**

All data, forms, and specimens will be labeled with each study participant's unique study identifier. All data transferred to the Data Coordinating Center for accumulation in the central database or to the NIDDK Central Repository will identify study participants only with their unique study identifier. Each study center will maintain a file on each study participant that includes personal identifiers, linking name and contact information to the unique study ID. These data will not be entered into the study data management system. Participants' names and addresses will be shared with the Pharmacy along with selected laboratory results (serum creatinine and, if needed, uric acid) for the purpose of adjusting the dosage and mailing the study medication. Identifiers may also be shared with the local laboratories if required by the laboratory ordering procedures. Study participants' files will be kept in secure locations and the clinical center will be responsible for taking every other reasonable measure (those set by the state, the site, and the study) to ensure and maintain record confidentiality and patient privacy. Participants will be given the opportunity to decide whether or not the clinical information gained from the study should be shared with their health care providers. Participants will be made aware that, despite these measures, confidentiality cannot be totally ensured. Each site will adhere as required by law to regulatory oversight by federal and state agencies that have authority over the conduct of clinical research such as the Department of Health and Human Services, the Food and Drug Administration, the National Institutes of Health, the Office of Human Research Protection, the Department of Social Services and the Data Safety Monitoring Board.

### **d. Risk-benefit ratio**

If urate-lowering therapy is demonstrated to be effective in preventing or slowing early GFR decline, the reduction in morbidity and mortality resulting from the prevention or delay of ESRD would have a major impact on the lives of T1D patients as well as on society at large, significantly reducing the human suffering and financial costs associated with this condition. Also, demonstrating a causal link between serum UA and kidney damage in T1D would prompt further research on the molecular mechanisms responsible for this link, which could lead to the development of further interventions to prevent renal disease in T1D. Overall, the risks to study participants are deemed reasonable in relation to the anticipated benefit of identifying an effective therapy for early GFR loss in type 1 diabetes.

### **13.6. Incentives/remuneration.**

If allowed by local regulations, participants will receive \$740 (\$20 for each visit completed at the clinic not involving the kidney function test and \$100 for each visit completed at the clinic involving the kidney function test) as a reimbursement for the time and discomfort associated with participating in this study. Payments will be made at each visit. Participants with financial hardships deriving, for example, from loss of income or child care costs, may be reimbursed for such costs on a case by case basis. Participants who do not complete the whole study will only be reimbursed for the visits they completed. Parking will be paid by vouchers on the days participants are asked to come in for a study visit. For participants who do not drive, public transportation costs may be reimbursed up to the same costs of parking. If this study should result in the development of any marketable product, profits will not be shared with participants.

### 13.7. Institutional Review Board

The protocol and informed consent forms and subsequent modifications will be reviewed and approved by the Human Subject Committees at all the centers involved in the study for compliance with applicable standards/regulations.

### 14. DATA AND SAFETY MONITORING PLAN

The Data and Safety Monitoring Plan for this study includes the following elements:

1. A *Data Safety Monitoring Board (DSMB)*, including outside experts in the design and conduct of clinical trials and in diabetic nephropathy, will be established by NIH. The purpose of the DSMB is to assure independent review as to whether study patients are exposed to unreasonable risk because of study participation, and to monitor study progress and integrity. The DSMB will receive detailed data from the Data Coordinating Center as frequently as deemed appropriate by the board, including summary tabulations and narratives of adverse events, and will meet periodically with the Study Investigators and the Data Coordinating Center personnel. They will have full access to all data, and their recommendations and input will be given high priority and will be incorporated into the study protocol. To this end, the DSMB will meet separately, "*in camera*" (Closed Sessions), with the Co-Director of the Data Coordinating Center, Dr. Andrzej Galecki, to review all adverse event data in relation to the randomized treatment groups in order to detect any increased frequency of significant adverse events which could be study drug related, and decide whether continuation of the trial is warranted.
2. *IRB monitoring* will be in place from:
  - ◆ Joslin Diabetes Center
  - ◆ University of Minnesota
  - ◆ University of Colorado
  - ◆ University of Michigan
  - ◆ Northwestern University
  - ◆ University of Toronto
  - ◆ Albert Einstein College of Medicine
  - ◆ Steno Diabetes Center
3. *SAE reporting.*  
All adverse events are reported to the DCC by completion of the Adverse Events Form. All SAEs as defined previously will require expedited event notification within 72 hours of occurrence or identification to the DCC. The DCC will promptly notify the study PIs, who may convene a Drug Monitoring Committee (DMC) conference to acquire further information about the event and take appropriate actions concerning the study medication (see Section 15.1).

An independent physician not involved in the study will serve as the Medical Safety Officer, reviewing all SAEs promptly after being reported in the database by the clinical sites. Based on the clinical site report and any additional input from the DMC, the Medical Safety Officer will prepare a preliminary SAE narrative report (in cases where the SAE is not resolved) for each SAE which will be distributed to the PIs, NIDDK Program Director, DSMB Chair, clinical site director, and appropriate DCC staff. Once the SAE is resolved, a final SAE narrative report is generated by the Medical Safety Officer. This report will be sent to the clinical site PI to review for accuracy and completeness. Following review by the clinical site PI, the Medical Safety Officer will send the final SAE narrative report to the PIs, NIDDK Program Director, DSMB Chair, clinical site director, and appropriate DCC staff. All SAE narrative reports, both preliminary and final, will be reviewed by the DSMB during their regularly scheduled meetings or on an expedited basis as determined by the NIDDK Program Director, who will solicit the input of the Chair of the DSMB as needed. The FDA definitions and requirements for expedited reporting will be used to determine if any individual SAE warrants notification to the FDA and to the IRBs of all participating PERL clinical sites.

The clinical site at which the SAE occurred is responsible for expedited reporting of the SAE to their respective IRB. Each site is responsible to report all AE's to their IRB according to its AE reporting policy and procedures.

On behalf of the NIDDK, the Data Coordinating Center will submit an expedited safety report to the FDA for all serious unexpected suspected adverse reactions (SUSARs). That is, when the SAE is unexpected and may be related to the study drug based on evidence of causality. This report will include information

on frequency of similar events along with a narrative of similar events to provide context for the individual report. Copies of the expedited safety report will be provided to the PIs, NIDDK, DSMB, and site investigators.

4. When collecting data on participants, adequate safety levels will be set for flagging test results. When these levels are reached, the Data Coordinating Center will notify the appropriate clinic that an abnormal result has been received. Detailed follow-up procedures will be set in the Manual of Operations that will be followed by the clinic when any abnormal results are received.
5. Monthly conference calls will be scheduled for the Steering Committee (SC) and the Trial Coordinators. Subject participation and compliance will be discussed in detail during these calls. A clinical psychology expert in the behavioral and compliance aspects of clinical trials, Dr. William Robiner from the University of Minnesota, will be included in the Trial Coordinator calls when discussing participant compliance issues.
6. A Drug Monitoring Committee (DMC) consisting of the PERL Center Directors and PIs, a research pharmacist, and the Project Manager will hold conference calls regarding any serious medication related problem that a participant has. Changes in study medication dose, medication discontinuation and medication re-institution will be discussed during these calls.
7. Twice a year, the Study Group will meet face-to-face with the Data Coordinating Center personnel for a 1½ day meeting to discuss the study in detail and any problems that may have occurred. The Trial Coordinators will hold a separate ½ day meeting with the Data Coordinating Center prior to the SC meeting and any issues needing discussion will be presented at that time and carried from and to the main Study Group meeting for discussion and resolution.

## 15. STUDY ADMINISTRATION

### 15.1. Organization

The major organizational components of the study are:

- The *Study Group* is composed of all investigators and study staff from the Clinical Sites, the Data Coordinating Center, and the Central Laboratory. The Study Group is responsible for the conduct of the study.
- The *Steering Committee* is responsible for the design of the study and provides guidance to its execution. Members are the co-Chairs of the PERL Consortium (Drs. Mauer and Doria), the Directors of the Clinical Sites (Drs. Caramori, Goldfine, Maahs, Perkins, Pop-Busui, Molitch, Crandall, and Rossing), the Directors of the Data Coordinating Center (Drs. Galecki and Spino), and the Director of the Central Laboratory (Dr. Eckfeldt), and the NIH program officer (TBD).
- The *Executive Committee* will consist of the two PIs, Drs Doria and Mauer, the DCC leaders, Drs Galecki and Spino, the Project Manager, and the Lead Clinical Coordinator. The EC will have at least monthly conference calls to discuss the overall conduct of the study and set the agendas for the Clinical Coordinators and Steering Committee conference calls. The EC will be responsible for the overall quality of the study, the setting of broad policy directions, and will address major budgetary issues, including, if necessary, reallocation of funds based on developed parameters of need and performance.
- The *Drug Monitoring Committee* is responsible for the oversight of the study drug administration as well as the RAS blocking and antihypertensive therapy during the trial. Members are Dr. Doria, Dr. Mauer, the PIs of the clinical sites, the Project Manager, the Lead Clinical Coordinator, and a research pharmacist. The participation of one of the PIs and 5 of the 8 Center Directors will be sufficient for making decisions.
- The *Clinical Sites* are located at the Joslin Diabetes Center, the University of Minnesota, the University of Colorado (Barbara Davis Center for Childhood Diabetes), the University of Michigan, Northwestern University, Albert Einstein College of Medicine, the University of Toronto and the Steno Diabetes Center (Denmark) are responsible for recruiting study participants and implementing the protocol.
- The *Data Coordinating Center (DCC)*, based at the University of Michigan, is directed by Drs. Galecki and Spino is responsible for managing the trial on a day-to-day basis, monitoring enrollment, retention, and protocol adherence and for collecting, monitoring, editing, and analyzing data from the Clinical Sites.



- The *Central Laboratory*, located at the University of Minnesota, is directed by Dr. Eckfeldt, and is responsible for all blood and urine tests.
- The *Data Safety Monitoring Board (DSMB)* will be composed of to-be-named outside experts in the design and conduct of clinical trials and in diabetic nephropathy. The board will be responsible for reviewing the study documents, monitoring study progress and participant safety.

Monthly conference calls will be scheduled for the Steering Committee and the trial coordinators to discuss subject participation and compliance. Twice a year, the Steering Committee, the Data Coordinating Center, and the trial coordinators will meet for two days to discuss the study progress. Dr. Robiner, the study psychologist, will attend this meeting annually.

A study website will be maintained where all study meetings and phone call minutes will be maintained and where an updated version of the Manual of Operations will be available.

### **15.2. Protocol Deviations, Violations, and Amendments.**

A Protocol Deviation is defined as any change, divergence, or departure from the approved study protocol that does not affect the participant's safety, rights, welfare or the integrity of the study and its resultant data. A Protocol Violation is defined as a protocol deviation that may affect the participant's rights, safety, or wellbeing and/or the completeness, accuracy, and reliability of the study data. Deviation will be reported to the IRB at the time of continuing review whereas violations will be reported as soon as study personnel are aware of the event. The PI will keep an internal protocol deviation and violation log that will be forwarded to the IRB at the time of continuing review. Adoption of protocol amendments will require three-fifths majority approval by members of the Steering Committee. The amended protocol is resubmitted to the IRB.

### **15.3. Financial Disclosure**

On an annual basis or whenever there is a significant change in status, participating investigators will be required to disclose any financial or related interest that could present an actual conflict of interest or could be perceived as presenting a conflict of interest. The Steering Committee will determine 1. if the disclosed interest could directly and significantly affect the performance of study responsibility and, 2. the management, reduction, or elimination of the conflict.

### **15.4. Publications**

It is anticipated that this research may lead to oral and written presentations including one or more jointly-authored publications. The contribution of investigators will be acknowledged in accordance with scientific custom in all published and oral communications concerning this study and its results.

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## **STUDY PROTOCOL**

**'A multicenter clinical trial of allopurinol to prevent GFR loss in type 1 diabetes'**

**Version 10.0**  
**Approved by the DSMB**  
**March 6, 2018**

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**PROTOCOL SUMMARY**

Study Title	A multicenter clinical trial of allopurinol to prevent GFR loss in type 1 diabetes
Study Phase	Phase 3
Objectives	To determine whether lowering serum uric acid by means of allopurinol early in the course of kidney disease may be effective in preventing or slowing the decline of renal function in T1D patients.
Study Design	Multicenter, double-blind, placebo-controlled, parallel-group randomized clinical trial.
Participating Centers	Joslin Diabetes Center (Boston), University of Minnesota (Minneapolis), University of Colorado (Denver), University of Michigan (Ann Arbor), University of Toronto (Toronto), Northwestern University (Chicago), Albert Einstein College of Medicine (New York), Steno Diabetes Center (Copenhagen, Denmark), University of Washington (Seattle), University of Calgary (Calgary), University of Alberta (Edmonton), Emory University (Atlanta), Washington University (St. Louis), University of Texas Southwestern (Dallas), Providence Medical Research Center (Spokane), BC Diabetes (Vancouver).
Subject Population	480 T1D subjects.

Inclusion criteria:

1. Male or female T1D patients.
2. T1D continuously treated with insulin within one year from diagnosis. If the onset was after age 35, the presence of one or more of the following will also be required:
  - documentation of the presence of circulating T1D-associated autoantibodies at diagnosis or at any other time
  - history of hospitalization for DKA
  - plasma C-peptide below the limit of detection with standard assay (with concurrent blood glucose >100 mg/dl)
3. Duration of T1D  $\geq$  8 years.
4. Age 18-70 years.
5. History or presence of microalbuminuria or moderate macroalbuminuria, or evidence of declining kidney function regardless of history or presence of albuminuria and/or RAS Blocker treatment. Micro- or moderate macroalbuminuria will be defined as at least two out of three consecutive urinary albumin excretion rates [AERs] or albumin creatinine ratios [ACRs] taken at any time during the two years before screening or at screening in the 30-5000 mg/24 hr (20-3333  $\mu$ g/min) or 30-5000 mg/g range, respectively, if not on RASB agents, or in the 18-5000 mg/24 hr (12-3333  $\mu$ g/min) or 18-5000 mg/g range, respectively, if on RASB agents); Evidence of declining kidney function will be defined as an eGFR (CKD-EPI) decline  $\geq$ 3.0 ml/min/1.73 m<sup>2</sup>/year, estimated from the slope derived from all the available serum creatinine measurements (including the one at screening assessment) from the previous 3 years. If at least 3 serum creatinine measures are not available in the previous 3 years, then the slope can be derived from creatinine values from the previous 5 years.
6. Estimated GFR (eGFR) based on serum creatinine between 40 and 99.9 ml/min/1.73 m<sup>2</sup> at screening. The upper and the lower limits should be

decreased by 1 ml/min/1.73 m<sup>2</sup> for each year over age 60 (with a lower limit of 35 ml/min/1.73m<sup>2</sup>) and by 10 ml/min/1.73 m<sup>2</sup> for strict vegans.

7. Serum UA (UA) ≥ 4.5 mg/dl at screening.
8. Valid baseline (Visit 4) iGFR measurement.

OR

9. Being an active participant in the PERL Pilot Study.

Exclusion criteria:

1. History of gout or xanthinuria or other indications for uric acid lowering therapy such as cancer chemotherapy.
2. Recurrent renal calculi.
3. Use of urate-lowering agents within 2 months before screening.
4. Current use of azathioprine, 6-mercaptopurine, didanosine, warfarin, tamoxifen, amoxicillin/ampicillin, or other drugs interacting with allopurinol.
5. Known allergy to xanthine-oxidase inhibitors or iodine containing substances.
6. HLA B\*58:01 positivity (tested before randomization).
7. Renal transplant.
8. Non-diabetic kidney disease.
9. SBP>160 or DBP >100 mmHg at screening or SBP>150 or DBP>95 mmHg at the end of the run-in period.
10. Cancer treatment (excluding non-melanoma skin cancer treated by excision) within two years before screening.
11. History of clinically significant hepatic disease including hepatitis B or C and/or persistently elevated serum liver enzymes at screening and/or history of HBV/HCV positivity.
12. History of acquired immune deficiency syndrome or human immunodeficiency virus (HIV) infection.
13. Hemoglobin concentration <11 g/dL (males), <10 g/dL (females) at screening.
14. Platelet count <100,000/mm<sup>3</sup> at screening.
15. History of alcohol or drug abuse in the past 6 months.
16. Blood donation in the 3 months before screening.
17. Breastfeeding or pregnancy or unwillingness to be on contraception throughout the trial.
18. Poor mental function or any other reason to expect patient difficulty in

complying with the requirements of the study.

19. Serious pre-existing medical problems other than diabetes, e.g. congestive heart failure, pulmonary insufficiency.

Study Duration	9-week run-in period, during which RAS inhibition will be introduced and/or standardized, if indicated, and BP normalized, if elevated above 140/90 mmHg, followed by a 3-year treatment period and then by a 2-month wash-out period.
Study Treatment, Dosage, and Route of Administration	After the run-in period, eligible subjects will be randomized in a 1 to 1 ratio to receive placebo or oral allopurinol at a dose of 100 mg per day for 4 weeks and then at a dose ranging from 200 to 400 mg per day depending on kidney function.
Efficacy Assessments	<p><u>Primary outcome measure:</u> GFR at the end of the 2-month wash-out period following the 3-year treatment period, measured by the plasma clearance of non-radioactive iohexol (iGFR) and adjusted for the iGFR at baseline.</p> <p><u>Secondary outcome measures:</u> 1. iGFR the end of the 3-yr treatment period (before the washout period) adjusted for the iGFR at baseline. 2. iGFR time trajectory estimated from periodical iGFR measurements. 3. eGFR at 4 months estimated from serum creatinine and cystatin C and adjusted for the eGFR at baseline. 4. eGFR time trajectory estimated from quarterly serum creatinine and cystatin C measurements (eGFR). 5. Time to serum creatinine doubling or end stage renal disease (ESRD). 6. AER at the end of the 2-month wash-out following the 3-yr treatment period, adjusted for the AER at baseline. 7. AER at the end of the 3-yr treatment period, adjusted for the AER at baseline. 8. Time to fatal or non-fatal cardiovascular events.</p>
Safety Assessment	Examination for skin rash, measurements of liver enzymes, serum creatinine, and CBC, carried out 1 month after randomization and every 3-4 months thereafter.
Statistical Methods	The majority of data analyses, including the primary analysis, will be performed according to an intention-to-treat approach. Differences between treatment arms in the primary outcome will be tested for significance by means of a linear model with correlated errors. Intervention effects on other secondary outcomes will be tested by mixed-effect models (GFR time trajectory), ANCOVA (AER), and survival analysis (time to serum creatinine doubling/ESRD and CVD events).
Date of protocol	February 22, 2018



**ABBREVIATIONS**

**AE:** Adverse Event

**ACE:** Angiotensin Converting Enzyme

**ACR:** Albumin Creatinine Ratio

**AER:** Albumin Excretion Rate

**ALT:** Alanine Transaminase

**ARB:** Angiotensin Receptor Blocker

**CBC:** Complete Blood Count

**CKD:** Chronic Kidney Disease

**CRF:** Case Report Form

**CVD:** Cardiovascular Disease

**DBP:** Diastolic Blood Pressure

**DCC:** Data Coordinating Center

**DMC:** Drug Monitoring Committee

**DN:** Diabetic Nephropathy

**DSMB:** Data and Safety Monitoring Board

**ESRD:** End Stage Renal Disease

**GFR:** Glomerular Filtration Rate

**ITT:** Intention to Treat

**HbA1c:** Glycated Hemoglobin A1C

**HBV:** Hepatitis B Virus

**HCV:** Hepatitis C Virus

**HIV:** Human Immunodeficiency Virus

**IRB:** Institutional Review Board

**MAP:** Mean Arterial Pressure

**NO:** Nitric Oxide

**PERL:** Preventing Early Renal Function Loss in Diabetes Consortium

**RAS:** Renin Angiotensin System

**RASB:** Renin Angiotensin System Blocker

**SAE:** Severe Adverse Event

**SBP:** Systolic Blood Pressure

**SC:** Steering Committee

**SOP:** Standard Operating Procedure

**T1D:** Type 1 Diabetes

**UA:** Uric Acid

## 1. INTRODUCTION

Diabetic nephropathy (DN) is the long-term complication of T1D that imposes the highest social and economic burden. After 40 years of diabetes, about one in three patients with T1D has developed kidney abnormalities, which frequently progress to end stage renal disease (ESRD).<sup>1</sup> Despite improvements during the past 20 years in glycemic and blood pressure control, and the introduction of 'renoprotective' drugs such as renin-angiotensin system (RAS) blockers, the overall incidence of DN is not declining.<sup>2-4</sup> Thus, DN remains one of the most important causes of excess morbidity and mortality in patients with diabetes mellitus, and novel therapies to complement and increase the therapeutic effects of glycemic control and RAS inhibition are urgently needed.

DN has been traditionally viewed as a multi-stage process, in which an initial clinical phase characterized by increased urinary excretion of small amounts of albumin (microalbuminuria) is followed by excretion of larger amounts of proteins (overt proteinuria), which then ushers in progressive decline in renal function ultimately leading to end-stage renal disease (ESRD).<sup>1</sup> This paradigm, however, is changing with the demonstration in prospective studies that, in a substantial proportion of T1D patients, renal function starts to decline before the onset of overt proteinuria.<sup>5-7</sup> These findings indicate that T1D patients should be screened for GFR loss when albumin excretion rate (AER) is still in the microalbuminuria range, and that interventions aimed at preventing ESRD should be started at these earlier stages. The earlier the rate of GFR loss is reduced through appropriate interventions, the longer will be the delay of ESRD.

Mounting evidence from epidemiological studies indicates that serum UA levels are strong risk factors for the development of chronic kidney disease and loss of kidney function among persons with T1D. Prospective data from the Second Joslin Kidney Study (JKS) identified elevated baseline serum UA as one of the strongest independent predictors of early GFR loss among T1D persons with microalbuminuria and normal renal function at baseline.<sup>8</sup> The unadjusted odds ratio of developing increased GFR loss was 1.5 (95% CI 1.3-1.9, p=0.0002) for each mg/dl increase in serum UA. This translates into a ~2.4-fold increase in the risk of early GFR loss for UA levels  $\geq$  4.5 mg/dl as compared to UA levels <4.5 mg/dl. The magnitude of this effect did not significantly change after adjustment for urinary AER, gender, HbA1c, or, importantly, baseline GFR. The U. of Colorado group also found that serum UA predicted the transition from normoalbuminuria to micro- or macro-albuminuria as well as the progression of subclinical atherosclerosis in the CACTI study.<sup>9,10</sup> As in the JKS, the effect of UA was not influenced by adjustment for other baseline variables. An association between UA and development of persistent macroalbuminuria has also been reported by the Steno group. It is very important to note that, in that study, the UA levels shortly after the onset of T1D was a significant independent predictor of macroalbuminuria 18 years later (hazard ratio 1.90 per mg/dl increase in UA level; p=0.04)<sup>11</sup>, this being suggestive of a pathogenetic role.

The prospective nature of these findings and their robustness after adjustment for potential confounders strongly support the concept that moderately elevated serum UA has a pathogenetic role in DN development and in the deterioration of kidney function observed in T1D. Consistent with this hypothesis, hyperuricemia has predicted chronic renal failure in population-based studies<sup>12-14</sup> and mild UA elevation has been shown to cause renal disease in animal models.<sup>15,16</sup> Alterations of nitric oxide (NO) pathways and induction of pro-inflammatory cytokines<sup>17,18</sup>, and increased oxidative stress resulting from the generation of UA by xanthine oxidase<sup>19,20</sup> have been proposed as being responsible for these effects. Two small clinical trials have recently provided proof of concept data for translating these findings into a novel intervention by showing that the urate-lowering agent allopurinol was effective in slowing the progression of non-diabetic renal disease among hyperuricemic as well as normouricemic individuals with moderately reduced GFR.<sup>21,22</sup> A beneficial effect of urate-lowering drugs on the progression of kidney disease has also been observed in animal models.<sup>23</sup> These findings, along with the observational data discussed above, strongly suggest that **lowering serum UA levels may prevent or slow the loss of kidney function among diabetic subjects.**

To test this hypothesis, we have established a consortium of investigators from academic centers where large rosters of T1D patients are available along with long-standing expertise in the study of diabetic complications, especially DN, and in DN clinical trials. Included in this initiative are the Joslin Diabetes Center, the Universities of Minnesota, Colorado, Toronto, Michigan, Washington (Seattle), Texas Southwestern, Calgary, and Alberta Northwestern University, Washington University (St. Louis), Emory University, Albert Einstein College of Medicine, BC Diabetes, Providence Medical Research Center, and Steno Diabetes Center in Copenhagen, Denmark. The Consortium, led by Dr. Alessandro Doria from the Joslin Kidney Study, and by Dr. Michael Mauer, who recently led the Renin Angiotensin System Study (RASS) clinical trial, has been named PERL (**P**reventing **E**arly **R**enal Function **L**oss in Diabetes) to emphasize the Consortium's focus on intervening early in the course of kidney disease, when renal damage is most likely to be able to be arrested or reversed and interventions are more likely to be effective.

PERL has designed the present 3-year clinical trial to test whether the uric acid lowering drug allopurinol can preserve kidney function among type 1 diabetic patients. In preparation for this trial, the Consortium has been conducting a pilot study to determine the study feasibility and establish study procedures. Funded by JDRF (JDRF file # 17-2012-377), the pilot study has a comparable design as the pivotal trial, but a smaller size and shorter duration. As of February 1, 2014 a total of 31 pilot study subjects have been randomized to allopurinol or placebo. Upon activation of the present trial, pilot participants will be re-consented and rolled-over to the present study at a time point corresponding to their next scheduled visit. The first 7 visits have identical timing in the two protocols. Visit schedules slightly differ after that, but given the current follow-up status, it will be possible to transfer all pilot participants to the pivotal trial before the timing diverges.

## 2. STUDY OBJECTIVE

To determine whether lowering serum UA by means of oral allopurinol is effective in preventing or slowing decline of renal function in T1D patients with microalbuminuria or moderate macroalbuminuria who still have only mildly or moderately impaired kidney function.

## 3. STUDY DESIGN

The study will be a multi-center, double-blind, placebo-controlled, parallel-group randomized clinical trial including a total of 480 patients with type 1 diabetes (T1D) who are at high risk for GFR loss because of increased albuminuria and a relatively high serum UA ( $\geq 4.5$  mg/dl), but have only mildly or moderately decreased renal function.

## 4. PARTICIPATING CENTERS

The study will involve 16 centers that are part of the PERL Consortium:

- Joslin Diabetes Center (Boston)
- University of Minnesota (Minneapolis)
- University of Colorado (Barbara Davis Center for Childhood Diabetes, Denver)
- University of Michigan (Ann Arbor)
- University of Toronto (Toronto)
- Northwestern University (Chicago)
- Albert Einstein College of Medicine (New York)

- Steno Diabetes Center (Copenhagen, Denmark)
- Washington University (St. Louis, MO)
- University of Calgary (Calgary, Alberta, Canada)
- University of Alberta (Edmonton, Alberta, Canada)
- Emory University (Atlanta)
- University of Washington (Seattle)
- University of Texas Southwestern (Dallas)
- Providence Medical Research Center (Spokane)
- BC Diabetes (Vancouver)

#### 4.1 Location of study visits

Study visits will be generally conducted at the Study Sites or their Satellites (hereby referred to as "In-Person Visits"). However, if a participant lives far from a study site or satellite, or travel impediments are present, visits V1, V3, V5-10, and V12-15 may be conducted remotely (Visit 2 and all the visits including an iohexol-GFR measurement, i.e., V4, V11, V16, V17, will always be done "In-person"). In the case of remote visits, study procedures that do not require physical interactions (e.g., collection of medical history, compliance issues) will be carried out over the phone or other media such as Skype (hereby referred to as "Phone Visits"). Blood draws and urine collections scheduled at the time of Phone Visits will be performed at local facilities close to where participants live (hereby referred to as "Remote Biospecimen Collections"). For any given study visit to be conducted remotely, a Phone Visit and a Remote Biospecimen Collection will be both required. Phone Visits and Remote Biospecimen Collections will be conducted according to the following protocol:

##### Phone Visits

- Phone Visits will be scheduled based on the same calendar and time windows used for In-Person Visits (see paragraph 8.1 and Fig 1).
- All Phone Visits will be carried out by the same trained study personnel performing the In-Person Visits according to the same standards as those in place for In-Person Visits.
- If Visit 1 is a Phone Visit, a copy of the informed consent form (ICF) will be mailed, faxed, or sent electronically to the study subject before the visit. After reviewing the ICF content with the study personnel over the phone/Skype, subjects who agree to participate in the study will be invited to mail, fax, or send electronically a signed copy of the ICF back to the study site. Phone Visit 1 and any other study activity will take place only after the signed ICF has been received by the study site.
- Study procedures that may be carried out during Phone Visits include:
  - Collection of demographic data.
  - Collection of medical history.
  - Collection of family history.
  - Review of concomitant medications.
  - Evaluation of eligibility.
  - Randomization.
  - Review of RASB medication and BP control.

- Study drug prescription and instructions.
  - Review of study drug compliance.
  - Review of adverse events.
  - Any other study procedure that can be carried out by talking on the phone.
- Study procedures will be carried out according to the same protocol as the corresponding In-Person Visits and as described in the Manual of Operations.
  - All study material that would be provided to participants at In-Person Visits (e.g., urine collection instructions, urine containers, study drug instruction, BP monitoring logs) will be mailed, faxed, or sent electronically to participants right after the Phone Visit. In addition, specific instructions will be provided for presentation to the local lab for specimen collection, handling and tube labeling for specimens requiring shipment to the Study Site or Central lab. Pre-addressed shipping containers will also be provided.
  - Following a Phone Visit, participants may be invited to an In-Person Visit at the Study Site, at their PCP's office, or at other local healthcare facilities if procedures that require physical interactions are deemed to be necessary (e.g., BP measurement to confirm the self-report of elevated BP values, physical exam to confirm the self report of skin rash). Sites for remote in-person visits will be chosen by the Study Site based on the participant's preference, logistic and financial considerations, and site's qualifications. Study personnel will discuss study requirements with the remote site health providers and operators and will provided with written instructions on how to carry out the procedures that will be conducted at their locations and report the results to the Study Site.

#### Remote Biospecimen Collections

- Local sites for Remote Biospecimen Collections will be chosen by the Study Site based on the participant's preference, logistic and financial considerations, and site's qualifications.
- Specific instructions will be provided for presentation to the local sites for collection, handling and tube labeling for specimens requiring shipment to the Study Site or Central Laboratory. Pre-addressed shipping containers will also be provided along with an inventory sheet for faxing to the Study Site or Central Laboratory and inclusion with the shipment.
- Blood samples for local lab tests (serum creatinine, K, and ALT, CBC, pregnancy tests) will be processed and analyzed at the facilities where samples are collected or shipped to commercial laboratories or to the Central Laboratory for testing. Results will be transmitted to the Study Site by fax or other secure methods.
- Blood and urine samples for central lab tests (serum creatinine, Cystatin C, uric acid, HbA1c, urinary ACR and AER) will be mailed to the Central Lab or to the Study Site where they will be processed, aliquoted, and forwarded to the Central Lab. Blood tubes and urine containers will be provided by the Study Site.

## **5. SUBJECT SELECTION**

### **5.1. Inclusion Criteria**

1. Male or female T1D patients between 18 and 70 years of age, inclusive.
2. T1D continuously treated with insulin within one year from diagnosis. If the onset was after age 35, documentation of the presence of one or more of the following will also be required:

- a. documentation of the presence of circulating T1D-associated autoantibodies at diagnosis or at any other time
  - b. history of hospitalization for DKA
  - c. plasma C-peptide below the limit of detection with standard assay (with concurrent blood glucose >100 mg/dl)
3. Duration of T1D  $\geq$  8 years;
  4. History or presence of microalbuminuria or moderate macroalbuminuria, or evidence of declining kidney function regardless of history or presence of albuminuria and/or RAS Blockage. Micro- or moderate macroalbuminuria will be defined as at least two out of three consecutive urinary albumin excretion rates [AERs] or albumin creatinine ratios [ACRs] taken during the two years before screening or at screening in the 30-5000 mg/24 hr (20-3333  $\mu$ g/min) or 30-5000 mg/g range, respectively, if not on RASB agents, or in the 18-5000 mg/24 hr (12-3333  $\mu$ g/min) or 18-5000 mg/g range, respectively, if on RASB. Evidence of declining kidney function will be defined as an eGFR (CKD-EPI) decline  $\geq$ 3.0 ml/min/1.73 m<sup>2</sup>/year, estimated from all the available creatinine measurements (including the one at screening assessment) from the previous 3 years. If at least 3 serum creatinine measures are not available in the previous 3 years, then the slope can be derived from creatinine values from the previous 5 years.
  5. Estimated GFR (eGFR) based on serum creatinine between 40 and 99.9 ml/min/1.73 m<sup>2</sup> at screening. The upper and the lower limits should be decreased by 1 ml/min/1.73 m<sup>2</sup> for each year over age 60 (with a lower limit of 35 ml/min/1.73m<sup>2</sup>) and by 10 ml/min/1.73 m<sup>2</sup> for strict vegans.
  6. Serum UA  $\geq$  4.5 mg/dl at the screening visit.
  7. Willing to comply with schedule of events and protocol requirements, including written informed consent.
  8. Valid baseline (Visit 4) iohexol GFR measurement prior to randomization.
- OR
9. Being an active participant in the PERL Pilot Study.

## 5.2. Exclusion Criteria

1. History of gout requiring allopurinol therapy or xanthinuria or other indications for uric acid lowering therapy such as cancer chemotherapy or extremely high serum uric acid values (>12 mg/dl).
2. Recurrent renal calculi (history of more than one episode).
3. Use of urate-lowering agents within 2 months before screening.
4. Current use of azathioprine, 6-mercaptopurine, didanosine, warfarin, tamoxifen, amoxicillin/ampicillin, or other drugs interacting with allopurinol.
5. Known allergy to xanthine-oxidase inhibitors or iodine containing substances.
6. HLA B\*58:01 genotype (determined prior to randomization) indicating increased risk of Stevens-Johnson syndrome in response to allopurinol.
7. Renal transplant.
8. Non-diabetic kidney disease as indicated by medical history and/or laboratory findings.
9. SBP>160 or DBP >100 mmHg at screening or SBP>150 or DBP>95 mmHg at the end of the run-in period.

10. Cancer treatment (excluding non-melanoma skin cancer treated by excision) within two years before screening.
11. History of clinically significant hepatic disease including hepatitis B or C and/or ALT (SGPT) >2.50 x ULN at screening and/or history of HBV/HCV antibody positivity.
12. History of acquired immune deficiency syndrome or human immunodeficiency virus (HIV) infection.
13. Hemoglobin concentration <11 g/dL (males), <10 g/dL (females) at screening.
14. Platelet count <100,000/mm<sup>3</sup> at screening.
15. Ongoing alcohol or drug abuse or history of treatment for these conditions in the past 6 months.
16. Blood donation in the 3 months before screening (subjects become eligible once 3 months have elapsed since the last donation).
17. Breastfeeding or pregnancy or unwillingness to be on contraception if still fertile.
18. Poor mental function or any other reason to expect patient difficulty in complying with the requirements of the study.
19. Serious pre-existing medical problems other than diabetes, e.g. congestive heart failure, pulmonary insufficiency.

### 5.3. Prohibited Medications and Restrictions

- Allopurinol and other urate lowering agents (e.g., probenecid, rasburicase rys) for the treatment of gout. Patients treated with uric acid lowering agents for elevated uric acid levels with no history of gout can, with the agreement of their treating physician, undergo a 2 month washout of uric acid lowering medication and then be tested to determine if uric acid entry criteria are met.
- Herbal supplements that may have urate lowering actions (e.g., Devil's Claw or Harpagophytum procumbens, Indigenous cinnamon or Cinnamomum osmophloeum, Skunkvine or Paederia scandens or Paederia foetida)
- Azathioprine
- 6-Mercaptopurine
- Didanosine
- Warfarin
- Tamoxifen
- Amoxicillin/ampicillin
- Any other drug for which there is evidence of interaction with allopurinol
- Dual RASB therapy (i.e., another RASB medication in addition to that already in use)
- Non-RASB antihypertensives that are not listed in the PERL approved menu of antihypertensive drugs, unless these were in use before joining the study.

### 5.4. Randomization Procedures

After the run-in period (described in Section 8.3) and with a valid baseline iohexol GFR measurement prior to randomization, participants will be randomized in a 1 to 1 ratio to receive either oral allopurinol or placebo. Randomization will be stratified by center, uric acid ( $\leq 6.0$  vs.  $>6.0$  mg/dl), and HbA1c ( $\leq 7.8$  vs.  $>7.8\%$ ). Randomization will be performed using permuted blocks, with a block size that is known only to the DCC. After a participant has been randomized, the clinical site will send a study medication request to the research pharmacy, including the participant's address, so that the study medication can be directly mailed to the participant. Clinical sites will not have access to the treatment



assignment (see 6.2., Blinding Procedures). This will be directly communicated or made electronically available to the pharmacy by the DCC.

## 5.5. Discontinuation of study drug

### 5.5.1. Reasons for discontinuation

The study drug will be *temporarily* discontinued if a participant:

- Has clinically significant persistent changes from baseline based on laboratory safety assessment results (the response to discontinuation will be monitored to assess whether the drug can be re-instituted, see next paragraph on permanent discontinuations).
- Requires treatment with allopurinol or medications that make allopurinol contraindicated (see 5.5.2 and 9.5).
- Becomes pregnant or breastfeeding (see 5.5.2)

Whenever the reason for temporary discontinuation of the study drug ceases to exist, the study medication will be resumed with the consensus of the drug monitoring committee, according to the following procedures:

- If the study medication was discontinued because of a suspected drug reaction or the participant was off-medication for 3 months or longer, the study drug will be re-started at a dosage of 100 mg for 4 weeks, which will then be increased to the full dosage appropriate for the eGFR. (see 6.1.2)
- If the study medication was not discontinued because of a drug reaction and the participant was off-medication for less than 3 months, the study medication will be re-started, at the full dosage appropriate for the eGFR.

The study drug will be *permanently* discontinued if a participant:

- Experiences an SAE related to the study drug or an intolerable AE such as a persistent allergy or rash.
- Has clinically significant persistent changes from baseline based on laboratory safety assessment results which do not respond to temporary 2-week discontinuation of study drug and re-institution of drug at ½ of the initial dose.
- Develops end-stage renal disease (confirmed eGFR  $\leq 15$  ml/min/1.73 m<sup>2</sup> in the absence of acute kidney injury [AKI], institution of chronic dialysis treatment or kidney transplantation) or iGFR decreases by 50% from one measurement to the next or serum creatinine levels double over any 12 month interval in the post-randomization period. If any of these renal function changes prove to be temporary, the study medication could be resumed as described above with the consensus of the drug monitoring committee.

### 5.5.2. Handling of study drug discontinuation

- Date and reason for drug discontinuation will be recorded on the relevant Case Report Form.
- All study discontinuations decided by a clinical site will have to be reviewed and approved by the Drug Monitoring Committee within 10 days from their start.
- If the study drug is discontinued due to treatment with medications that make allopurinol contraindicated (e.g. amoxicillin/ampicillin) or due to pregnancy/breastfeeding, the possibility of resuming the study drug will be evaluated by the Drug Monitoring Committee once those medications have been discontinued or pregnancy/breastfeeding has ended.

- If the study drug is temporarily discontinued and then re-instated, the end-date of the intervention will remain the same as if the study drug had not been discontinued. All visits will be carried out as scheduled while the study drug is temporarily discontinued.
- Unless a participant withdraws consent all participants that are permanently discontinued from study drug or who discontinue study medication on their own will be followed for the full study period (i.e., 164 weeks, including the washout period) and all data will be collected as scheduled.
- If a participant reaches ESRD as defined above under 5.5.1., he/she will be permanently discontinued from the study and invited to participate in a study close-out call or visit to be held within three months from the occurrence of ESRD. Data collection at this call or visit will be limited to standard adverse event reporting. In addition, sites should continue to contact participants who have reached end-stage renal disease to determine their final status until 3 years and 2 months after randomization. Major attempts will be made to schedule an end-of-study assessment for all participants who are lost to follow-up during the course of the study.

### 5.5.3. Replacements

Participants that withdraw consent from the study during the Run-in period (i.e., before randomization) or do not qualify for study continuation at the end of the Run-in period will be replaced until the target number of randomized study participants is reached. Participants that withdraw consent from the study or discontinue the study drug after randomization will not be replaced.

### 5.5.4. Termination of Study

Premature termination of this clinical trial may occur because of a regulatory authority decision, drug safety problems as determined by the Data Safety Monitoring Board (DSMB), or at the discretion of the funding agency (NIDDK).

## 6. STUDY TREATMENTS

### 6.1. Study Drug Description, Dosage, Administration, and Accountability

#### 6.1.1. Description

Eligible study subjects who agree to participate in the study will all be randomized to receive placebo or allopurinol – a serum UA lowering medication that has been on the market since 1964 as the main drug for the therapy of symptomatic hyperuricemia and for the prophylaxis of gout in cancer patients receiving chemotherapy. Allopurinol is an inhibitor of xanthine oxidase, which is responsible for the conversion of hypoxanthine to xanthine and of xanthine to UA. It is metabolized to the corresponding xanthine analogue, oxypurinol (alloxanthine), which is also an inhibitor of xanthine oxidase. At the average dosage (300 mg/day), allopurinol causes a 30-40% reduction in serum UA<sup>24-26</sup>, but up to a 60% reduction can be obtained using the maximum dosage of 600 mg.<sup>27</sup> While allopurinol is mostly used in individuals with gout and very high UA levels, several studies have shown that it is also effective at lower UA levels<sup>27-29</sup>.

Because of its rapid oxidation to its active metabolite oxypurinol, allopurinol has a short plasma half-life (~1-2 hrs). However, since oxypurinol has a longer half-life (~15 hrs), effective xanthine oxidase inhibition can be maintained over 24 hrs with a single daily dose of allopurinol. Since both allopurinol and oxypurinol are eliminated through the kidneys, patients with impaired renal function require lower doses than those with normal renal function. A common rule of thumb is to use 75% of the dosage in individuals with eGFR in the 50-90 ml/min range, and 50% of the dosage in individuals in the 10-50 ml/min range.

### 6.1.2. Dosage

After an initial four weeks where all participants randomized to allopurinol will take 100 mg per day, the allopurinol dosage will vary from 200 to 400 mg per day based on eGFR levels. Participants will take 400 mg per day if their eGFR is  $\geq 50$  ml/min/1.73 m<sup>2</sup>, 300 mg per day if their eGFR is in the 25 to  $< 50$  ml/min/1.73 m<sup>2</sup> range, and 200 mg per day if the eGFR is in the 15 to  $< 25$  ml/min/1.73 m<sup>2</sup> range. Allopurinol will be continued at this dosage throughout the study unless the eGFR changes, in which case the dosage will be modified to that appropriate for the new eGFR class.

All participants, whether they are randomized to allopurinol or placebo, will be given four tablets per day to be taken orally following breakfast. Tablets will be provided in four vials (A, B, C, and D) or in blister packs, in which each blister contains the four tablets for a given day. If the medication is provided in bottles, participants randomized to allopurinol will receive a dosage of 100 mg as a 100 mg tablet (from vial A) plus three placebo tablets (from vials B, C, D), 200 mg as two 100 mg (from vials A and C) and two placebo tablets (from vials B and D), 300 mg as three 100 mg (from vials A, B, C) and one placebo tablet (from vial D), 400 mg as four 100 mg tablets (from vials A, B, C, D). Subjects randomized to placebo will be given four placebo tablets (from vials A, B, C, D). If the medication is provided in blister packs, each blister will contain the four tablets for a given day, with the same proportion of active and placebo tablets described above for each allopurinol dosage and for placebo.

The dose adjustment will be carried out as follows:

1. At each follow-up visit, a study drug requisition will be sent by the clinical site to the research pharmacy indicating the study ID, name, and address of the participant, the most recent eGFR value (CKD-EPI), calculated using a recent local lab creatinine value, and the number of days to be covered by the drug supply.
2. At the pharmacy, a clinical pharmacist will determine the allopurinol dose (ranging from 0 to 400 mg) that should be given at that time according to the study protocol given the participant's treatment assignment and the most recent eGFR value (CKD-EPI) calculated using a recent local lab serum creatinine value.
3. The research pharmacy will mail the new batch of study medication directly to the study participant.
4. At some sites the study medication may be dispensed directly to the study participant at a relevant in person study visit or by mail from the site following a relevant in-person or phone study visit.
5. Participants will be instructed to immediately inform the clinical site upon receipt of the new tablets and mail the pill bottles or blister packs with the tablets remaining from the previous prescription in a provided pre-addressed mailer, to the clinical site for drug accounting and compliance assessment.

### 6.1.3. Compliance and accountability

Skills will be taught and reinforced at each visit with regard to scheduling and administration of pills at home and while traveling. Methods (e.g. record-keeping) will be taught to help participants monitor tablet usage and enhance compliance. To complement the regular compliance interventions at the scheduled visits, study information and motivational materials (postcards, newsletters, etc.) will be mailed. In addition, at midpoint between clinic visits, participants will be phoned by the clinic staff to review pill-taking. Patients will be provided with random but known numbers of excess medications, providing extras in case of pill loss. Adherence will be monitored by instructing participants to expect extra pills and to mail the pill bottles or the blister packs with the tablets remaining from the previous prescription to the study center upon receipt of a new batch of tablets. The number of extra pills included in each supply of medications will be decided by the pharmacist, who will keep a record of it and will transmit this information to the Study Site. Personnel at the Study Site will enter this

information in the appropriate electronic Case Report Form along with the expected number of pills used during the period covered by the supply and the number of unused pills returned by the participant. These data will be used to analyze compliance. If poor adherence is noticed, measures will be taken to increase compliance, such as explaining the purpose of the study again, providing pill reminders, and more frequently contacting the study subject by phone. Participants at each visit will be asked about their perceived compliance and about any difficulties with taking the study medications, but the individualized strategies to improve compliance will not be openly linked to the pill counts, i.e. participants will not be informed of the results of pill counting. Participants showing poor compliance will not be withdrawn from the study.

## 6.2. Blinding Procedures

Study participants, the investigators and research staff at the Clinical Sites, and the PERL co-PIs (Drs. Doria and Mauer) will be blinded to treatment assignment whereas the Data Coordinating Center Co-Directors and staff and the pharmacy personnel will have access to this information. Serum uric acid values, from which the treatment assignment might be inferred, will not be transmitted to the Clinical Sites by the central or local laboratory and will not be available for viewing in the study database. Should unblinding of a study participant be necessary because of an emergency, the site personnel will login to the password-protected electronic database application that will provide the treatment assignment. Audit procedures will ensure that the name of the individual associated with the login will be communicated to the Data Coordinating Center project manager and Co-Directors. As an additional safety measure, the personnel at the Clinical Sites will be provided with telephone numbers to contact the Data Coordinating Center and/or Pharmacy personnel having access to the treatment assignment on a 24-7 basis. If unblinding occurs, the circumstances that led to it will be reviewed and reported.

## 7. STUDY OUTCOMES

### 7.1. Primary outcome

The primary outcome will be the iGFR at the end of the 2-month wash-out period following the 3-year treatment period, measured by the plasma clearance of non-radioactive iohexol (iGFR) and adjusted for the iGFR at baseline. The rationale of measuring the primary outcome at the end of the wash-out period is to test allopurinol for permanent effects of on the natural history of kidney disease, independent from any transient, hemodynamic effect that the medication may have on GFR. Plasma iohexol clearance has been shown to provide accurate and reproducible GFR measurements.<sup>30,31</sup> It is highly correlated with inulin clearance (the gold standard to measuring GFR)<sup>32</sup> and is a safe, cost-effective method to test hundreds of patients enrolled in multicenter clinical trials.<sup>33</sup> The method consists of injecting a 5 mL bolus of Iohexol (Omnipaque, 300 mg iodine/mL) and drawing blood samples at baseline and 120, 150, 180, 210, and 240 minutes after the injection. Plasma concentrations of iohexol at different time points are measured by HPLC and used to calculate the plasma clearance of iohexol ( $Cl = \text{Dose}/\text{AUC}$ , where AUC is the area under the plasma concentration time curve), which is taken after appropriate body surface area corrections as a measure of GFR.<sup>30,31</sup>

#### 7.1.1. iGFR quality assurance

It is of the foremost importance that reliable iGFR measurements are obtained. To maximize accuracy and precision, the following procedure will be in place.

1. Personnel performing the iohexol clearance test will undergo a standardized training program administered under the Site Directors' supervision through in-person meetings or on-line modules. All clinical site staff will complete the online knowledge testing in order to perform the tests.
2. Participants will be instructed to discontinue non-steroidal anti-inflammatory drugs (NSAIDs) for at least 3 days and avoid large protein meals for one day prior to the test, since these

could influence GFR. They will also be instructed to aim for a fasting glycemia between 90 and 160 mg/dl on the day of the test. Before the test, participants will have a light breakfast at the clinic along with their morning insulin. The insulin dose will be adjusted to keep their blood glucose in the 90-160 mg/dl range. If blood glucose is outside this range right before or during the test, small amounts of intravenous insulin (if blood glucose is too high) or orange juice/milk (if glucose is too low) may be administered to bring blood glucose levels within the desired interval.

3. In the case of extreme deviations from the target blood glucose values, the test may be rescheduled to another day (within 2 weeks). The test will also be postponed in the case of recent febrile illness, diarrhea or vomiting, dehydration, poor fluid intake, recent intake of nephrotoxic drugs such as NSAIDs, urinary tract infection, or a positive pregnancy test.

### 7.1.2. iGFR quality control

The quality of iGFR results will be monitored by:

1. Systematically checking for deviations from the study protocol, such as deviations from the target blood draws time points during the test or the presence of medical conditions that should have prompted a postponement of the test.
2. Calculating the R-square ( $R^2$ ) of the regression between log-iohexol values and time. iGFR tests will be defined as technically acceptable if the  $R^2$  is  $>0.90$ .  $R^2$  calculations will be performed by the DCC using all 5 time points of the iGFR test.

### 7.1.3. Technically unacceptable iGFR measures

If an iGFR test is deemed to be technically unacceptable according to the above QC criterion ( $R^2 \leq 0.90$ ), or a study protocol deviation is suspected, the following procedures will be followed:

1. Source documents related to the test in question will be reviewed to verify whether there was a protocol deviation or a technical error in the iGFR procedure (e.g., presence of contraindications to iGFR, swapping of tubes, wrong collection times, typos, etc.). In the case of an  $R^2 \leq 0.90$ , the iohexol measurements will be repeated by the central laboratory.
2. If a technical error is found and the error can be rectified, or the new laboratory measures yield an  $R^2 > 0.90$ , the iGFR value will be recalculated after the appropriate corrections are made. The study site or the central laboratory, as applicable, will be alerted about the error and measures aimed at improving iGFR quality will be implemented.
3. If the error is confirmed and cannot be fixed, or no error can be found, the iGFR will be dropped and will be repeated within 4 weeks from when the iGFR results become available.
4. If the repeated test is technically unacceptable, or the test cannot be repeated within 4 weeks for logistical reasons, the iGFR value at that time point will be considered as missing for the analysis of the primary outcome. It is therefore critical that every effort be made to obtain this repeat iGFR measure.

## 7.2. Secondary outcomes

1. Iohexol-clearance GFR at the end of the 3-year treatment period (before the washout).
2. Iohexol-clearance GFR time trajectory estimated from periodical iohexol-GFR measurements.
3. Estimated (eGFR) at 4 months estimated from serum creatinine and cystatin C and adjusted for the eGFR at baseline.
4. Estimated GFR (eGFR) time trajectory estimated from quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations.<sup>34,35</sup>
5. Time to doubling of baseline serum creatinine value or ESRD (eGFR  $\leq 15$  ml/min/1.73 m<sup>2</sup>, institution of dialysis, kidney transplantation).
6. Geometric mean of two AER measurements at the end of the 2-month wash-out period following the 3-year treatment period, adjusted for the mean urinary AER at baseline. Urinary

AER will be determined in timed overnight urine collections brought by study participants to regular clinic visits, and expressed in  $\mu\text{g}/\text{minute}$  and as urinary albumin/creatinine ratios.

7. Geometric mean of urinary AER during the last three months of the treatment period (Visits 15 and 16), adjusted for the mean urinary AER at baseline.
8. Time to fatal or non-fatal cardiovascular events, defined as the composite of CVD death (ICD-10 code I10 to I74.9), myocardial infarction, stroke (ischemic or hemorrhagic), coronary artery bypass grafting, or percutaneous coronary intervention.

## **8. STUDY PROCEDURES**

### **8.1. Schedule of Events**

The schedule of events that will take place in the proposed study is outlined in Figure 1. Visits will be frequent during the Run-In period and during the first 30 days after randomization in order to escalate the allopurinol dosage and closely monitor the occurrence of AEs. After that, participants will be seen every 3-4 months to monitor their UA levels, renal function, occurrence of AEs, and medication compliance and, if necessary, to perform interventions to improve compliance. Visit 1 will be considered as Time 0 for scheduling Visits 2-5, Visit 5 will be considered as Time 0 for scheduling Visit 6-16, Visit 16 as Time 0 for scheduling Visit 17. The study windows that define when study visits may occur are noted in Figure 1 and differ by type of visit. Visits 2, 3, and 6 will be carried out within 6 business days (before or after) from their scheduled dates; visits 11, 16, and 17 within 2 weeks before and 4 weeks after their scheduled dates; visit 4A (if necessary) within 1 week before and 3 weeks after its scheduled date, and all other visits within 2 weeks (before or after) from their schedule dates. Additional blood or urine samples may be required in between visits if clinically significant changes are observed in blood or urine measurements that need to be confirmed or otherwise monitored. iGFR measurements may be repeated for medical reasons or technical problems (see 7.1). Safety laboratory tests (CBC, serum creatinine,  $\text{K}^+$ , ALT, and pregnancy tests in women) will be performed by local laboratories. Outcome variables (plasma iohexol, serum creatinine and cystatin C, urinary AER), HbA1c, and serum uric acid will be measured by the Central Laboratory at the University of Minnesota, directed by Dr. Amy Karger.

**Figure 1. Schedule of Events**

Year	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	3	3	3
Week	-12	-9	-7	-3	0	4	16	32	48	64	80	96	112	128	142	156	164	
Visit #	1	2	3	4	4a*	5	6	7	8	9	10	11	12	13	14	15	16	17
^Type of Visit: In-Person Visit Required (V); Phone Call (C); Other Visit (In-Person or Remote Visit, O)	O	V	O	V	V	C	O	O	O	O	O	V	O	O	O	O	V	V
EVENT	Screen	Run-in				RANDO 100 mg	Allopurinol or placebo										Wash-out EOS	
		200-400 mg																
Informed Consent	x	x																
Demographics	x																	
Initial Medical Hx		x																
Interval Medical Hx and BP Control			x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Concomitant Meds	x	x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Blood Pressure and Measurements	x	x	(x)	x	(x)		(x)	(x)	(x)	(x)	(x)	x	(x)	(x)	(x)	(x)	x	x
ECG Report		x		x	(x)													
Physical Exam		x		(x)	(x)													
Skin Assessment				x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Eligibility	x			x	(x)	x												
Randomization						x												
Family History				x	(x)													
RAS and BP Med Log		x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
IGFR Procedure				x	(x)													
PERL Study Drug Prescription						x	x	x	x	x	x	x	x	x	x	x	x	x
Study Drug Compliance							x	x	x	x	x	x	x	x	x	x	x	x
<b>CENTRAL LAB</b>																		
Serum uric acid, serum creat, cystatin C	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Urine ACR/AER	x		x	x	(x)													
HbA1c	x			x	(x)			x	x	x	x	x	x	x	x	x	x	x
HLA B*58:01				x	(x)													
IGFR				x	(x)													
NIDDK Repository: serum, plasma, urine				x														
<b>LOCAL LAB</b>																		
Pregnancy test serum HCG	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Pregnancy test urine dipstick		x			(x)													
ALT, K, CBC, serum creatinine, urine	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Protocol Deviation		x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Adverse Events		x	x	x	(x)	x	x	x	x	x	x	x	x	x	x	x	x	x

\*If normal blood pressure control is not achieved at Visit 4, the run-in period may be extended for two more weeks after which participants will be examined as in Visit 4 (Visit 4A). In this event, the GFR measurement scheduled for Visit 4 will be conducted at Visit 4A.

^ Study visits will be generally conducted at the Study Sites or their Satellites. "In-Person Visits" (V) are required for Visit 2 and all visits requiring iohexol-GFR measurements. If a participant lives far from a study site or satellite, or travel impediments are present, other (O) visits may be conducted remotely or in-person. For any given study visit to be conducted remotely, a Phone Visit and a Remote Biospecimen Collection will be both required; a Phone Visit is performed by the study coordinator using the telephone or other media such as Skype to collect results of study procedures that do not require physical interactions (e.g., collection of medical history), and a Remote Biospecimen Collection is performed at a clinical laboratory close to where participants live.

Note: (x) indicates an optional assessment. For BP and Measurements, (x) indicates an optional assessment only if the patient is NOT seen in-person.

## 8.2. Screening and Enrollment in the Run-in Period (Visit 1)

Subjects who have a confirmed history of micro- or macroalbuminuria (at least two out of three consecutive urinary AER or ACR in micro- or macroalbuminuria range as defined in Section 5.1) will not need to bring a sample of urine to Visit 1. Subjects who have incomplete or no previous evidence of micro- or macroalbuminuria or have unknown albuminuria status, will be mailed two containers before Visit 1 along with instructions for collecting two samples of urine from their first morning void and bringing it to the visit to confirm the presence of micro- or macro-albuminuria. During **Visit 1**, subjects will undergo the following procedures:

- Obtain written informed consent.
- Collect prior and concomitant medications, and demographic information.
- Measure weight and height.
- Measure vital signs.
- Perform pregnancy test in women of childbearing potential.
- Review the fetal risks of RAS blockade.
- Collect samples for clinical laboratory assessment.
- Provide a container and instructions for an overnight urine collection to be made immediately before Visit 3 if subject qualifies for the study.
- Upon receipt of laboratory measurements, confirm that inclusion/exclusion criteria are met.

The Screening Visit can be repeated after 4 weeks if the circumstances that led to the exclusion of a participant are deemed to have possibly changed.

Patients on losartan with uric acid levels between 4.2-4.4mg/dl at initial screening may, with the agreement of the patient and their PCP, be switched to another angiotensin receptor blocker (ARB) and have their uric acid level rechecked in one month.

## 8.3. Run-in Period (Visits 2, 3, and 4)

Starting at **Visit 2**, eligible subjects who agree to participate in the study will enter a run-in period of 9 weeks (see note at the end of this section for exceptions to this duration). During this visit, subjects will undergo the following procedures:

- Obtain written informed consent to enter run-in period (if the consent at V1 was only for screening).
- Review the fetal risks of RAS blockade.
- Collect medical history.
- Perform ECG.
- Collect concomitant medications.
- Measure weight and height.
- Measure vital signs.
- Physical Examination
- Obtain a urine pregnancy test

RAS antagonist treatment will be standardized, and BP, if elevated (>140/90 mm Hg), normalized. Letters will be written to the participants' physicians informing them about the study and notifying them of the study's protocol RAS blocker requirements and blood pressure goals. The letter will propose active participation of the participants' physicians in blood pressure management with the



availability of advice from the PERL site physicians and, if needed, the PERL Drug monitoring Committee for out of range blood pressure values during the course of the study. The run-in period will start at **Visit 2**. If a participant is already on a RAS Blocker, its dose will be increased, if necessary, to make it at least equivalent to ramipril 10 mg (if on ACE inhibitor [ACEI]) or irbesartan 300 mg (if on an angiotensin receptor blocker [ARB]), if acceptable to the patient's primary physician, if tolerated and if not contraindicated (see below). Participants who were not taking a RAS Blocker will be prescribed and instructed to start taking 10 mg of ramipril daily or 300 mg of irbesartan daily (if ramipril is contraindicated or has side effects) or another ACE inhibitor or ARB at equivalent doses if there are impediments to the use of ramipril or irbesartan. Participants who have contraindications to RAS blockers (e.g., SBP<100 mmHg, K<sup>+</sup>>5.5 mEq) or do not have evidence or history of micro- or macroalbuminuria (as defined in 5.1.4), are normotensive, and are not being treated with RASB or other anti-hypertensive agents will not be treated with these drugs, as this represents the standard of care.

Participants who are placed for the first time on RAS blockers as part of this study will start with half a dose; if there are no side effects, this will be increased to a full dose at Visit 3 and their serum K<sup>+</sup> and creatinine measured at a local laboratory after 2 weeks. RAS blockers will be immediately discontinued in the case of allergic reactions or angioedema or the suspicion of pregnancy. If pregnancy is confirmed, the patient will remain off RAS blockade until the pregnancy and breast-feeding are completed. Their dose will be decreased to half if symptomatic hypotension (SBP<100%) or intractable cough develops, followed by their discontinuation. For persistent cough with ramipril or other ACEI, irbesartan or another ARB will be prescribed in substitution of the ACEI.

In the case of hyperkalemia (K<sup>+</sup> >6.0 mEq) or serum creatinine elevation (>30% increase over baseline values), the participant will be asked to immediately obtain a confirmatory lab value at their local lab or clinical site and then discontinue the RAS blocker while awaiting this confirmatory result. If confirmed, the participant will resume RAS blockade at half dose 72 hours later and will have repeat labs one week later. If the problem persists, RAS blockade will be discontinued for the remainder of the trial and BP managed by alternate drugs (see below). If not confirmed, the participant will resume RAS blockade at their usual dose and have a repeat lab check one week later. These same steps will be taken if hyperkalemia develops during the trial.

Participants will continue to take any other antihypertensive drug that they may have been taking before study entry. Participants will be provided with a blood pressure monitoring device (if they do not already have access to one), will be trained on its use, and will be instructed to periodically monitor their blood pressure at home and to record the results into a BP diary, and to communicate them to study personnel if values are abnormal.

If hypotension develops (SBP<100 or significant lightheadedness), the dosage of non-RAS antagonist antihypertensive drugs will be progressively reduced until discontinuation, followed by a reduction of RAS blockers to half the dose and their discontinuation if the problem persists. If BP is found to be elevated (>140/90 mm Hg) on three consecutive occasions, the dosage of existing non-RAS antagonists antihypertensive drugs will be maximized, followed, if necessary, by the introduction of antihypertensive drugs of a different class. These will be chosen in collaboration with the other health care providers that are involved in managing the participant's anti-hypertensive therapy<sup>33</sup>. If the goal of BP ≤140/90 is not achieved with these drugs, a Drug Monitoring Committee conference call will be convened to consider the possibility of causes of hypertension other than diabetic nephropathy and discuss alternative therapeutic approaches. BP will continue to be monitored and the anti-hypertensive therapy to be adjusted in a similar way throughout the study.

After 2 weeks of run-in, participants will come in for **Visit 3** during which they will undergo the following procedures:

- Obtain interval medical history (with special emphasis on CVD events).
- Review concomitant medications and AEs

- Review RASB and BP therapy.
- Collect samples for clinical laboratory assessments as outlined in Figure 1.
- Perform pregnancy test in women of childbearing potential.
- Be provided with a container and instructions for an overnight urine collection to be made immediately before Visit 4.

After 6 weeks of run-in, participants will come in for **Visit 4** during which they will undergo the following procedures:

- Obtain interval medical history (with special emphasis on CVD events).
- Conduct a physical exam (if deemed to be required by the study physician)
- Review concomitant medications and AEs
- Review BP therapy.
- Review the fetal risks of RAS blockade.
- Measure height, weight and vital signs.
- Perform ECG.
- Collect samples for clinical laboratory assessments (including HLA B\*58:01) as outlined in Figure 1.
- Perform pregnancy test in women of childbearing potential.
- Measure iohexol GFR.

If normal blood pressure control is not achieved at Visit 4, the run-in period may be extended for two more weeks after which participants will be examined as in Visit 4 (Visit 4A). In this event, the GFR measurement scheduled for Visit 4 will be conducted at Visit 4A. Participants whose SBP is >150 or whose DBP is >95 mmHg at the end of the run-in period will be discontinued from the study (prior to randomization).

**IMPORTANT:** Visit 2 and Visit 3 can be skipped, i.e., a participant can move directly from Visit 1 to Visit 4, if the following criteria are met at Visit 1:

1. The participant is eligible based on the results of Visit 1 assessments, including laboratory values;
  2. Blood pressure is <140/90 mmHg;
- AND
3. The participant meets one of the following criteria:
    - Has been treated with a RASB for at least two months at a dose at least equivalent to Ramipril 10 mg or Irbesartan 300 mg;
    - Has contraindications to RASB;
    - Does not have evidence or history of micro- or macroalbuminuria (as defined in 5.1.4) and is not being treated with RASB or other anti-hypertensive agents.

If the above criteria are met and Visits 2 and 3 are skipped, Visit 4 will be scheduled 3 weeks after Visit 1 with a window of 2 weeks before and 3 weeks after the target date. The collection of medical history and the physical exam scheduled at Visit 2 will be conducted at Visit 4.

#### **8.4. Enrollment in the Study and Randomization (Visit 5)**

- At the end of the run-in period, eligibility will be re-assessed based on the BP measures obtained at Visits 4 or 4A (if applicable), HLA-based genetic susceptibility to allopurinol skin reactions<sup>36,37</sup> (tested at Visit 4) and a valid baseline iGFR measurement. Participants who are eligible for randomization based on those measures (SBP ≤ 150 and DBP ≤ 95 mmHg) and a negative HLA B\*58:01 test will be telephoned by the study coordinator to discuss how the

study medication should be taken and its potential side effects.

- Immediately after the phone call, the participants will be randomized.
- Immediately after randomization the first batch of study medication will be mailed to the participant by the research pharmacy along with written instructions on how to take it. Participants will be instructed to notify the study personnel by phone and start taking the study medication as soon as they receive it.
- If the participant is positive for HLA-based genetic susceptibility to allopurinol skin reactions, or acceptable BP measurements, or a valid iGFR measurement cannot be obtained, he/she will be discontinued from the study prior to randomization.

### **8.5. Treatment Period (Visits 6 to 15)**

During the treatment period, the following procedures will be completed at each visit for each participant:

- Obtain interval medical history (with special emphasis on BP control and CVD events).
- Review of concomitant medications and AEs.
- Review RASB and BP therapy.
- Measure height, weight, and vital signs according to the schedule outlined in Figure 1
- Inspect for skin rash.
- Conduct a physical exam (Visit 11).
- Perform ECG according to the schedule outlined in Figure 1 (Visit 11).
- Collect samples for clinical laboratory assessments and for storage of serum, plasma and urine for later biomarker research according to the schedule outlined in Figure 1.
- Perform pregnancy test in women of childbearing potential.
- Measure GFR by means of plasma disappearance of non-radioactive iohexol, iGFR at Visit 11.
- Provide a container and instructions for an overnight urine collection whenever an AER measurement is scheduled at the following visit.

In the days immediately after each visit, upon completion of serum creatinine measurements, participants will receive a new batch of study medication by mail from the research pharmacy. Upon receipt of the new tablets, participants will be instructed to immediately mail the pill bottles or the blister packs with the tablets remaining from the previous prescription to the study center for drug accounting and compliance assessment (see 6.1.2). A pre-stamped and addressed envelope will be provided to participants for this purpose.

At some sites the study medication may be dispensed directly to the study participant at a relevant in person study visit or by mail from the site following a relevant in-person or phone study visit.

### **8.6. End of Intervention (Visit 16)**

At the end of the treatment period (Visit 16), the following procedures will be completed for each participant:

- Obtain interval medical history (with special emphasis on CVD events).
- Review of concomitant medications and AEs.
- Review RASB and BP therapy.
- Collect unused study medication and document compliance.

- Measure height, weight and vital signs.
- Inspect for skin rash.
- Conduct a physical exam.
- Perform ECG.
- Collect samples for clinical laboratory assessments as outlined in Figure 1 and for storage for later biomarker research.
- Perform pregnancy test in women of childbearing potential.
- Measure iGFR.
- Provide containers and instructions for 2 overnight urine collections to be made immediately before Visit 17.

Participants will be instructed to stop taking the study medication and to mail the pill bottles or the blister packs with the tablets remaining from the last prescription to the study center if they did not already bring the unused study medication at the visit. The RAS and BP therapy will be continued as before until the closing visit (Visit 17). The importance of coming back in 8 weeks for the closing visit (Visit 17) will be emphasized.

### **8.7. End of Wash-out Period (Visit 17)**

After the end of the treatment period, participants will enter an 8-week wash-out period at the end of which the following procedures will be completed:

- Obtain medical history.
- Review of concomitant medications and AEs.
- Measure height and weight and vital signs.
- Inspect for skin rash.
- Collect samples for clinical laboratory assessments as outlined in Figure 1 and for storage for later biomarker research.
- Perform pregnancy test in women of childbearing potential.
- Measure iGFR.

### **8.8. RAS blocking and anti-hypertensive therapy after completion of the study**

When the participant completes the study, control of the RAS-blocking and anti-hypertensive therapy will be relinquished to the participants' physicians, who will decide whether or not to continue the therapy established during the study. Participants will continue the anti-hypertensive therapy established during the study until they see their physicians.

### **8.9. Future biomarker studies**

Plasma, serum, and urine specimens and DNA will be stored the Advanced Research and Diagnostics Laboratory at the University of Minnesota and the NIDDK Central Repository for possible future studies of biomarkers of kidney disease in diabetes or other diabetic complications. Twelve ml of plasma, 12 mL of serum, and 24 ml of urine will be collected at Visit 4, 11, 16, and 17, with one quarter of the aliquots of each stored at the University of Minnesota and three quarters of the aliquots sent to the NIDDK Central Repository for storage. Ten ml of whole blood will be obtained at Visit 3. This will be used for white blood cell DNA extraction and subsequent storage. Altogether, the stored plasma and serum aliquots will correspond to about 210 ml of blood collected for this purpose over the entire duration of the study. Participants will be allowed to elect to participate in the study while not having any or one or more of these samples stored, if they so choose.

## 8.10. Early Withdrawal

Unless the participant withdraws consent, all randomized participants will be followed for the full study period (through week 164) and all data will be collected as scheduled.

## 9. SAFETY ASSESSMENTS

### 9.1. Demographic Data/Medical History

After collecting a detailed medical history at Visit 1, this information will be updated at each visit through a structured interview, with a special emphasis on skin symptoms and signs such as rash, itching and exfoliation and on pregnancy in females. Participants will be instructed to communicate any change in their health status and intervening hospitalizations to the study coordinator in-between visits. In particular, they will be instructed to discontinue study medication and immediately contact the study coordinator if they develop a suspicious skin rash, swelling of the lips or mouth, arthralgias, and/or jaundice, which may indicate a hypersensitivity reaction to allopurinol. Fever and chills should also be reported but would not require cessation of medication prior to discussion with study personnel.

### 9.2 Skin exam

The skin of study participants will be examined for the presence of any kind of rash at each in-person visit. Participants will be instructed to carry-out periodical skin self-exams. If skin abnormalities are reported to the study personnel during the phone visits or on any other occasion, participants will be asked to immediately report to the study site, their PCP's office, or other local healthcare facilities for an in-person skin exam. Suspicion of drug allergy or Stevens-Johnson Syndrome SJS would require immediate discontinuation of study medication and dermatologic consultation.

### 9.3. Vital Signs

Blood pressure and heart rate will be recorded at each in-person visit. BP readings at home will be reviewed during each phone visits; if abnormal values are reported, participants will be asked to visit the study site, their PCP's office, or other local healthcare facilities to have their BP measured.

### 9.4. Clinical Laboratory Tests

Serum ALT, creatinine and K<sup>+</sup>, and CBC will be monitored and a pregnancy test, if a female of child bearing potential, performed at each visit. Participants who are started for the first time on RAS blockers as part of this study will have their serum K<sup>+</sup> and creatinine measured at a local laboratory after 2 weeks of full dose RASB treatment (i.e., after Visit 3). HbA1c will be measured at Visits 1, 4, and 7-17. An ECG will be performed at Visits 2, 4, 11, and 16.

### 9.5. Management of Uric Acid Levels

Study participants and study personnel, other than the DCC and the study pharmacists, will be masked as to the uric acid levels obtained during the study. The patients' physicians will receive written requests to refrain from measuring uric acid levels during the time of the patients' participation in the study, except as is mandatory for the patient's wellbeing, e.g., in the treatment of malignancy or diagnosis of a clinical syndrome highly likely to represent gout. If gout is diagnosed, open-label treatment with allopurinol will become indicated. In such case, the study drug will be discontinued but the patient will remain in the study and will continue to be followed as if he/she was taking the study medication. If uric acid lowering for malignancy treatment is required, the patient will receive open-label treatment until such time as return to study drug is deemed clinically reasonable by their physician.

## 10. ADVERSE EVENT REPORTING

### 10.1. Definitions

An Adverse Event (AE) is any untoward medical occurrence in a study participant regardless of its relationship to study treatment. A treatment-emergent AE is an adverse event occurring during the period between the first dose and 30 days after the final dose of the study medication. A Serious Adverse Event (SAE) is any untoward medical occurrence that results in death, is life-threatening, requires hospitalization or prolongation of an existing hospitalization, results in persistent or significant disability, or is a congenital anomaly/birth defect. Important medical events that do not fall into the above categories may also be considered an SAE when, based on medical judgment, such events may jeopardize the patient's safety and require medical/surgical intervention to prevent one of the outcomes listed in the SAE definition. The term SAE is not intended as a measure of severity or intensity. All AE's/SAE's that occur after the time of informed consent will be reported.

A Suspected Adverse Reaction is any adverse event for which there is a reasonable possibility that the drug caused the adverse event. For the purposes of IND safety reporting, "reasonable possibility" means there is evidence to suggest a causal relationship between the drug and the adverse event. Suspected adverse reaction implies a lesser degree of certainty about causality than adverse reaction, which means any adverse event caused by a drug. An Unexpected Adverse Event or Unexpected Suspected Adverse Reaction is an adverse event or suspected adverse reaction that is not listed in the investigator brochure or is not listed at the specificity or severity that has been observed; or, if an investigator brochure is not required or available, is not consistent with the risk information described in the general investigational plan or elsewhere in the current application, as amended. For example, under this definition, hepatic necrosis would be unexpected (by virtue of greater severity) if the investigator brochure referred only to elevated hepatic enzymes or hepatitis. Similarly, cerebral thromboembolism and cerebral vasculitis would be unexpected (by virtue of greater specificity) if the investigator brochure listed only cerebral vascular accidents. "Unexpected", as used in this definition, also refers to adverse events or suspected adverse reactions that are mentioned in the investigator brochure as occurring with a class of drugs or as anticipated from the pharmacological properties of the drug, but are not specifically mentioned as occurring with the particular drug under investigation. An Expected Adverse Event or Expected Adverse Reaction is any adverse experience that has been identified in nature or severity in the current investigator brochure and/or protocol.

## 10.2. Adverse Events Reporting

All AEs will be reported on the Adverse Events form that will be completed by the study staff, who are masked as to study treatment assignment, at each regular follow-up visits. This will insure that AEs are ascertained in an unbiased manner using the same standardized methodology for participants in both treatment arms. Forms will include standardized questions relating to specific events of import in diabetic patients on either of the study treatment arms as well as any significantly abnormal physical finding identified on examination and any significantly abnormal laboratory results obtained on the patient between visits or at the time of the visit. AEs reported or ascertained between clinic visits will be captured and reported at the time of the next schedule visit. Pre-existing conditions (that is, any condition that was known to be present prior to the signing of informed consent or was identified during the screening procedures at Visit 1) will not be considered or recorded as AEs unless the condition worsens in intensity or frequency after Visit 1. Likewise, continuing AEs will not be reported as AEs at subsequent visits unless they increase in severity or frequency between visits, they results in criteria for a SAEs, and/or they resolve between visits. Each site will be responsible for reporting all AE's to their IRB according to its AE reporting policy and procedures.

## 10.3. Assessment of Causality and Severity

The seriousness of adverse events will be ascertained by the study staff according to the criteria listed in 10.1 and the need for further evaluation, follow-up, or referral. The relationship between study participation and AEs will be determined according to the following criteria:

**A. Not related** – temporal relationship of the onset of the event, relative to study participation, is not reasonable or another cause can by itself explain the occurrence of the event.

**B. Possibly related** – temporal relationship of the onset of the event, relative to study participation, is reasonable but the event could have been due to another, equally likely cause.

**C. Probably related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and the event is more likely explained by the study treatment than by another cause.

**D. Definitely related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and there is no other cause to explain the event.

#### 10.4. Serious Adverse Events Reporting

See Section 15 – Data and Safety Monitoring Plan.

### 11. STATISTICAL ANALYSIS

This section presents a summary of the planned statistical analyses. A statistical analysis plan (SAP) will be written for the study that contains detailed descriptions of the analyses to be performed. The SAP will be written prior to database lock.

#### 11.1. Analysis Population

For most of the analyses, including the primary efficacy analysis described in section 11.3, an intention to treat (ITT) analytical approach will be employed. Accordingly, the population for statistical analysis will consist of all randomized study participants considered in their original randomization group, regardless of treatment discontinuation or loss to follow-up.

Selected secondary efficacy analyses will be performed using a per-protocol analytical approach. In this case, the analysis population will consist of the ITT population excluding data points which 1. had cumulative exposure to the study medication from randomization that was less than 80% of the theoretical full exposure; or 2. during major protocol deviations (e.g., treatment with prohibited medications), which could affect primary outcome.

#### 11.2. Initial Data Analysis

The initial data analysis will be performed to detect any differences in distributions of characteristics measured at baseline, 4, 20, 36, and 38 months (0, 16, 80, 156, and 164 weeks, respectively) between study groups. The number of patients screened, enrolled, and completing the study will be summarized within and across study centers. Measures of central tendency (means, medians) and variability (standard deviations, ranges) will be estimated from the data for continuous variables. Frequency distributions will be provided for categorical data. This preliminary analysis step will provide us with insight into data, distributions of the variables considered, and will allow us to find additional invalid values not detected earlier during data validation.

#### 11.3. Primary Efficacy Analysis

For the primary endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention), we will follow the recommendations by Carpenter et al<sup>38,39</sup> and perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable. By conditioning on the baseline iGFR measure we will also effectively use this variable as a covariate. Treatment group, study center, stratifying variables, albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did not qualify by eGFR slope and were normoalbuminuric at baseline), baseline AER, time, and time by treatment interaction will also be included as covariates in the model. Three features make this analytical approach especially attractive:

1. If there is no dropout (a very unlikely case), the estimate of the treatment effect at the end of the 2-month wash-out period following the 3-year intervention and its precision obtained using this approach will be exactly the same as those based on a classical approach employing an analysis of covariance (ANCOVA) model with treatment group, study center, iGFR and AER/ACR measured at baseline included as covariates.
2. If the iGFR measure at the end of the wash-out period is missing, we will be able to efficiently use the information contained in the intermediate iGFR measurements obtained at 80 and 156 weeks, by virtue of them being correlated with the GFR measurement at washout. Estimate of the treatment effect obtained this way is valid under the missing at random (MAR) assumption. This is in contrast to the ANCOVA approach, which would lead to the loss of this information and would require a more stringent assumption about the mechanism of data missingness, i.e. a missing completely at random (MCAR) mechanism.
3. The underlying analytical framework allows the use of all post-randomization data and is well suited to investigate the reason for withdrawal, for example to study whether participants having low iGFR values are more likely to withdraw.

Calculations will be performed using SAS PROC/MIXED. Results of the analysis will be expressed in terms of point estimate and its corresponding 95% confidence interval for the treatment effect at the end of the 2-month wash-out period following the 3-year treatment and will be accompanied by the corresponding p value.

#### 11.4. Secondary Efficacy Analyses

1. The effect of treatment on the iGFR at the end of the 3-year treatment period (before the washout) will be evaluated using the same analytical approach employed for the primary outcome.
2. The effect of treatment on the eGFR at 4 months after randomization will be evaluated using the same analytical approach employed for the primary outcome.
3. The iGFR and eGFR time trajectories, estimated from periodical iGFR measures and quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations<sup>34,35</sup>, respectively, will be analyzed using linear mixed-effects models.<sup>40-42</sup> The main objective of the analysis will be to construct confidence interval for the effect of the intervention over three years of observation (treatment main effect) and investigate whether the effect of the intervention changes with time (time by treatment interaction).
4. Time to serum creatinine doubling or ESRD in the two treatment groups is subject to censoring due to dropouts or reaching the end of study before the participant experiences the event. Survival time will be defined as the time from randomization to the event (the first of serum creatinine doubling from baseline or occurrence of ESRD, defined as eGFR  $\leq$  15 ml/min/1.73 m<sup>2</sup>, hemodialysis, or kidney transplant) or, for participants who did not experienced an event, to the last study visit. Data will be summarized by means of Kaplan-Meier survival curves and by providing the proportions of participants surviving without events at 1, 2, 3 years, and at the end of the wash-out period along with their 95% CIs. Given the potentially small number of events, differences between study groups will be tested by means of the log rang test or by means of simple Cox regression models including a limited number of predictors in addition to treatment group.
5. The effect of treatment on the AER at the end of the wash-out period, based on the geometric mean of two AER measured at this time point and adjusted for the geometric mean of AER at baseline (Visit 3 and 4), will be investigated in a linear regression model framework as in the case of the primary outcome.



6. The effect of treatment on the AER at the end of the treatment period, based on the geometric mean of the AER measures at visit 15 and 16 adjusted for the geometric mean of AER at baseline (Visit 3 and 4) will be investigated as in #5.
7. Time to fatal or non-fatal cardiovascular events will be analyzed as proposed for time to serum creatinine doubling or ESRD.
8. We will perform a per-protocol analysis (as defined in 11.1) for the primary efficacy endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention).

### 11.5. Incomplete Data

Missing values represent a potential source of bias. Efforts will be made to keep all participants in the study. If this is not feasible, at least some information regarding the status at the end of the trial will be obtained. For randomized patients, the number of completers and dropouts will be summarized. This procedure will help to compare characteristics of the participants' groups who drop out from the study with those who completed the study by treatment group, within and across study centers. The models considered in the proposal allow for a missing at random (MAR) mechanism. MAR means that the missing values mechanism can be explained by observed data and does not depend on the *unobserved* values of outcome measures. The differences in distributions between characteristics of the groups may indicate potential sources of bias due to missing values. For instance, some patients may dropout from the study due to *unobserved* factors related to the intervention itself. If we suspect such bias is present, the methods discussed in this section, assuming (MAR), are not applicable. We will incorporate plausible missing values mechanism into the model as discussed in Little<sup>43</sup> and investigate how such mechanism may affect the estimates of treatment effect. To this end, sensitivity analyses will be conducted involving selection and/or pattern-mixture models<sup>44</sup> with an appropriate submodel used to describe dropout.

### 11.6. Pilot participants

All pilot participants who were already randomized to allopurinol or placebo during the pilot will be included in the final analysis of the pivotal trial. Those who do not consent to the pivotal trial will be treated as having dropped from the study at a time corresponding to their last pilot visit. Sensitivity analyses will be performed to investigate whether results may be potentially affected by the roll-over of pilot subjects in the pivotal trial.

### 11.7. Model assumptions and alternative analyses

Model assumptions will be thoroughly checked for individual and systematic departures, using informal, e.g. inspection of residuals, and formal methods such as score test for extra parameter or methods based on likelihood displacement. If individual outliers are detected, their influence will be evaluated using influence diagnostics methods based on comparing estimates from models fitted to data with and without outlying values. Whenever we are not successful in fitting the parametric model (linear or non-linear), then non-parametric analyses and/or transformation of the variables involved in the analysis will be considered. To investigate the potential hemodynamic influence of allopurinol on treatment effect, in addition to the aforementioned analyses, we will consider models including the post-randomization measure of GFR at 4 months as an additional covariate. To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses (based on the primary efficacy analysis described in section 11.3, with the inclusion of an interaction term of the treatment group by the subgroup variable) will be performed by age groups ( $\leq 40$  and  $> 40$  yrs), gender, racial/ethnic group, HbA1c ( $\leq 7.8$  and  $> 7.8\%$ ), serum uric acid ( $\leq 6.0$  and  $> 6.0$  mg/dl), baseline iGFR ( $\leq 70$  ml/min and  $> 70$  ml/min/1.73m<sup>2</sup>), AER at baseline ( $\leq 300$  and  $> 300$  mg/24 hr), and albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did not qualify by eGFR slope and were normoalbuminuric at baseline). To investigate possible influence of using selected covariates on the treatment effect estimate in the models considered in Section 11, we will

perform appropriate sensitivity analyses. These additional analyses will be considered as strictly exploratory.

### 11.8. Safety Analyses

Adverse events will be independently reviewed by an independent data safety monitoring board (DSMB, see Sections 15 and 16). All safety data will be available in data listing in the clinical protocol report. Data will be described in terms of descriptive statistics and presented by treatment group. Presentation will include graphs (scatterplots, boxplots, histograms), measures of central tendency (mean, median) and variability (confidence intervals) for continuous variables and frequency tables for categorical variables.

### 11.9. Interim Analysis

No formal interim analyses of efficacy to stop for benefit or futility are planned, given the timing of the primary endpoint.

### 11.10. Sample Size

Since a variance-covariance matrix for the iGFR measures is not available and this matrix is essential in order to perform formal power calculations for a model with correlated errors, we performed alternative power calculations based on an intent-to-treat analysis within an ANCOVA framework. Specifically, we assumed that the primary hypothesis is tested in the following model:

$$M1: \text{iGFR at washout} = \text{iGFR at baseline} + \text{treatment group}$$

Compared to the model that will be used in the primary analysis, model M1 is simplified in two aspects. First, it does not use information from iGFR values measured at intermediate time points. Second, it does not include covariates such as the stratifying variables (HbA1c and UA) or other GFR predictors such as baseline AER. Both of these aspects may lead to loss of precision of the treatment effect estimate. Consequently, our sample size calculations should be considered as conservative.

The hypothesis being tested, i.e. the effect of treatment on iGFR at washout, corresponds to testing whether the treatment group factor in Model M1 is significant. The choice of the ANCOVA model for the purpose of power calculations is sensible, as residuals from a univariate model involving baseline iGFR as covariate fitted to data from RASS study conform to normal distribution. Sample size calculations were performed based on Cohen<sup>45</sup> and making the following assumptions:

1. Postulated effect on iGFR at washout ( $\cdot$ ) = 3 ml/min/1.73 m<sup>2</sup>. We deem this effect to be clinically meaningful and attainable. It is clinically meaningful because it would translate on average into a 10-year delay in the progression to ESRD. It is attainable because it is smaller than the difference in 3-year GFR that we observed in the JKS between subjects with serum UA  $\geq$  4.5 mg/dl compared to those with levels below this value. The postulated effect was based on the following changes in GFR levels in the two treatment groups:
  - a. Untreated group = 3 ml/min/1.73 m<sup>2</sup> per year. This estimate is based on data from the Joslin Kidney Study (JKS), in which the median GFR loss among 43 subjects meeting the above criteria was 3.1 ml/min/1.73 m<sup>2</sup> per year, with 70% of subjects having a GFR loss  $>$ 1.5 ml/min/1.73 m<sup>2</sup> per year. Also, among 116 subjects from Steno who met the albuminuria and GFR criteria, but for whom serum uric acid values were not available, the median GFR loss was 3.3 ml/min/1.73 m<sup>2</sup> per year, with 71% of subjects having a GFR loss  $>$ 1.5 ml/min/1.73 m<sup>2</sup> per year.
  - b. Treated group = 2 ml/min/1.73 m<sup>2</sup> per year. The average GFR loss in the JKS subjects with serum UA  $<$ 4.5 mg/dl was 1.5 ml/min per year. On this basis, we conservatively assumed that the allopurinol treatment, if effective, would decrease the GFR loss to 2 ml/min per year (a 33% decrease compared to the untreated group).

2. **Standard deviation (SD) of residual error** = 10.1 ml/min/1.73 m<sup>2</sup>. This was estimated based on the root-mean-squared error from a regression model with eGFR at 3 yrs as the dependent variable and baseline eGFR as the independent variable fitted to data concerning T1D patients from the Joslin Kidney Study meeting the PERL inclusion criteria.

Assuming a two-sided alpha error equal to 0.05, the effective sample size needed to detect the pre-specified treatment effect ( $\Delta = 3 \text{ ml/min/1.73 m}^2$ ) at washout adjusted for baseline iGFR with 80% power is equal to n=180 per group. To take into account the anticipated overall dropout rate (up to 5%/yr or 15% over the entire duration of the study) and drug discontinuation or non-compliance in the treatment group (up to 2%/yr or 6% over the entire duration of the study), and to maintain the desired

power of at least 80%, it will be necessary to recruit n=240 subjects per group. In Table 1, we show the power of the proposed sample size for Model M1 under different dropout and non-compliance scenarios. We also provide the corresponding power for a model (Model M2) including the two stratifying variables (Hb1Ac and UA) and baseline AER as covariates to illustrate the effect of adding these variables to Model M1. In this analysis, we assumed that adding these covariates reduces the residual variance by 10%, which corresponds to these covariates explaining merely 4% of the total iGFR variation over and above the variability explained by iGFR at baseline. As shown in Table 1, once these covariates are accounted for, power is expected to exceed the conservative estimates provided by Model M1 and reach almost 90% for 15% dropout and 6% non-compliance rates.

**Table 1. Power to detect treatment effect for two ANCOVA models under different drop-out and non-compliance scenarios.**

Overall Dropout (%)	Non-compliance (%)	Model	
		M1	M2
9	0	.87	.92
12	0	.86	.91
15	0	.85	.90
9	6	.83	.89
12	6	.82	.88
15	6	.80	.87

## 12. DATA COLLECTION AND QUALITY ASSURANCE

Comprehensive data coordinating center (DCC) functions for this clinical trial, including clinical monitoring, database development, web-based data entry and management, as well as the creation and export of study reports for the DSMB will be provided by the University of Michigan Statistical Analysis of Biomedical and Education Research (SABER) group. Housed in the top nationally ranked Department of Biostatistics, SABER, in its 13-year existence, has served as the DCC for over 50 studies, including multiple NIH-sponsored networks.

The DCC will use OpenClinica® (OpenClinica Clinical Trial Software; OpenClinica, LLC, Waltham, MA), a clinical trial software platform for electronic remote (i.e., site-based entry) data capture and clinical data management, as the basis for our custom-designed data entry and management system. We expect that the majority of data will be collected via Case Report Forms (CRFs); however, other data sources, such as laboratory data from the central laboratory, may be used. In these circumstances, the DCC will also utilize electronic data transfer. Protocols for the transfer of data, with careful attention to data integrity, will be written by experienced programmers and stored in the OpenClinica database or data mart.

The DCC has established a set of standard operating procedures (SOPs) governing the processes used to ensure patient privacy and data confidentiality, including the use of anonymous participant IDs on CRFs and in reports. In addition to clinical study databases, the UM DCC has also incorporated MEDdra® [www.meddrasso.com/] and database into our systems to have the capacity to code adverse events and illnesses by body organ system, respectively. OpenClinica® enables compliance with Good Clinical Practice (GCP) and regulatory requirements by providing differentiated user roles and privileges, password and user authentication security, electronic signatures, SSL encryption, and comprehensive auditing to record and monitor access and data changes.

### **12.1. Case Report Forms**

Study information will be collected for each participant by study staff using standardized electronic Case Report Forms (CRFs). CRFs will be developed by the DCC, modeling their formats on the CRFs developed for the RASS clinical trial<sup>33</sup>, to which the study group has access through Dr. Mauer. CRFs will not report information about treatment assignment, in order to maintain blinding of study site. Forms will be stored at a secure location at the clinical sites.

### **12.2. Quality Control and Quality Assurance**

DCC staff will prepare data management and clinical monitoring plans. The clinical monitoring plan will detail procedures to assess accuracy of the database relative to source documents, as well as site adherence to regulatory and study procedures. Emphasis will be placed on the process of consenting subjects, compliance with regulatory requirements and study protocol, values of key endpoints, and identification of SAEs that may not have been reported. The data management plan will describe the front-and back-end edit checks, as well as forms tracking procedures, that will be implemented to ensure timely and high-quality data collection. It will also define the periodic reports that will be shared with site coordinators and PIs that summarize site performance. The clinical monitoring and data management procedures will be consistent with the International Conference on Harmonisation (ICH E6) standards for Good Clinical Practice (GCPs).<sup>46</sup>

#### **12.2.1. Clinical monitoring**

During trial conduct, the DCC will conduct periodic monitoring visits to ensure that the protocol and GCPs are being followed. The monitors may review source documents to confirm that the data recorded on CRFs is accurate. The investigator and institution will allow DCC monitors direct access to source documents to perform this verification. It is important that the investigator(s) and their relevant personnel are available during the monitoring visits and that sufficient time is devoted to the process.

Clinical monitoring will be conducted through approximately yearly on-site visits by qualified personnel. In the first year, site initiation visits will be conducted to review the protocol, verify that the Site Director and his/her collaborators receive all necessary trial documents for a proper trial conduct, and review the procedures related to CRFs completion and query resolution. Investigators will be instructed about the importance of recording accurate and clean data, avoiding protocol violations, and retaining participants in the study. Follow-up monitoring visits will involve the verification of source documents and reporting of adverse events. Monitors will also verify that an informed consent form is on file for each subject screened, with appropriately dated signatures and all pages present, that all of the inclusion and exclusion criteria were met for each subject enrolled into the study, and that all the withdrawals and dropouts of enrolled participants from the trial are reported and explained in the appropriate CRFs. Accrual and retention rates will also be monitored and if these fall appreciably below the projected levels, attempts will be made to identify the reasons. At the end of each monitoring visit, a clinical monitoring report will be prepared by the DCC Clinical Monitor and sent to the site PI and to a NIDDK representative with recommendations to correct problems and/or improve the trial quality.

#### **12.2.2. Statistical monitoring**

Clinical monitoring will be complemented by statistical central monitoring, as described by Venet et al.<sup>47</sup>. Such a statistical approach to central monitoring relies on assessing the clinical data for departures from expected patterns (e.g., baseline variables in our randomized trial should be comparable between treatment arms for each site; visit days should be randomly distributed over the week), and assessment of a greater number of data values than those associated with site performance metrics.

The statistical monitoring plan will be incorporated into the larger data management plan, and will identify the specific descriptive statistics, graphical presentations, and formal hypothesis tests to be performed and at what frequency. Pattern recognition may require a sufficient number of participants followed for a sufficient amount of time in order to be valid.

### **12.2.3. Laboratory quality monitoring**

The Central Laboratory at the U. of Minnesota uses internal quality control methods to assess assay precision and drift as well external quality control (e.g., proficiency testing) to assess accuracy. The laboratory is well versed in both aspects and adheres to rigid performance standards. For all analytes, enough commercial lyophilized control material is typically purchased prior to beginning the study, so that a single control pool lot can be used throughout. Control pool mean and between-day SD's for each analyte are routinely established at a minimum of two different analyte concentrations on a minimum of one hundred different analytical batches. In the unlikely event that a new control pool lot becomes necessary during the study, both the new and old control pool lots will be run for a minimum of 20 days to establish a target mean for the new pool. The target SD's are not changed if the new and old lots of control material are the same "matrix" (e.g., lyophilized human serum). An in-house control pool is also typically prepared from the exact same specimen type (e.g., 0.5-mL aliquots of pooled serum or EDTA-plasma from several individual donors), kept at -70°C, and incorporated as an internal control in all analytical batches throughout the study. This sort of control is very useful since occasionally the commercial pools of lyophilized serum have matrix effects that make assessment of the true accuracy of the given assay difficult to assess. Accuracy is assessed by comparison of external quality control proficiency testing results. For most of the assays, the laboratory participates in either the College of American Pathologists' Surveys Program (uric acid, creatinine, ALT, glycosylated hemoglobin, urine albumin, urine creatinine) or by sample exchange in a survey program with another laboratory (iohexol). The laboratory is CLIA certified.

### **12.3. Study Record Retention**

The source documents will be stored for at least 10 years after the study ends.

### **12.4. Data and Biosample Archiving in the NIDDK Central Repository**

In agreement with NIDDK's policy on the sharing of data from large, NIDDK-sponsored, multi-site studies, the data and biosamples collected in the course of the trial will be archived in anonymized form in the NIDDK Central Repository for future distribution to the scientific community. All samples and data transferred to the Repository will be under the custodianship of the NIDDK, although the study's Steering Committee will have proprietary control of and exclusive access to the sample and data for an agreed-upon period of time before these are made available to the wider scientific community.

## **13. PROTECTION OF HUMAN SUBJECTS**

### **13.1. Characteristics of the study population**

Participants will be patients who have had T1D for 8 years or more and who will be 18-70 years old at entry into the study. We anticipate that there will be approximately an equal number of males and females. There will be no selection criterion based on race, although most patients will be of Western background and European extraction, given the demographics of the cities in which the centers are located and the fact that T1D is 30-40% less common among Blacks and Hispanics than among Whites. Inclusion and exclusion criteria are as noted above. Female patients of child-bearing potential will be included in the study but only if pregnancy is not planned during the time frame of the study. Women who become pregnant during the study will be discontinued from the study medication, if they had already been randomized, and from RAS blockers until pregnancy and breast feeding are complete; iGFR will not be obtained during or for 6 months after pregnancy is completed. Individuals younger than

18 will not be included since kidney complications are rare before this age. Patients will be tested for HLA-based genetic susceptibility to Stevens-Johnson Syndrome and excluded if this is found.

### **13.2. Sources of research material**

1. Specimens on patients obtained specifically for research purposes.
  - a. Renal function studies requiring multiple blood specimens drawn from an indwelling IV over 4-6 hours for measurement of glomerular filtration rate at yearly intervals.
  - b. Collection of urine for measurement of urinary albumin and creatinine at 3-4 month intervals.
  - c. Blood for measurement of serum UA, creatinine, and liver enzymes, and WBC at quarterly intervals.
2. Specimens or measures obtained quarterly as component of routine patient care
  - a. HbA1c
  - b. Blood pressure
  - c. Height and weight
3. Patient and family medical information.

All study participants will be assigned a unique study identifier and in no publications or public presentations will information be available which could identify individual study participants or their families.

### **13.3. Plans for recruitment of subjects and consent procedures.**

Potential participants will be sought (1) from among the patients attending the study centers (including the satellite centers) involved in the study, (2) by placing advertisements at other health care facilities and in newspapers or other media, and (3) by soliciting referrals from other health care providers. At each clinical site, potential candidates will be identified and contacted according to the procedures established by the local IRBs in compliance with local laws protecting patient confidentiality. Invitation letters to patients attending the study centers will clearly offer the possibility to opt out of any further contacts with the study. Patients who agree to participate will be screened by means of a telephone or in-person interview to determine whether exclusion criteria apply. Subjects who respond to advertisements will undergo the same screening interview. Subjects who pass this initial screening will be given or mailed an informed consent form and will be invited to come to the clinic for a screening visit (Visit 1) during which a final eligibility determination will be made on the basis of a detailed medical history and laboratory tests. Written consent will be obtained on that occasion from all subjects undergoing the screening visit after explaining again the purpose and procedures of the study. In the initial contact and again at the time of the screening visit, study subjects will be encouraged to ask questions and they will be reassured that they may withdraw from the study at any time. Written consent will be obtained again at V2 if the consent at V1 was only for the screening procedures.

### **13.4. Potential Risks**

#### **13.4.1. Risks associated with screening procedures and blood tests**

After participating in the screening tests and procedures, or after the run-in period, subjects may find out that they are not eligible to participate in the study. In that case, they will be told the reasons for their ineligibility and will be given the results of clinically approved tests such as serum UA, serum creatinine, urinary albumin/creatinine ratio, and ALT. The results of the test for genetically increased risk of allopurinol-induced SJS will also be given to their physician, if the subject agrees with this. Thus, they may learn about as yet unknown health problems such as anemia or liver disease, more advanced kidney disease, or the need for allopurinol avoidance. This and/or the exclusion from the study may

cause psychological distress. The drawing of blood samples may cause some pain and discomfort and hematoma formation at the site of venipuncture. The total amount of blood taken for the entire study will be about 520 mL (16 ml at Visits 1, 3, 6-10, and 12-15; 67 ml at Visits 4, 11, 16, and 17). At the dose used in the study, there are no known risks to the infusion of the substance used for the measurement of renal function other than the very small risk of allergic reactions (<0.5%), diminished by the exclusion of patients with a history of iodine allergy and by having appropriate treatment drugs for allergic reactions on hand.

#### **13.4.2. Risks associated with allopurinol treatment**

Allopurinol has been used for several decades for the long-term therapy of symptomatic gout. The risks associated with its use are low and include:

- a. Skin rashes, usually pruritic maculopapular skin eruptions, sometimes scaly or exfoliative, are the most commonly reported adverse effect of allopurinol. Skin reactions were observed in the past in up to 3% of treated patients, but more recent data suggest that their frequency is now less than 1% ([www.drugs.com/pro/allopurinol.html](http://www.drugs.com/pro/allopurinol.html)) perhaps more likely due to changes in the filler compounds rather than the actual drug. Rashes may be followed by more severe hypersensitivity reactions such as exfoliative lesions and the Stevens-Johnson syndrome (erythema multiforme major), which can be fatal. Although such occurrence is very rare, in the order of 1 in 10,000<sup>48</sup>, treatment with allopurinol will be immediately discontinued if a rash develops and will not be reinstated. As noted, those with HLA-based genetic susceptibility to allopurinol-related SJS will be screened out. About 0.7% of Whites and 2-3% of African Americans and Asians are carriers of such genetic susceptibility.
- b. An increased frequency of acute gout attacks has been reported during the early stages of allopurinol administration, possibly resulting from the mobilization of urates from tissue deposits causing fluctuations in serum UA levels. Early studies estimated the risk of such events to be about 6%, but an analysis of current usage suggests that the risk has now decreased to less than 1% ([www.drugs.com/pro/allopurinol.html](http://www.drugs.com/pro/allopurinol.html)). The risk is expected to be even lower in this study population since individuals with a previous history of gout will be excluded and UA levels will be on average lower than in patients usually taking allopurinol for elevated UA levels.
- c. Reversible liver damage as well as asymptomatic rises in liver enzymes has been observed in 1-2% of patients taking allopurinol. Some very rare cases of irreversible liver damage have been observed in the context of the Stevens-Johnson syndrome.
- d. Bone marrow depression has been reported in patients receiving allopurinol, most of whom received concomitant drugs with the potential for causing this reaction. Bone marrow depression has been rarely observed in patients receiving allopurinol alone.
- e. Experience with allopurinol during human pregnancy is limited because women of reproductive age rarely require this treatment. Given this paucity of data, the study will consider it unsafe for the fetus or the mother to receive this drug. Allopurinol has been found in the milk of a mother on this drug and, therefore, will not be taken by nursing mothers.

#### **13.4.3. Risks associated with RAS blocker treatment**

Treatment with RAS blockers (either ACE inhibitors such as ramipril or angiotensin receptor blockers such as irbesartan) is currently the standard of care for diabetic individuals who have micro- or macroalbuminuria. The risk associated with the use of these drugs during the trial will not be greater than the risks participants would face outside the trial by being treated with these agents. These risks include allergic reactions, hyperkalemia, hypotension, increased serum creatinine, persistent cough (with ramipril), liver damage, bone marrow depression, and fetal and neonatal morbidity and death when RAS

blockers are taken during pregnancy. The occurrence of these adverse events will be monitored during the trial.

### **13.5. Procedures for protecting against and minimizing potential risks**

#### **a. General**

The patients are under constant medical supervision. They are told that the data which are collected will be used for scientific report, but they will not be identified in such reports.

#### **b. Specific**

1. Regular pregnancy tests and education regarding fetal risk of the study drug will be provided to female patients of child bearing age.
2. Quarterly measures of liver enzyme and white blood cell count will allow for early detection of liver injury or leucopenia potentially representing drug toxicity.
3. Quarterly measures of serum creatinine will allow titration of allopurinol in relation to kidney function to avoid excessive dosage of the medication.
4. IV's for kidney function studies will be placed by trained skilled clinical research nurses or technicians or by experienced physicians.
5. Blood drawing for laboratory studies will be performed by trained skilled phlebotomy personnel at the respective institutions, thus limiting the risk of discomfort or local hematoma formation.
6. Participants will be advised not to donate blood throughout the time they are in the study.
7. Regarding possible drug toxicity:
  - a. To avoid fetal risks from the study drug, patients planning pregnancies will not be included. Sexually active female patients will be instructed to immediately discontinue study drugs and RAS blockers if a menstrual period is missed by more than two weeks and, if found to be pregnant, the study medication and RAS blocker will be discontinued and not resumed until pregnancy and nursing are completed. Pregnancy tests will be done on all women of child-bearing potential at each visit.
  - b. Subjects with known allergy to xanthine-oxidase inhibitors will be excluded from the study. Patients will be instructed to immediately report skin reactions and allergic symptoms and to immediately stop the study medication should these occur. Patients will be given antihistamines for symptom relief. A small supply of antihistamines to be used in such an event will be supplied to each patient. Should an allergic reaction or skin rash occur, the study drug will be permanently discontinued.
  - c. To minimize the risk of gout attacks, subjects with a gout history will be excluded from the study and the allopurinol dosage in those enrolled in the study will be gradually increased over several weeks. Should a gout attack occur, this will be treated with colchicine or anti-inflammatory agents according to current standards of care by study personnel. Study uric acid levels <2.0 mg/dl will be flagged by the DCC and reported to the appropriate study pharmacist who will initiate a 50% dose reduction in study drug at the next quarterly visit. In order to avoid gout attacks, if uric acid levels exceed 12 mg/dl this will be flagged by the DCC and the center informed and open-label allopurinol will be started and titrated with the goal of bringing and keeping serum uric acid below 7.0 mg/dl. Participants will continue to be followed according to the study protocol and will be analyzed according to their blinded treatment groups.
  - d. Primary care physicians will be notified (with the participants' permission) of the patients' participation in the trial, so that they avoid the prescription of drugs interacting with allopurinol or notify the study personnel that treatment with such drugs is necessary.



- e. Participants will be reminded at each visit to immediately notify the study personnel if they start a new drug, so that possible interactions with allopurinol can be identified at once and appropriate precautions can be taken including discontinuation of the study drug.
- f. Subjects taking drugs known to interact with allopurinol in causing bone marrow depression will be excluded from the study. White blood cell counts will be done before the study drugs are prescribed, and quarterly thereafter. The study drug should be temporarily discontinued should evidence of bone marrow depression ( $WBC < 3500/mm^3$ ) be present and confirmed. WBC should be repeated two weeks after study drug discontinuation. If WBC recovers, consider re-challenging and repeating WBC two weeks after drug re-introduction. In addition, if WBC is confirmed to be  $< 2500/mm^3$  and/or ANC is  $< 1000/mm^3$ , the event also needs to be reported as an AE. The Drug Monitoring Committee will review each case and decide whether a referral to a hematologist is warranted and whether study treatment can be reinstated after blood values have returned to normal. If drugs potentially causing bone marrow depression in combination with allopurinol are begun after entry into the trial, observations for this side effect will be intensified or, if recommended by the Drug Monitoring Committee, study drug may be interrupted.
- g. To minimize the risk of allergic reactions during the iGFR measurement, subjects with a history of iodine allergy will be excluded from the study.
- h. To minimize the risk of liver injury, subjects with clinically significant hepatic disease and/or elevated liver enzymes above 2.5 x the upper limit of normal at the screening visit will be excluded from the study. In those subjects that are enrolled in the study, liver enzyme levels will be monitored at each follow-up visit. If levels are abnormal, the measurement will be repeated and if values are confirmed to be elevated the study drug will be discontinued. The Drug Monitoring Committee will review each case and decide whether a referral to a hepatologist is warranted and whether study treatment can be reinstated after enzyme values have returned to normal on the recommendation of a hepatologist.
- i. To minimize the impact of blood draws, participants with low hemoglobin levels ( $< 11$  g/dL in males,  $< 10$  g/dl in females) will be excluded. Subjects will be advised not to donate blood while participating in the study and for two months after their participation has ended. If they have just donated blood, their screening for the study will be delayed by 3 months. Hemoglobin levels will be monitored quarterly.
- j. Most of the participants will already be on RAS Blockers. For those who were not previously taking these medications, risks will be minimized by not prescribing RAS Blockers to participants who have contraindications to these drugs and by prescribing an ARB whenever ACE inhibitors are contraindicated. If adverse events develop that are deemed to be related to the use of RAS blockers, the dose of these drugs will be decreased, followed by their discontinuation if the problem persists (see 8.2. Run-in period), thus adhering to current standards of care. If receiving discontinuation of RAS blockade becomes necessary, BP will be managed by alternate drugs as described above.
- k. Blood pressure will be measured quarterly with the goal of maintaining BP  $\leq 140$  mmHg systolic and  $\leq 90$  mmHg diastolic. If elevated, a recheck will be performed within 2 weeks and if still elevated additional antihypertensive non-RAS blockers will be added in collaboration with the participants' physicians. Failure to achieve satisfactory BP control within 2 months would lead to a case review by the Drug Monitoring Committee.
- l. Participants with a decrease in both iGFR (meeting the  $R^2$  criterion described in 7.1.2) and eGFR from one measurement to the following one corresponding to a GFR decline  $> 20\%$  per year will be referred to a nephrologist to investigate the causes of such rapid loss of kidney function. If a decrease of such magnitude is observed for the iGFR but is less than a 20%

decline per year for the eGFR, the iGFR measurement will be repeated. If the >20% per year iGFR decrease is confirmed, the participant will be referred to a nephrologist for further evaluation. In this case, the first iGFR value will be used for the analysis of the primary outcome. If the >20% per year iGFR decrease is not confirmed, the participant will not be referred to a nephrologist. The first iGFR test will be reviewed to verify whether medical conditions that should have prompted a test postponement, such as dehydration or recent use of NSAID (see 7.1.1.3), were present. In that case, the repeated iGFR value will be used for the primary outcome analysis. Otherwise, the first iGFR value will be used.

- m. Data monitoring will be performed on a regular basis. Data entry computers will be programmed to flag any parameters outside clinically acceptable ranges.

### **c. Protection of confidentiality**

All data, forms, and specimens will be labeled with each study participant's unique study identifier. All data transferred to the Data Coordinating Center for accumulation in the central database or to the NIDDK Central Repository will identify study participants only with their unique study identifier. Each study center will maintain a file on each study participant that includes personal identifiers, linking name and contact information to the unique study ID. These data will not be entered into the study data management system. Participants' names and addresses will be shared with the Pharmacy along with selected laboratory results (serum creatinine and, if needed, uric acid) for the purpose of adjusting the dosage and mailing the study medication. Identifiers may also be shared with the local laboratories if required by the laboratory ordering procedures. Study participants' files will be kept in secure locations and the clinical center will be responsible for taking every other reasonable measure (those set by the state, the site, and the study) to ensure and maintain record confidentiality and patient privacy. Participants will be given the opportunity to decide whether or not the clinical information gained from the study should be shared with their health care providers. Participants will be made aware that, despite these measures, confidentiality cannot be totally ensured. Each site will adhere as required by law to regulatory oversight by federal and state agencies that have authority over the conduct of clinical research such as the Department of Health and Human Services, the Food and Drug Administration, the National Institutes of Health, the Office of Human Research Protection, the Department of Social Services and the Data Safety Monitoring Board.

### **d. Risk-benefit ratio**

If urate-lowering therapy is demonstrated to be effective in preventing or slowing early GFR decline, the reduction in morbidity and mortality resulting from the prevention or delay of ESRD would have a major impact on the lives of T1D patients as well as on society at large, significantly reducing the human suffering and financial costs associated with this condition. Also, demonstrating a causal link between serum UA and kidney damage in T1D would prompt further research on the molecular mechanisms responsible for this link, which could lead to the development of further interventions to prevent renal disease in T1D. Overall, the risks to study participants are deemed reasonable in relation to the anticipated benefit of identifying an effective therapy for early GFR loss in type 1 diabetes.

### **13.6. Incentives/remuneration**

If allowed by local regulations, participants will be reimbursed for the time and effort associated with participating in this study. Reimbursement amounts will be decided locally. Payments will be made at each visit. Participants with financial hardships deriving, for example, from loss of income or child care costs, may be reimbursed for such costs on a case by case basis. Participants who do not complete the whole study will only be reimbursed for the visits they completed. Transportation costs (e.g., parking or public transportation) may be reimbursed according to local policies. Remote site participant costs associated with travel to a Main Site will be reimbursed according to Federal travel, hotel, and per diem rates (gsa.gov).

### **13.7. Institutional Review Board**

The protocol and informed consent forms and subsequent modifications will be reviewed and approved by the Human Subject Committees at all the centers involved in the study for compliance with applicable standards/regulations.

#### 14. DATA AND SAFETY MONITORING PLAN

The Data and Safety Monitoring Plan for this study includes the following elements:

1. A *Data Safety Monitoring Board (DSMB)*, including outside experts in the design and conduct of clinical trials and in diabetic nephropathy, will be established by NIH. The purpose of the DSMB is to assure independent review as to whether study patients are exposed to unreasonable risk because of study participation, and to monitor study progress and integrity. The DSMB will receive detailed data from the Data Coordinating Center as frequently as deemed appropriate by the board, including summary tabulations and narratives of adverse events, and will meet periodically with the Study Investigators and the Data Coordinating Center personnel. They will have full access to all data, and their recommendations and input will be given high priority and will be incorporated into the study protocol. To this end, the DSMB will meet separately, *"in camera"* (Closed Sessions), with the Co-Director of the Data Coordinating Center, Dr. Andrzej Galecki, to review all adverse event data in relation to the randomized treatment groups in order to detect any increased frequency of significant adverse events which could be study drug related, and decide whether continuation of the trial is warranted.
2. *IRB monitoring* will be in place from:
  - ◆ Joslin Diabetes Center
  - ◆ University of Minnesota
  - ◆ University of Colorado
  - ◆ University of Michigan
  - ◆ Northwestern University
  - ◆ University of Toronto
  - ◆ Albert Einstein University
  - ◆ Washington University
  - ◆ Steno Diabetes Center
  - ◆ University of Calgary
  - ◆ University of Alberta
  - ◆ Emory University
  - ◆ University of Washington
  - ◆ University of Texas Southwestern
  - ◆ Providence Medical Research
  - ◆ BC Diabetes
3. *SAE reporting*.

All adverse events are reported to the DCC by completion of the Adverse Events Form. All SAEs as defined previously will require expedited event notification within 72 hours of occurrence or identification to the DCC. The DCC will promptly notify the study PIs, who may convene a Drug

Monitoring Committee (DMC) conference to acquire further information about the event and take appropriate actions concerning the study medication (see Section 15.1).

An independent physician not involved in the study will serve as the Medical Safety Officer, reviewing all SAEs promptly after being reported in the database by the clinical sites. Based on the clinical site report and any additional input from the DMC, the Medical Safety Officer will prepare a preliminary SAE narrative report (in cases where the SAE is not resolved) for each SAE which will be distributed to the PIs, NIDDK Program Director, DSMB Chair, clinical site director, and appropriate DCC staff. Once the SAE is resolved, a final SAE narrative report is generated by the Medical Safety Officer. This report will be sent to the clinical site PI to review for accuracy and completeness. Following review by the clinical site PI, the Medical Safety Officer will send the final SAE narrative report to the PIs, NIDDK Program Director, DSMB Chair, clinical site director, and appropriate DCC staff. All SAE narrative reports, both preliminary and final, will be reviewed by the DSMB during their regularly scheduled meetings or on an expedited basis as determined by the NIDDK Program Director, who will solicit the input of the Chair of the DSMB as needed. The FDA definitions and requirements for expedited reporting will be used to determine if any individual SAE warrants notification to the FDA and to the IRBs of all participating PERL clinical sites.

The clinical site at which the SAE occurred is responsible for expedited reporting of the SAE to their respective IRB. Each site is responsible to report all AE's to their IRB according to its AE reporting policy and procedures.

On behalf of the NIDDK, the Data Coordinating Center will submit an expedited safety report to the FDA for all serious unexpected suspected adverse reactions (SUSARs). That is, when the SAE is unexpected and may be related to the study drug based on evidence of causality. This report will include information on frequency of similar events along with a narrative of similar events to provide context for the individual report. Copies of the expedited safety report will be provided to the PIs, NIDDK, DSMB, and site investigators.

4. When collecting data on participants, adequate safety levels will be set for flagging test results. When these levels are reached, the Data Coordinating Center will notify the appropriate clinic that an abnormal result has been received. Detailed follow-up procedures will be set in the Manual of Operations that will be followed by the clinic when any abnormal results are received.
5. Monthly conference calls will be scheduled for the Steering Committee (SC) and the Trial Coordinators. Subject participation and compliance will be discussed in detail during these calls. A clinical psychology expert in the behavioral and compliance aspects of clinical trials, Dr. William Robiner from the University of Minnesota, will be included in the Trial Coordinator calls when discussing participant compliance issues.
6. A Drug Monitoring Committee (DMC) consisting of the PERL Center Directors and PIs, a research pharmacist, and the Project Manager will discuss any serious medication related problem that a participant has. Changes in study medication dose, medication discontinuation and medication re-institution will be included in these discussions.
7. Twice a year, the Study Group will meet face-to-face with the Data Coordinating Center personnel for a 1½ day meeting to discuss the study in detail and any problems that may have occurred. The Trial Coordinators will hold a separate ½ day meeting with the Data Coordinating Center prior to the SC meeting and any issues needing discussion will be presented at that time and carried from and to the main Study Group meeting for discussion and resolution.

## **15. STUDY ADMINISTRATION**

### **15.1. Organization**

The major organizational components of the study are:

- The *Study Group* is composed of all investigators and study staff from the Clinical Sites, the Data Coordinating Center, and the Central Laboratory. The Study Group is responsible for the conduct of the study.
- The *Steering Committee* is responsible for the design of the study and provides guidance to its execution. Members are the co-Chairs of the PERL Consortium (Drs. Mauer and Doria), the Directors of the Clinical Sites (Drs. Caramori, Rosas, Polsky, Perkins, Pop-Busui, Molitch, Crandall, Rossing, Sigal, Senior, Umpierrez, De Boer, Lingvay, Tuttle, Aronson and Elliott), the Directors of the Data Coordinating Center (Drs. Galecki and Spino), and the Director of the Central Laboratory (Dr. Karger), the NIH program officers (Drs. Jones and Parsa), and the JDRF program officer (Dr. Pragnell).
- The *Executive Committee* will consist of the two PIs, Drs Doria and Mauer, the DCC leaders, Drs Galecki and Spino, the Project Manager, the Lead Clinical Coordinator, and the NIH officers. The EC will have at least monthly conference calls to discuss the overall conduct of the study and set the agendas for the Clinical Coordinators and Steering Committee conference calls. The EC will be responsible for the overall quality of the study, the setting of broad policy directions, and will address major budgetary issues, including, if necessary, reallocation of funds based on developed parameters of need and performance.
- The *Drug Monitoring Committee* is responsible for the oversight of the study drug administration as well as the RAS blocking and antihypertensive therapy during the trial. Members are Dr. Doria, Dr. Mauer, the PIs of the clinical sites, the Project Manager, the Lead Clinical Coordinator, and a research pharmacist. The participation of one of the PIs and 5 of the 16 Center Directors will be sufficient for making decisions.
- The *Clinical Sites* are located at the Joslin Diabetes Center, the University of Minnesota, the University of Colorado (Barbara Davis Center for Childhood Diabetes), the University of Michigan, Northwestern University, Albert Einstein College of Medicine, Washington University (St. Louis), the University of Toronto, the Steno Diabetes Center (Denmark), the University of Calgary (Calgary, Alberta, Canada), University of Alberta (Edmonton, Alberta, Canada), Emory University, the University of Washington (Seattle), University of Texas Southwestern, Providence Medical Research, and BC Diabetes are responsible for recruiting study participants and implementing the protocol.
- The *Data Coordinating Center (DCC)*, based at the University of Michigan, is directed by Drs. Galecki and Spino and is responsible for managing the trial on a day-to-day basis, monitoring enrollment, retention, and protocol adherence and for collecting, monitoring, editing, and analyzing data from the Clinical Sites.
- The *Central Laboratory*, located at the University of Minnesota, is directed by Dr. Karger, and is responsible for all blood and urine tests.
- The *Data Safety Monitoring Board (DSMB)* will be composed of to-be-named outside experts in the design and conduct of clinical trials and in diabetic nephropathy. The board will be responsible for reviewing the study documents, monitoring study progress and participant safety.

Monthly conference calls will be scheduled for the Steering Committee and the trial coordinators to discuss subject participation and compliance. Twice a year, the Steering Committee, the Data Coordinating Center, and the trial coordinators will meet for two days to discuss the study progress. Dr. Robiner, the study psychologist, will attend this meeting annually.

A study website will be maintained where all study meetings and phone call minutes will be maintained and where an updated version of the Manual of Operations will be available.

## **15.2. Protocol Deviations, Violations, and Amendments**

A Protocol Deviation is defined as any change, divergence, or departure from the approved study protocol that does not affect the participant's safety, rights, welfare or the integrity of the study and its resultant data. A Protocol Violation is defined as a protocol deviation that may affect the participant's rights, safety, or wellbeing and/or the completeness, accuracy, and reliability of the study data. Deviation will be reported to the IRB at the time of continuing review whereas violations will be reported as soon as study personnel are aware of the event. The PI will keep an internal protocol deviation and violation log that will be forwarded to the IRB at the time of continuing review. Adoption of protocol amendments will require three-fifths majority approval by members of the Steering Committee. The amended protocol is resubmitted to the IRB.

### 15.3. Financial Disclosure

On an annual basis or whenever there is a significant change in status, participating investigators will be required to disclose any financial or related interest that could present an actual conflict of interest or could be perceived as presenting a conflict of interest. The Steering Committee will determine (1) if the disclosed interest could directly and significantly affect the performance of study responsibility and, (2) the management, reduction, or elimination of the conflict.

### 15.4. Publications

It is anticipated that this research may lead to oral and written presentations including one or more jointly-authored publications. The contribution of investigators will be acknowledged in accordance with scientific custom in all published and oral communications concerning this study and its results.

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**Summary of Changes to PERL Protocols**

**NOTE.** Version 6 of the protocol (dated December 17, 2013) is the first protocol version under which participants were enrolled in the PERL pivotal trial. As such, this version is considered as the “original version” against which subsequent versions are compared. Protocol versions before v6 were for study design purposes only.

#	Location	Change	Rationale
<b>List of changes to Version 7.0 Amendment 2 July 14, 2014</b>			
<b>p.1</b>	Title page	Change version and date Version 7.0 Amendment 2 July 14, 2014	Updates to title page
		Program Officers Teresa Jones, MD Division of Diabetes, Endocrinology, & Metabolic Diseases NIDDK, National Institutes of Health 6707 Democracy Blvd. Room 609 Bethesda, MD 20892-5460 Phone: (301) 435-2996 Fax: (301) 480-3503 <a href="mailto:jonester@mail.nih.gov">jonester@mail.nih.gov</a>	New Project Officer
<b>p.3</b>	General Information	Sponsor for IND #115313 Alessandro Doria, MD PhD MPH Joslin Diabetes Center and Harvard Medical School One Joslin Place Boston, MA 02215 Phone (617) 309-2406 Fax (617) 309-2667 <a href="mailto:alessandro.doria@joslin.harvard.edu">alessandro.doria@joslin.harvard.edu</a>	Identify new Sponsor
<b>p.3</b>	Directors of Clinical Sites	Mount Sinai Hospital Room L5-210, Mail Box 16 60 Murray St. Toronto, ON Canada, M5G 1X5 Phone: (416) 586-8763 Fax: (647) 826-1528 <a href="mailto:bruce.perkins@mtsinai.ca">bruce.perkins@mtsinai.ca</a>	Change of information

#	Location	Change	Rationale
p.3	Directors of Clinical Sites	Rodica Pop-Busui, MD PhD Associate Professor of Internal Medicine Metabolism, Endocrinology and Diabetes University of Michigan 5329 Brehm Tower 1000 Wall Street Ann Arbor MI 48105 Phone: (734) 763-3056 <a href="mailto:rpbusui@umich.edu">rpbusui@umich.edu</a>	Change of information
p.4	Directors of Clinical Sites	Janet McGill, MD Washington University School of Medicine 660 S. Euclid, Campus Box 8127 St. Louis, MO 63110 Phone: (314) 362-8681 Fax: (314) 362-4833 <a href="mailto:hmcgill@dom.wustl.edu">hmcgill@dom.wustl.edu</a>	New Investigator
p.4	Global Clinical Coordinator	Debbie Conboy, RN CDE	Revise listing
p.6	PROTOCOL SUMMARY Inclusion Criteria	Criteria 4: Increase upper age limit to 70 years.  Criteria 5: Increase screening urinary albumin excretion rates [AERs] or albumin creatinine ratios [ACRs] 30-5000 mg/24 hr (20-3333 $\mu$ g/min) or 30-5000 mg/g range, if not on RASB agents OR 18-5000 mg/24 hr (12-3333 $\mu$ g/min) or 18-5000 mg/g range, if on RASB agents  Clarify that creatinine measurement is derived from serum	Age limit raised to include patients diagnosed with T1D at an age older than 35, who may now be in the study based on the previous amendment.  The Steering Committee considered 2500 mg/g overly restrictive.  May increase the eligible

#	Location	Change	Rationale
		Criteria 6: Decrease Estimated GFR (eGFR) based on serum creatinine to between 40 and 99.9 ml/min/1.73 m <sup>2</sup> at screening	population.
p.7.	PROTOCOL SUMMARY Exclusion Criteria	Criteria 9: Increase: SBP>150 or DBP>95 mmHg at the end of the run-in period.	Raising the BP criterion at the end of the run-in period will facilitate retention in the study
p.8	PROTOCOL SUMMARY Study Duration	Increase: acceptable BP 140/90 mmHg	
p.8	PROTOCOL SUMMARY Date of Protocol	Change: July 7, 2014	
p.12	4. PARTICIPATING CENTERS	Increase: in number of centers to nine Add: Washington University (St. Louis, MO)	New site assist to enhance recruitment
p.13.	4. PARTICIPATING CENTERS 4.1 Location of study visits	Delete: (Visit 2 and all the visits including an iohexol-GFR measurement, i.e., V4, V11, V16, V17, will always be done “In-person”). Delete: Study procedures that require physical interactions (e.g., BP measurement) will be carried out at PCPs’ offices, health care facilities, and/or clinical laboratories close to where participants live (hereby referred to as “Remote Visits”)  Insert: Blood draws and urine collections scheduled at the time of Phone Visits will be performed at local facilities close to where participants live (hereby referred to as “Remote Biospecimen Collections”). For any given study visit to be conducted remotely, a Phone Visit and a Remote Biospecimen Collection will be both required.  Delete: For any given study visit to be conducted remotely, a Phone Visit and a Remote Visit will be both required  Insert: Phone Visits and Remote Biospecimen Collections  Insert: Study Site  Delete: for these Remote Visits.	

#	Location	Change	Rationale
p.14	<p>4. PARTICIPATING CENTERS</p> <p>4.1 Location of study visits</p>	<p>Insert: Following a Phone Visit, participants may be invited to an In-Person Visit at the Study Site, at their PCP's office, or at other local healthcare facilities if procedures that require physical interactions are deemed to be necessary (e.g., BP measurement to confirm the self-report of elevated BP values, physical exam to confirm the self report of skin rash). Sites for remote in-person visits will be chosen by the Study Site based on the participant's preference, logistic and financial considerations, and site's qualifications. Study personnel will discuss study requirements with the remote site health providers and operators and will provided with written instructions on how to carry out the procedures that will be conducted at their locations and report the results to the Study Site.</p>	<p>Revised structure of Remote Visits</p>
p.14	<p>4. PARTICIPATING CENTERS</p> <p>4.1 Location of study visits</p>	<p>Delete: Remote Visits text as follows</p> <p>Remote Visits will be scheduled as close as possible to the corresponding Phone Visit and within the same time window as outlined in Section 8.1</p> <p>Remote Visits will take place only after a signed copy of the ICF has been received by the study site (see Point #2 under Phone Visits).</p> <p>Sites for Remote Visits will be chosen by the Study Site based on the participant's preference, logistic and financial considerations, and site's qualifications. More than one remote site may be selected for a given visit if different procedures must be carried out at different facilities (e.g., if the PCP office and Clinical Laboratory are not part of the same structure or institution).</p> <p>Study personnel will discuss study requirements with the remote site health providers and operators and will provided be with written instructions on how to carry out the study procedures that will be conducted at their locations. Procedures that may be carried out at the remote site (PCP's office, other healthcare facilities, clinical laboratories) will include:</p> <p>Weight and height measurements.</p> <p>Heart rate and blood pressure measurements.</p> <p>ECG.</p> <p>Physical exam.</p> <p>Skin assessment.</p> <p>Blood draws for local lab tests (serum creatinine, K, and ALT, CBC, pregnancy tests).</p>	<p>Revised structure of Remote Visits</p>

#	Location	Change	Rationale
		<p>Blood draw for central lab tests (serum creatinine, Cystatin C, uric acid, and HbA1c).</p> <p>Collection of urine samples from first morning voids (Visit 1) or overnight collections (Visits 3, 9, 13, and 15).</p> <p>Data (#5a-5e above) will be collected on forms provided by the Study Site and will be transmitted back to the Site by fax or other secure methods</p>	
<b>p.14</b>	<p>4. PARTICIPATING CENTERS</p> <p>4.1 Location of study visits</p>	<p>Insert: Remote Biospecimen Collections</p> <p>Local sites for Remote Biospecimen Collections will be chosen by the Study Site based on the participant's preference, logistic and financial considerations, and site's qualifications.</p>	<p>Revised procedure for Remote Visits specimen collection, handling and shipping.</p>
<b>p.15</b>	<p>4. PARTICIPATING CENTERS</p> <p>4.1 Location of study visits</p>	<p>Revise: text related to Remote Biospecimen Collection as follows</p> <p>Specific instructions will be provided for presentation to the local lab sites for specimen collection, handling and tube labeling for specimens requiring shipment to the Study Main site Central Laboratory. Pre-addressed shipping containers will also be provided for these Remote Visits along with a labeled an inventory sheet for each shipping container will also be provided faxing to the Study Main Site or Central Laboratory and inclusion with the shipment.</p> <p>Blood samples for local lab tests (serum creatinine, K, and ALT, CBC, pregnancy tests) will be processed and analyzed at the remote site facilities where samples are collected or shipped to commercial laboratories for testing. Results will be transmitted to the Study Site by fax or other secure methods.</p> <p>Blood and urine samples for central lab tests (serum creatinine, Cystatin C, uric acid, HbA1c, urinary ACR and AER) will be mailed by the Remote Site to the Central Lab a commercial lab or to the Main Study Site where they will be processed, aliquoted, and forwarded to the Central Lab. Blood tubes and urine containers will be provided by the Study Site.</p>	<p>Revised procedure for specimen of Remote Visits collection, handling, shipping.</p>
<b>p.15</b>	<p>5. SUBJECT SELECTION Inclusion Criteria</p>	<p>Text revised to be consistent with changes identified in the Protocol Summary Subject Population Inclusion Criteria</p>	<p>Consistency</p>
<b>p.16</b>	<p>5. SUBJECT SELECTION Exclusion Criteria</p>	<p>Text revised to be consistent with changes identified in the Protocol Summary Subject Population Exclusion Criteria</p>	<p>Consistency</p>
<b>p.20</b>	<p>7. STUDY OUTCOMES</p>	<p>Correct spelling: insulin</p>	

#	Location	Change	Rationale
p.23-24	7.1. Primary outcome 8. STUDY PROCEDURES Figure 1. Schedule of Events	Revised to reflect protocol changes in study evaluations and footnotes	The schedule has been revised to incorporate the protocol changes.
p.25	8. STUDY PROCEDURES 8.2 Screening and Enrollment in the Run-in Period (Visit 1)	Revised text: Subjects who have a confirmed history of micro- or macroalbuminuria (at least two out of three consecutive urinary AER or ACR in micro- or macroalbuminuria range as defined in Section 5.1) will not need to bring a sample of urine to Visit 1 (if the evidence of micro- or macroalbuminuria dates back to more than two years before screening, evidence of ongoing GFR decline should be gathered, see Section 5.1). Subjects who have incomplete evidence of micro- or macroalbuminuria (one of the last two urinary AER or ACR in the micro- or macroalbuminuria range),	
p.25-26	8. STUDY PROCEDURES 8.3 Screening and Enrollment in the Run-in Period (Visits 2,3,4)	RAS antagonist treatment will be standardized, and BP, if elevated (>1430/90 mm Hg), normalized. Letters will be written to the participants' physicians informing them about the study and notifying them of the study's protocol RAS blocker requirements and blood pressure goals. The letter will propose active participation of the participants' physicians in blood pressure management with the availability of advice from the PERL site physicians and, if needed, the PERL Drug monitoring Committee for out of range blood pressure values during the course of the study. Letters will be written to the participants' physicians informing them about the study and notifying them that study physicians will be assuming control of the participants' antihypertensive therapy. The run-in period will start at Visit 2. If a participant is already on a RAS Blocker, its dose will be increased adjusted, if necessary, to make it at least equivalent to ramipril 10 mg (if on ACE inhibitor [ACEI]) or irbesartan 300 mg (if on an angiotensin receptor blocker [ARB]), if acceptable to the patient's primary physician, if tolerated and if not contraindicated (see below). Participants who were not taking a RAS Blocker will be prescribed and instructed to start taking 10 mg of ramipril daily or 300 mg of irbesartan daily (if ramipril is contraindicated or has side effects) or another ACE inhibitor or ARB at equivalent doses if there are impediments to the use of ramipril or irbesartan. Participants who have contraindications to RAS blockers (e.g.,	Antihypertensive therapy should now be managed jointly by the study physicians and the participants' personal physicians.



#	Location	Change	Rationale
		SBP<100 mmHg, K+>5.5 mEq) or are normoalbuminuric and normotensive and are not currently treated with RAS blockers, will not be treated with these drugs, as this represents the standard of care.	
p26-27	8. STUDY PROCEDURES 8.3 Screening and Enrollment in the Run-in Period (Visits 2,3,4)	<p>Revised text: If BP is found to be elevated (&gt;1430/980 mm Hg) on three consecutive occasions, the dosage of existing non-RAS antagonists antihypertensive drugs will be maximized, followed, if necessary, by the introduction of antihypertensive drugs of a different class. These will be chosen by the study site physicians in collaboration with the other health care providers that are involved in managing the participant's anti-hypertensive therapy. from a restricted menu of approved medications at recommended dosage, following the general protocol that was used in RASS33. The same menu and protocol will be used to start antihypertensive treatment in participants who have persistently high BP levels and were not on antihypertensive therapy prior to study entry. If the goal of BP (&gt;1430/980 mm Hg) is not achieved with these drugs, a Drug Monitoring Committee conference call will be convened to consider the possibility of causes of hypertension other than diabetic nephropathy and discuss alternative therapeutic approaches. BP will continue to be monitored and the anti-hypertensive therapy to be adjusted in a similar way throughout the study.</p> <p>Revised Visit Procedures list: After 2 weeks of run-in, participants will come in for Visit 3 during which they will undergo the following procedures: Review RASB and adjust BP therapy. Measure weight and vital signs. After 6 weeks of run-in, participants will come in for Visit 4 during which they will undergo the following procedures: Review RASB and adjust BP therapy If normal blood pressure control is not achieved at Visit 4, the run-in period may be extended for two more weeks after which participants will be examined as in Visit 4 (Visit 4A). In this event, the GFR measurement scheduled for Visit 4 will be conducted at Visit 4A. Participants whose SBP is &gt;15040 or whose DBP is &gt;9590 mmHg at the end of the run-in period will be discontinued from the study (prior to randomization).</p>	<p>Antihypertensive therapy should now be managed jointly by the study physicians and the participants' personal physicians.</p> <p>The procedure schedule has been revised to incorporate the protocol changes.</p>

#	Location	Change	Rationale
p.28	8. STUDY PROCEDURES 8.5 Treatment Period (Visits 6-15)	Revise procedures list: Obtain interval medical history (with special emphasis on BP control and CVD events). Review of concomitant medications and AEs. Review and adjust RASB and BP therapy. Measure height, (Visits 7 and 11) weight, and vital signs according to the schedule outlined in Figure 1 Inspect for skin rash (Visit 11)	The procedure schedule has been revised to incorporate the protocol changes.
p.28	8. STUDY PROCEDURES 8.6 End of Intervention (Visit 16)	Revise procedure list: Review RASB and adjust BP therapy.	The procedure schedule has been revised to incorporate the protocol changes.
p.30	9. SAFETY ASSESSMENTS 9.2 Skin Exam	Revised text: At each visit study on and after Visit 4, The skin of study participants will be examined for the presence of any kind of rash at each in-person visit. Participants will be instructed to carry-out periodical skin self-exams. If skin abnormalities are reported to the study personnel during the phone visits or on any other occasion, participants will be asked to immediately report to the study site, their PCP's office, or other local healthcare facilities for an in-person skin exam.	Reflect change in remote visit schedule.
p.30	9. SAFETY ASSESSMENTS 9.3 Vital Signs	Revised text: Blood pressure and heart rate will be recorded at each in-person visit. BP readings at home will be reviewed during each phone visits; if abnormal values are reported, participants will be asked to visit the study site, their PCP's office, or other local healthcare facilities to have their BP measured	Antihypertensive therapy should now be managed jointly by the study physicians and the participants' personal physicians
p.42	13. PROTECTION OF HUMAN SUBJECTS 13.5.b.k. Procedures for protecting against and minimizing potential risks b. Specific	Revised text: Blood pressure will be measured quarterly with the goal of maintaining BP $\leq 130/140$ mmHg systolic and $\leq 85/90$ mmHg diastolic. If elevated, a recheck will be performed within 2 weeks and if still elevated additional antihypertensive non-RAS blockers will be added in collaboration with the participants' physicians from a limited menu of agents as prescribed in the MOO. Failure to achieve satisfactory BP control within 2 months would lead to a Drug Monitoring Committee conference call. The patient's physicians would be asked to relinquish BP management to PERL personnel in order to achieve uniformity of goals, but the patient's physicians would be informed of any BP	Reflects change related to acceptable BP level

#	Location	Change	Rationale
		medication changes.	
<b>List of Changes in PERL Protocol Version 8.0 November 18, 2014</b>			
p.1	Title page	Change version and date <b>Version 8.0 Approved by the DSMB November 18, 2014</b>	Version date changed to date of DSMB approval
p.2-4	Table of Contents	Page numbers 8-49 changed	Page numbers shifted.
p.7	Directors of Clinical Sites	Ronald J. Sigal, MD, MPH Cumming School of Medicine, University of Calgary 1820 Richmond Road SW Calgary, Alberta, Canada T2T 5C7 Phone: (403) 955-8327 Fax: (403) 955-8249 <a href="mailto:rsigal@ucalgary.ca">rsigal@ucalgary.ca</a>	New Investigator
p.7	Directors of Clinical Sites	Peter Senior, MD Alberta Diabetes Institute 2-004 Li Ka Shing Center for Health Research Innovation Edmonton, Alberta, Canada T6G2E1 Phone: (780) 407-1480 Fax: (780) 492-9555 <a href="mailto:petersenior@ualberta.ca">petersenior@ualberta.ca</a>	New Investigator
p.7-8	Directors of Clinical Sites	Guillermo E. Umpierrez, MD Emory University School of Medicine 101 Wodruff Circle, 1 <sup>st</sup> Floor, RM 1311 Atlanta, GA 30322 Phone: (404) 778-1663 <a href="mailto:geumpie@emory.edu">geumpie@emory.edu</a>	New Investigator
p.8	Directors of Clinical Sites	Irl B. Hirsch, MD University of Washington Medical Center – Roosevelt 4245 Roosevelt Way, NE, 3 <sup>rd</sup> floor, Box 354691 Seattle, WA 98105 Phone: (206) 598-4884 Fax: (202) 598-4976 <a href="mailto:ihirsch@uw.edu">ihirsch@uw.edu</a>	New Investigator
p.9	<b>PROTOCOL SUMMARY</b> Subject Population Inclusion/Exclusion Criteria	<i>Revised Text:</i> The upper <b>and the lower limits</b> should be decreased by 1 ml/min/1.73 m <sup>2</sup> for each year over age 60 (with a lower limit of 35 ml/min/1.73m <sup>2</sup> ) and by 10 ml/min/1.73 m <sup>2</sup> for strict vegans.	Change in Inclusion criteria to reflect changes of eGFR with age

#	Location	Change	Rationale
p.11	<b>PROTOCOL SUMMARY</b> Study Duration	<i>Revised Text:</i> 9-week run-in period, during which RAS inhibition will be introduced and/or standardized, <b>if indicated</b> , and BP normalized, if elevated above 140/90 mmHg, followed by a 3-year treatment period and then by a 2-month wash-out period.	Modification of visit schedule to acknowledge that some patients enter without the need for RAS treatment
p.11	<b>PROTOCOL SUMMARY</b> Date of Protocol	<i>Revised Text:</i> November 18, 2014	Date approved by DSMB
p.16	Participating centers	<i>New Text:</i> University of Calgary (Calgary, Alberta, Canada) Alberta Diabetes Institute (Edmonton, Alberta, Canada) Emory University (Atlanta) University of Washington Medical Center (Seattle)	New sites added to enhance recruitment
p.18	<b>5. SUBJECT SELECTION</b> Inclusion Criteria	<i>Revised Text:</i> The upper <b>and the lower limits</b> should be decreased by 1 ml/min/1.73 m <sup>2</sup> for each year over age 60 (with a lower limit of 35 ml/min/1.73m <sup>2</sup> ) and by 10 ml/min/1.73 m <sup>2</sup> for strict vegans.	Change in inclusion criteria to reflect changes of eGFR with age
p.22	<b>6. STUDY TREATMENTS</b> 6.1.2 Dosage	<i>New text:</i> 4. At some sites the study medication may be dispensed directly to the study participant at a relevant in person study visit or by mail from the site following a relevant in-person or phone study visit.	Change in study medication dispensing
p.26	<b>8. STUDY PROCEDURES</b> Figure 1. Schedule of Events	<i>Revised Figure 1:</i> To reflect protocol changes in study evaluations and footnotes	The schedule has been revised to incorporate the protocol changes.
p.27	<b>8. STUDY PROCEDURES</b> 8.3 Run-in Period (visits 2, 3 and 4)	<i>Revised Text:</i> Starting at Visit 2, eligible subjects who agree to participate in the study will enter a run-in period of 9 weeks ( <b>see note at the end of this section for exceptions to this duration</b> ). During this visit, subjects will undergo the following procedures:	Reference to modification of number of run-in study visits and consequent duration of run-in period
p.28	<b>8. STUDY PROCEDURES</b> 8.3 Run-in Period (visits 2, 3 and 4)	<i>Revised Text:</i> RAS antagonist treatment will be standardized, and BP, if elevated (>140/90 mm Hg), normalized. Letters will be written to the participants' physicians informing them about the study and notifying them of the study's protocol RAS blocker requirements and blood pressure goals. The letter will propose active participation of the	Further clarify the use of anti-hypertensive therapy

#	Location	Change	Rationale
		<p>participants' physicians in blood pressure management with the availability of advice from the PERL site physicians and, if needed, the PERL Drug monitoring Committee for out of range blood pressure values during the course of the study. The run-in period will start at Visit 2. If a participant is already on a RAS Blocker, its dose will be increased, if necessary, to make it at least equivalent to ramipril 10 mg (if on ACE inhibitor [ACEI]) or irbesartan 300 mg (if on an angiotensin receptor blocker [ARB]), if acceptable to the patient's primary physician, if tolerated and if not contraindicated (see below). Participants who were not taking a RAS Blocker will be prescribed and instructed to start taking 10 mg of ramipril daily or 300 mg of irbesartan daily (if ramipril is contraindicated or has side effects) or another ACE inhibitor or ARB at equivalent doses if there are impediments to the use of ramipril or irbesartan. Participants who have contraindications to RAS blockers (e.g., SBP&lt;100 mmHg, K+&gt;5.5 mEq) <b>or do not have evidence or history of micro- or macroalbuminuria (as defined in 5.1.4), are normotensive, and are not being treated with RASB or other anti-hypertensive agents</b> will not be treated with these drugs, as this represents the standard of care.</p>	
p.28	<p><b>8. STUDY PROCEDURES</b> 8.3 Run-in Period (visits 2, 3 and 4)</p>	<p><i>Revised Text:</i> After 2 weeks of run-in, participants will come in for <b>Visit 3</b> during which they will undergo the following procedures:  <ul style="list-style-type: none"> <li>Obtain interval medical history (with special emphasis on CVD events).</li> <li>Review concomitant medications and AEs</li> <li>Review RASB and BP therapy.</li> <li>Collect samples for clinical laboratory assessments as outlined in Figure 1.</li> <li>Perform pregnancy test in women of childbearing potential.</li> <li>Be provided with a container and instructions for an overnight urine collection to be made immediately before Visit 4.</li> </ul> <i>Delete Text:</i> (including HLA B*58.01) in collection of samples for clinical laboratory assessments procedures list</p>	<p>Modification of visit procedure list, to exclude collection of HLA B*58.01 at this visit</p>
p.29	<p><b>8. STUDY PROCEDURES</b> 8.3 Run-in Period (visits 2, 3 and 4)</p>	<p><i>Revised Text:</i> After 6 weeks of run-in, participants will come in for Visit 4 during which they will undergo the following procedures:  <ul style="list-style-type: none"> <li>• Obtain interval medical history (with special emphasis on CVD events).</li> <li>• Conduct a physical exam (if deemed to be required by the study physician)</li> <li>• Review concomitant medications and AEs</li> <li>• Review BP therapy.</li> </ul> </p>	<p>Modification of visit procedure list to include collection of HLA B*58.01</p>

#	Location	Change	Rationale
		<ul style="list-style-type: none"> <li>• Review the fetal risks of RAS blockade.</li> <li>• Measure height, weight and vital signs.</li> <li>• Perform ECG.</li> <li>• Collect samples for clinical laboratory assessments (<b>including HLA B*58:01</b>) as outlined in Figure 1.</li> <li>• Perform pregnancy test in women of childbearing potential.</li> <li>• Measure iohexol GFR.</li> </ul>	
p.29	<b>8. STUDY PROCEDURES</b> 8.3 Run-in Period (visits 2, 3 and 4)	<p><i>New Text:</i></p> <p>If normal blood pressure control is not achieved at Visit 4, the run-in period may be extended for two more weeks after which participants will be examined as in Visit 4 (Visit 4A). In this event, the GFR measurement scheduled for Visit 4 will be conducted at Visit 4A. Participants whose SBP is &gt;150 or whose DBP is &gt;95 mmHg at the end of the run-in period will be discontinued from the study (prior to randomization).</p> <p><b>IMPORTANT: Visit 2 and Visit 3 can be skipped, i.e., a participant can move directly from Visit 1 to Visit 4, if the following criteria are met at Visit 1:</b>  <b>The participant is eligible based on the results of Visit 1 assessments, including laboratory values;</b>  <b>Blood pressure is &lt;140/90 mmHg;</b>  <b>AND</b>  <b>The participant meets <u>one</u> of the following criteria:</b>  <b>Has been treated with a RASB for at least two months at a dose at least equivalent to Ramipril 10 mg or Irbesartan 300 mg;</b>  <b>Has contraindications to RASB;</b>  <b>Does not have evidence or history of micro- or macroalbuminuria (as defined in 5.1.4) and is not being treated with RASB or other anti-hypertensive agents.</b>  <b>If the above criteria are met and Visits 2 and 3 are skipped, Visit 4 will be scheduled 3 weeks after Visit 1 with a window of 2 weeks before and 3 weeks after the target date. The collection of medical history and the physical exam scheduled at Visit 2 will be conducted at Visit 4.</b></p>	Modification of visit schedule to reduce number of run-in study visits and consequent duration of run-in period for participants who qualify
p.29	<b>8. STUDY PROCEDURES</b> 8.4. Enrollment in the Study and Randomization (Visit 5)	<p><i>Revised Text:</i></p> <ul style="list-style-type: none"> <li>• At the end of the run-in period, eligibility will be re-assessed based on the BP measures obtained at Visits 4 or 4A (if applicable), HLA-based genetic susceptibility to allopurinol skin reactions<sup>36,37</sup> (<b>tested at Visit 4</b>), and a valid baseline iGFR measurement. Participants who are eligible for randomization based on those measures (SBP ≤ 150 and DBP ≤ 95 mmHg) and a negative HLA B*58:01 test will be</li> </ul>	Modification of visit schedule

#	Location	Change	Rationale
		<p>telephoned by the study coordinator to discuss how the study medication should be taken and its potential side effects.</p> <p><i>Deleted Text:</i> (tested at vVisit 3)</p>	
P.30	<p><b>8. STUDY PROCEDURES</b> 8.5. Treatment Period (Visits 6 to 15)</p>	<p><i>Revised Text:</i> During the treatment period, the following procedures will be completed at each visit for each participant: Obtain interval medical history (with special emphasis on BP control and CVD events). Review of concomitant medications and AEs. Review RASB and BP therapy. Measure height, weight, and vital signs according to the schedule outlined in Figure 1 Inspect for skin rash. (<del>Visit 11</del>) Conduct a physical exam (Visit 11). Perform ECG according to the schedule outlined in Figure 1 (Visit 11). Collect samples for clinical laboratory assessments and for storage of serum, plasma and urine for later biomarker research according to the schedule outlined in Figure 1. Perform pregnancy test in women of childbearing potential. Measure GFR by means of plasma disappearance of non-radioactive iohexol, iGFR at Visit 11. Provide a container and instructions for an overnight urine collection whenever an AER measurement is scheduled at the following visit.</p>	Modification of visit procedure list
P.30	<p><b>8. STUDY PROCEDURES</b> 8.5. Treatment Period (Visits 6 to 15)</p>	<p><i>New Text:</i> <b>At some sites the study medication may be dispensed directly to the study participant at a relevant in person study visit or by mail from the site following a relevant in-person or phone study visit.</b></p>	Modification of study drug dispensing
P.33	<p><b>10. ADVERSE EVENT REPORTING</b> 10.1. Definitions</p>	<p><i>New Text:</i> An Adverse Event (AE) is any untoward medical occurrence in a study participant regardless of its relationship to study treatment. A treatment-emergent AE is an adverse event occurring during the period between the first dose and 30 days after the final dose of the study medication. A Serious Adverse Event (SAE) is any untoward medical occurrence that results in death, is life-threatening, requires hospitalization or prolongation of an existing hospitalization, results in persistent or significant disability,</p>	Clarification of the AE reporting period

#	Location	Change	Rationale
		<p>or is a congenital anomaly/birth defect. Important medical events that do not fall into the above categories may also be considered an SAE when, based on medical judgment, such events may jeopardize the patient's safety and require medical/surgical intervention to prevent one of the outcomes listed in the SAE definition. The term SAE is not intended as a measure of severity or intensity. <b>All AE's/SAE's that occur after the time of informed consent will be reported.</b></p>	
P.33	<p><b>10. ADVERSE EVENT REPORTING</b> 10.2 Adverse Event Reporting</p>	<p><i>Revised Text:</i> All AEs will be reported on the Adverse Events form that will be completed by the study staff, who are masked as to study treatment assignment, at each regular follow-up visits. This will insure that AEs are ascertained in an unbiased manner using the same standardized methodology for participants in both treatment arms. Forms will include standardized questions relating to specific events of import in diabetic patients on either of the study treatment arms as well as any significantly abnormal physical finding identified on examination and any significantly abnormal laboratory results obtained on the patient between visits or at the time of the visit. AEs reported or ascertained between clinic visits will be captured and reported at the time of the next schedule visit. Pre-existing conditions (that is, <b>any conditions that was known to be present prior to the signing of informed consent or was identified during the screening procedures at Visit 1</b>) will not be considered or recorded as AEs unless the condition worsens in intensity or frequency after <b>Visit 1</b>. Likewise, continuing AEs will not be reported as AEs at subsequent visits unless they increase in severity or frequency between visits, they results in criteria for a SAEs, and/or they resolve between visits. Each site will be responsible for reporting all AE's to their IRB according to its AE reporting policy and procedures</p>	Clarification of AE's
p.46	<p><b>14. DATA AND SAFETY MONITORING PLAN</b> 2. IRB Monitoring</p>	<p><i>New Text:</i></p> <ul style="list-style-type: none"> <li>• Joslin Diabetes Center</li> <li>• University of Minnesota</li> <li>• University of Colorado</li> <li>• University of Michigan</li> <li>• Northwestern University</li> <li>• University of Toronto</li> <li>• Albert Einstein University</li> <li>• Washington University</li> <li>• Steno Diabetes Center</li> <li>• <b>University of Calgary</b></li> <li>• <b>Alberta Diabetes Institute</b></li> <li>• <b>Emory University</b></li> <li>• <b>University of Washington</b></li> </ul>	New clinical sites



#	Location	Change	Rationale
p.47	<b>15. STUDY ADMINISTRATION</b> 15.1. Organization	<p><i>Revised Text:</i>            The Steering Committee is responsible for the design of the study and provides guidance to its execution. Members are the co-Chairs of the PERL Consortium (Drs. Maurer and Doria), the Directors of the Clinical Sites (Drs. Caramori, Goldfine, Maahs, Perkins, Pop-Busui, Molitch, Crandall, Rossing, <b>Sigal, Senior, Umpierrez and Hirsh</b>), the Directors of the Data Coordinating Center (Drs. Galecki and Spino), and the Director of the Central Laboratory (Dr. Eckfeldt), the NIH program officers (Drs. Jones and Flessner), and the JDRF program officer (Dr. Nickerson).</p> <p><i>Revised Text:</i>            The <i>Clinical Sites</i> are located at the Joslin Diabetes Center, the University of Minnesota, the University of Colorado (Barbara Davis Center for Childhood Diabetes), the University of Michigan, Northwestern University, Albert Einstein College of Medicine, Washington University (<b>St. Louis</b>), the University of Toronto the Steno Diabetes Center (Denmark), <b>the University of Calgary (Calgary, Alberta, Canada), Alberta Diabetes Institute (Edmonton, Alberta, Canada), Emory University and the University of Washington Medical Center (Seattle)</b> are responsible for recruiting study participants and implementing the protocol.</p>	New site directors added  New clinical sites added
<b>List of Changes in PERL Protocol Version 9.0 August 16, 2016</b>			
p.1	Title page	Change version and date <b>Version 9.0 Approved by the DSMB August 16, 2016</b>	Version date changed to date of DSMB approval
p.2-4	Table of Contents	Page numbers 8-49 changed	Page numbers shifted.
p.5	Program Officers	<p><i>Revised Text:</i>  <b>Marlon Pragnell, PhD</b>            Senior <b>Scientist, Translational Development</b>            Juvenile Diabetes Research Foundation            26 Broadway            New York, NY 10004            Phone: (212) 479-7690            Fax: (212) 785-9609  <u><a href="mailto:mpragnell@jdrf.org">mpragnell@jdrf.org</a></u></p> <p><i>Deleted Text:</i>            Helen Nickerson            Scientific Program Manager: Complications            Hnicjerson</p>	New Program Officer

#	Location	Change	Rationale
p.6	Directors of Clinical Sites	<p><i>Revised Text:</i>  <b>Sarit Polsky, MD, MPH</b>  Barbara Davis Center for Diabetes  University of Colorado at Denver - Anschutz Medical Campus  1775 Aurora Court, Mail Stop A140  Aurora, CO 80045 P  Phone: (303) 724-8575  Fax: (303) 724-6784  <a href="mailto:sarit.polsky@ucdenver.edu">sarit.polsky@ucdenver.edu</a></p> <p><i>Deleted Text:</i>  David Maahs PhD  david.maahs@ucdenver.edu</p>	New Investigator
p.6-7	Directors of Clinical Sites	<p><i>New Text:</i>  <b>Ronnie Aronson, MD</b>  LMC Diabetes and Endocrinology  1929 Bayview Avenue, Suite 107  Toronto, ON Canada M4G 3E8  Ph: (416) 646-2929  Fax: (416) 645-2930  Email: <a href="mailto:Ronnie.Aronson@LMC.CA">Ronnie.Aronson@LMC.CA</a></p>	Additional Investigator
p.8	Directors of Clinical Sites	<p><i>New Text:</i>  <b>Ildiko Lingvay, MD, MPH, MSCS</b>  University of Texas Southwestern Medical Center  5323 Harry Hines Blvd., U9.134E  Dallas, TX 75390-9302  Phone: (214) 648-2779  Fax: (214) 648-2885  <a href="mailto:ildiko.lingvay@utsouthwestern.edu">ildiko.lingvay@utsouthwestern.edu</a></p>	New Investigator
p.8	Directors of Clinical Sites	<p><i>New Text:</i>  <b>Katherine R. Tuttle, MD</b>  Providence Medical Research Center  104 W. 5th Avenue, Suite 350E  Spokane, WA 99204  Phone: (509) 474-4345  Fax: (509) 474-4325  <a href="mailto:katherine.tuttle@providence.org">katherine.tuttle@providence.org</a></p>	New Investigator

#	Location	Change	Rationale
p.8	Directors of Clinical Sites	<i>New Text:</i> <b>Tom Elliott MBBS, FRCPC</b> BC Diabetes 4102-2775 Laurel Street Vancouver, BC V5Z 1M9 Phone: (604) 683-3734 ext. 1001 Fax: (604) 628-3821 <a href="mailto:telliott@bcdiabetes.ca">telliott@bcdiabetes.ca</a>	New Investigator
p.9	Central Laboratory	<i>Revised Text:</i> <b>Advanced Research and Diagnostics Laboratory</b> Director: <b>Amy Karger, MD, PhD</b> University of Minnesota Mayo Mail Code 609, Room Mayo D211 420 Delaware Street, SE Minneapolis, MN 55455 Ph: (612) 624-2150 Fax: (612) 625-1121 <a href="mailto:karge026@umn.edu">karge026@umn.edu</a>  <i>Deleted Text:</i> John Eckfeldt	New Director
p.10	<b>PROTOCOL SUMMARY</b> Participating centers	<i>New Text:</i> University of Washington (Seattle) University of Calgary (Calgary) University of Alberta (Edmonton) Emory University (Atlanta) Washington University (St. Louis) University of Texas Southwestern (Dallas) Providence Medical Research Center (Spokane) BC Diabetes (Vancouver).	New sites added to enhance recruitment
p.12	<b>PROTOCOL SUMMARY</b> Statistical methods	<i>Revised Text:</i> <b>The majority of data analyses, including the primary analysis, will be performed according to an intention-to-treat approach.</b>	Change in the analysis plan
p.12	<b>PROTOCOL SUMMARY</b> Date of Protocol	<i>Revised Text:</i> July 13, 2016	Date approved by DSMB

#	Location	Change	Rationale
p.15	<b>1. INTRODUCTION</b>	<i>Revised Text:</i> It is very important to note that, in that study, the UA levels shortly after the onset of T1D was a significant independent predictor of macroalbuminuria 18 years later (hazard ratio 1.90 per mg/dl increase in UA level; p=0.04)11, this <b>being</b> suggestive of a pathogenetic role.	Grammar clarification
p.16	<b>1. INTRODUCTION</b>	<i>New text:</i> To test this hypothesis, we have established a consortium of investigators from academic centers where large rosters of T1D patients are available along with long-standing expertise in the study of diabetic complications, especially DN, and in DN clinical trials. Included in this initiative are the Joslin Diabetes Center, the Universities of Minnesota, Colorado, Toronto, Michigan, <b>Washington (Seattle), Texas Southwestern, Calgary, and Alberta</b> Northwestern University, <b>Washington University (St. Louis), Emory University</b> , Albert Einstein College of Medicine, <b>BC Diabetes, Providence Medical Research Center</b> , and the Steno Diabetes Center in Copenhagen, Denmark. The Consortium, led by Dr. Alessandro Doria from the Joslin Kidney Study, and by Dr. Michael Mauer, who recently led the Renin Angiotensin System Study (RASS) clinical trial, has been named PERL (Preventing Early Renal Function Loss in Diabetes) to emphasize the Consortium's focus on intervening early in the course of kidney disease, when renal damage is most likely to be able to be arrested or reversed and interventions are more likely to be effective.	Addition of new centers
p.16-17	<b>4. PARTICIPATING CENTERS</b>	<i>Revised /New Text:</i> The study will involve <b>16</b> centers that are part of the PERL Consortium: <ul style="list-style-type: none"> <li>Joslin Diabetes Center (Boston)</li> <li>University of Minnesota (Minneapolis)</li> <li>University of Colorado (Barbara Davis Center for Childhood Diabetes, Denver)</li> <li>University of Michigan (Ann Arbor)</li> <li>University of Toronto (Toronto)</li> <li>Northwestern University (Chicago)</li> <li>Albert Einstein College of Medicine (New York)</li> <li>Steno Diabetes Center (Copenhagen, Denmark)</li> <li>Washington University (St. Louis, MO)</li> <li>University of Calgary (Calgary, Alberta, Canada)</li> <li><b>University of Alberta (Edmonton, Alberta, Canada)</b></li> <li>Emory University (Atlanta)</li> <li>University of Washington (<b>Seattle</b>)</li> <li><b>University of Texas Soutwestern (Dallas)</b></li> <li><b>Providence Medical Research Center (Spokane)</b></li> </ul>	This section has been revised to update the number of sites, add new sites and clarification of the name of an existing center for consistency

#	Location	Change	Rationale
p.18	<b>4. PARTICIPATING CENTERS</b> 4.1 Location of Study Visits	<b>BC Diabetes (Vancouver)</b>  <i>New Text:</i> <ul style="list-style-type: none"> <li>Blood samples for local lab tests (serum creatinine, K, and ALT, CBC, pregnancy tests) will be processed and analyzed at the facilities where samples are collected or shipped to commercial laboratories <b>or to the Central Laboratory</b> for testing. Results will be transmitted to the Study Site by fax or other secure methods.</li> </ul>	Reflects the ability of the Central Laboratory to analyze remote local lab specimens, in the event a lab in the subject's local area is unable to be identified.
p.21	<b>5. SUBJECT SELECTION</b> 5.5.1 Reasons for discontinuation	<i>Revised Text:</i> The study drug will be <i>temporarily</i> discontinued if a participant: Has clinically significant persistent changes from baseline based on laboratory safety assessment results (the response to discontinuation will be monitored to assess whether the drug can be re-instituted, see next paragraph on permanent discontinuations). Requires treatment with allopurinol or medications that make allopurinol contraindicated (see 5.5.2 and 9.5). Becomes pregnant or breastfeeding (see 5.5.2) <b>Whenever the reason for temporary discontinuation of the study drug ceases to exist, the study medication will be resumed with the consensus of the drug monitoring committee, according to the following procedures:</b> <b>If the study medication was discontinued because of a suspected drug reaction or the participant was off-medication for 3 months or longer, the study drug will be re-started at a dosage of 100 mg for 4 weeks, which will then be increased to the full dosage appropriate for the eGFR. (see 6.1.2)</b> <b>If the study medication was not discontinued because of a drug reaction and the participant was off-medication for less than 3 months, the study medication will be re-started, at the full dosage appropriate for the eGFR.</b> The study drug will be <i>permanently</i> discontinued if a participant: Experiences an SAE related to the study drug or an intolerable AE such as a persistent allergy or rash. Has clinically significant persistent changes from baseline based on laboratory safety assessment results which do not respond to temporary 2-week discontinuation of study drug and re-institution of drug at ½ of the initial dose. Develops end-stage renal disease (eGFR ≤15 ml/min/1.73 m <sup>2</sup> , institution of chronic dialysis treatment or kidney transplantation) or iGFR decreases by 50% from one measurement to the next or serum creatinine levels double over any 12-month interval in the post-randomization period. <b>If these renal function changes prove to be temporary, the study medication could be resumed as described above with the consensus of the drug monitoring committee.</b>	Further clarify the resuming of study drug after discontinuation

#	Location	Change	Rationale
p.23	<b>6. STUDY TREATMENTS</b> 6.1.2. Dosage	<p><i>Revised Text:</i></p> <p>All participants, whether they are randomized to allopurinol or placebo, will be given four tablets per day to be taken orally following breakfast. Tablets will be provided in four vials (A, B, C, and D) <b>or in blister packs, in which each blister contains the four tablets for a given day. If the medication is provided in bottles</b>, participants randomized to allopurinol will receive a dosage of 100 mg as a 100 mg tablet (from vial A) plus three placebo tablets (from vials B, C, D), 200 mg as two 100 mg (from vials A and C) and two placebo tablets (from vials B and D), 300 mg as three 100 mg (from vials A, B, C) and one placebo tablet (from vial D), 400 mg as four 100 mg tablets (from vials A, B, C, D). Subjects randomized to placebo will be given four placebo tablets (from vials A, B, C, D). <b>If the medication is provided in blister packs, each blister will contain the four tablets for a given day, with the same proportion of active and placebo tablets described above for each allopurinol dosage and for placebo.</b></p> <p>The dose adjustment will be carried out as follows:</p> <p>At each follow-up visit, a study drug requisition will be sent by the clinical site to the research pharmacy indicating the study ID, name, and address of the participant, the most recent eGFR value (CKD-EPI), calculated using a recent local lab creatinine value, and the number of days to be covered by the drug supply.</p> <p>At the pharmacy, a clinical pharmacist will determine the allopurinol dose (ranging from 0 to 400 mg) that should be given at that time according to the study protocol given the participant's treatment assignment and the most recent eGFR value (CKD-EPI) calculated using a recent local lab serum creatinine value.</p> <p>The research pharmacy will mail the new batch of study medication directly to the study participant.</p> <p>At some sites the study medication may be dispensed directly to the study participant at a relevant in person study visit or by mail from the site following a relevant in-person or phone study visit.</p> <p>Participants will be instructed to immediately inform the clinical site upon receipt of the new tablets and mail the pill bottles <b>or blister packs</b> with the tablets remaining from the previous prescription in a provided pre-addressed mailer, to the clinical site for drug accounting and compliance assessment.</p>	Addition of ability to provide study medication in blister packs

#	Location	Change	Rationale
p.23-24	<b>6. STUDY TREATMENTS</b> 6.1.3. Compliance and Accountability	<i>Revised Text:</i> Skills will be taught and reinforced at each visit with regard to scheduling and administration of pills at home and while traveling. Methods (e.g. record-keeping) will be taught to help participants monitor tablet usage and enhance compliance. To complement the regular compliance interventions at the scheduled visits, study information and motivational materials (postcards, newsletters, etc.) will be mailed. In addition, at midpoint between clinic visits, participants will be phoned by the clinic staff to review pill-taking. Patients will be provided with random but known numbers of excess medications, providing extras in case of pill loss. Adherence will be monitored by instructing participants to expect extra pills and to mail the pill bottles <b>or the blister packs</b> with the tablets remaining from the previous prescription to the study center upon receipt of a new batch of tablets. The number of extra pills included in each supply of medications will be decided by the pharmacist, who will keep a record of it and will transmit this information to the Study Site. Personnel at the Study Site will enter this information in the appropriate electronic Case Report Form along with the expected number of pills used during the period covered by the supply and the number of unused pills returned by the participant. These data will be used to analyze compliance. If poor adherence is noticed, measures will be taken to increase compliance, such as explaining the purpose of the study again, providing pill reminders, and more frequently contacting the study subject by phone. Participants at each visit will be asked about their perceived compliance and about any difficulties with taking the study medications, but the individualized strategies to improve compliance will not be openly linked to the pill counts, i.e. participants will not be informed of the results of pill counting. Participants showing poor compliance will not be withdrawn from the study.	Addition of ability to provide study medication in blister packs
p.26	<b>8. STUDY PROCEDURES</b> 8.1. Schedule of Events	<i>New Text:</i> Outcome variables (plasma iohexol, serum creatinine and cystatin C, urinary AER), HbA1c, and serum uric acid will be measured by the Central Laboratory at the University of Minnesota, directed by Dr. <b>Amy Karger</b>  <i>Deleted Text:</i> John Eckfeldt	Adding new central lab director
p.29	<b>8. STUDY PROCEDURES</b> 8.1. Schedule of Events	<i>Revised Text:</i> Note: (x) indicates an optional assessment; <b>For “BP and Measurements”, (x) indicates an optional assessment only if the patient is NOT seen in-person.</b>	Clarification as to when the BP measurement is truly optional
P.31	<b>8. STUDY PROCEDURES</b> 8.5. Treatment Period (Visits 6 to 15)	<i>Revised Text:</i> In the days immediately after each visit, upon completion of serum creatinine measurements, participants will receive a new batch of study medication by mail from the research pharmacy. Upon receipt of the new tablets, participants will be instructed	Addition of ability to provide study medication in blister packs

#	Location	Change	Rationale
		to immediately mail the pill bottles <b>or the blister packs</b> with the tablets remaining from the previous prescription to the study center for drug accounting and compliance assessment (see 6.1.2). A pre-stamped and addressed envelope will be provided to participants for this purpose.	
P.32	<b>8. STUDY PROCEDURES</b> 8.6. End of Intervention (Visit 16)	<i>New Text:</i> Participants will be instructed to stop taking the study medication and to mail the pill bottles <b>or the blister packs</b> with the tablets remaining from the last prescription to the study center if they did not already bring the unused study medication at the visit. <u>The RAS and BP therapy will be continued as before until the closing visit (Visit 17).</u> The importance of coming back in 8 weeks for the closing visit (Visit 17) will be emphasized.	Addition of ability to provide study medication in blister packs
P.33	<b>9. SAFETY ASSESSMENTS</b> 9.1. Demographic Data/Medical History	<i>New Text:</i> After collecting a detailed medical history at Visit 1, this information will be updated at each visit through a structured interview, with a special emphasis on skin symptoms and signs such as rash, itching and exfoliation and on pregnancy in females. Participants will be instructed to communicate any change in their health status and intervening hospitalizations to the study coordinator in-between visits. In particular, they will be instructed to discontinue study medication and immediately contact the study coordinator if they develop a <b>suspicious</b> skin rash, swelling of the lips or mouth, arthralgias, and/or jaundice, which may indicate a hypersensitivity reaction to allopurinol. Fever and chills should also be reported but would not require cessation of medication prior to discussion with study personnel.  <i>Deleted Text:</i> Of any kind	Clarification of skin rash triggering discontinuation of study drug
P.35	<b>11. STATISTICAL ANALYSIS</b>	<i>New Text:</i> <b>This section presents a summary of the planned statistical analyses. A statistical analysis plan (SAP) will be written for the study that contains detailed descriptions of the analyses to be performed. The SAP will be written prior to database lock.</b>	Clarification of statistical analysis
p.35	<b>11. STATISTICAL ANALYSIS</b> 11.1. Analysis Population	<i>New Text:</i> <b>For most of the analyses, including the primary efficacy analysis described in section 11.3, an intention to treat (ITT) analytical approach will be employed. Accordingly, the population for statistical analysis will consist of all randomized study participants considered in their original randomization group, regardless of treatment discontinuation or loss to follow-up.</b> <b>Selected secondary efficacy analyses will be performed using a per-protocol</b>	Revised analysis plan



#	Location	Change	Rationale
		<p><b>analytical approach. In this case, the analysis population will consist of the ITT population excluding data points which 1. had cumulative exposure to the study medication from randomization that was less than 80% of the theoretical full exposure; or 2. during major protocol deviations (e.g., treatment with prohibited medications), which could affect primary outcome.</b></p>	
p.35	<p><b>11. STATISTICAL ANALYSIS</b> 11.3. Primary Efficacy Analysis</p>	<p><i>New Text:</i> For the primary endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention), we will follow the recommendations by Carpenter et al<sup>38,39</sup> and perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable. By conditioning on the baseline iGFR measure we will also effectively use this variable as a covariate. Treatment group, study center, stratifying variables, <b>albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did not qualify by eGFR slope and were normoalbuminuric at baseline)</b>, baseline AER, time, and time by treatment interaction will also be included as covariates in the model. Three features make this analytical approach especially attractive:</p>	Revised analysis plan
p.37	<p><b>11. STATISTICAL ANALYSIS</b> 11.4. Secondary Efficacy Analyses</p>	<p><i>New Text:</i> <b>We will perform a per-protocol analysis (as defined in 11.1) for the primary efficacy endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention).</b></p>	Revised analysis plan
p.37-38	<p><b>11. STATISTICAL ANALYSIS</b> 11.7. Model assumptions and alternative analyses</p>	<p><i>New Text:</i> Model assumptions will be thoroughly checked for individual and systematic departures, using informal, e.g. inspection of residuals, and formal methods such as score test for extra parameter or methods based on likelihood displacement. If individual outliers are detected, their influence will be evaluated using influence diagnostics methods based on comparing estimates from models fitted to data with and without outlying values. Whenever we are not successful in fitting the parametric model (linear or non-linear), then non-parametric analyses and/or transformation of the variables involved in the analysis will be considered. To investigate the potential hemodynamic influence of allopurinol on treatment effect, in addition to the aforementioned analyses, we will consider models including the post-randomization measure of GFR at 4 months as an additional covariate. To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup <b>analyses (based on the primary efficacy analysis described in section 11.3, with the inclusion of an interaction term of the treatment group by the subgroup variable)</b> will be performed by age groups (<math>\leq 40</math> and <math>&gt;40</math> yrs), gender, racial/ethnic group, HbA1c (<math>\leq 7.8</math></p>	Revised analysis plan

#	Location	Change	Rationale
		and >7.8%), serum uric acid ( $\leq 6.0$ and $> 6.0$ mg/dl), baseline iGFR ( $\leq 70$ ml/min and $> 70$ ml/min/ $1.73\text{m}^2$ ), AER <b>at baseline</b> ( $\leq 300$ and $> 300$ mg/24 hr), <b>and albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did qualify by eGFR slope and were normoalbuminuric at baseline). To investigate possible influence of using selected covariates on the treatment effect estimate in the models considered in Section 11, we will perform appropriate sensitivity analyses.</b> These additional analyses will be considered as strictly exploratory.	
p.45	<b>13. PROTECTION OF HUMAN SUBJECTS</b> 13.5. Procedures for protecting against and minimizing potential risks, b. specific	<p><i>New Text:</i></p> <p>Subjects taking drugs known to interact with allopurinol in causing bone marrow depression will be excluded from the study. White blood cell counts will be done before the study drugs are prescribed, and quarterly thereafter. The study drug <b>should be temporarily discontinued</b> should evidence of bone marrow depression (<math>\text{WBC} &lt; 3500/\text{mm}^3</math>) <b>be present and confirmed. WBC should be repeated two weeks after study drug discontinuation. If WBC recovers, consider re-challenging and repeating WBC two weeks after drug re-introduction. In addition, if WBC is confirmed to be <math>&lt; 2500/\text{mm}^3</math> and/or ANC is <math>&lt; 1000/\text{mm}^3</math>, the event also needs to be reported as an AE.</b> The Drug Monitoring Committee will review each case and decide whether a referral to a hematologist is warranted and whether study treatment can be reinstated after blood values have returned to normal. If drugs potentially causing bone marrow depression in combination with allopurinol are begun after entry into the trial, observations for this side effect will be intensified or, if recommended by the Drug Monitoring Committee, study drug may be interrupted.</p> <p><i>Deleted Text:</i> develop</p>	Additional details directing follow up and reporting of possible bone marrow depression
p.45	<b>13. PROTECTION OF HUMAN SUBJECTS</b> 13.5. Procedures for protecting against and minimizing potential risks, b. specific	<p><i>New Text:</i></p> <p>Blood pressure will be measured quarterly with the goal of maintaining BP <math>\leq 140</math> mmHg systolic and <math>\leq 90</math> mmHg diastolic. If elevated, a recheck will be performed within 2 weeks and if still elevated additional antihypertensive non-RAS blockers will be added in collaboration with the participants' physicians. Failure to achieve satisfactory BP control within 2 months would lead to a <b>case review by the Drug Monitoring Committee.</b></p> <p><i>Deleted Text:</i> Conference call</p>	Allows flexibility to discuss cases via e-mail or means other than a call

#	Location	Change	Rationale
p.47	<b>14. DATA AND SAFETY MONITORING PLAN</b>	<p><i>New Text:</i>  <i>IRB monitoring</i> will be in place from:            Joslin Diabetes Center            University of Minnesota            University of Colorado            University of Michigan            Northwestern University            University of Toronto            Albert Einstein University            Washington University            Steno Diabetes Center            University of Calgary  <b>University of Alberta</b>            Emory University            University of Washington  <b>University of Texas Southwestern</b>  <b>Providence Medical Research</b>  <b>BC Diabetes</b></p> <p><i>Deleted Text:</i>            Alberta Diabetes Institute</p>	Addition of new centers, and clarifying the name of an existing center for consistency
p.48	<b>14. DATA AND SAFETY MONITORING PLAN</b>	<p><i>Revised Text:</i>            A Drug Monitoring Committee (DMC) consisting of the PERL Center Directors and PIs, a research pharmacist, and the Project Manager will <b>discuss</b> any serious medication related problem that a participant has. Changes in study medication dose, medication discontinuation and medication re-institution will be <b>included in these discussions.</b></p> <p><i>Deleted text:</i>            hold conference calls            discussed during these calls</p>	Allows flexibility to discuss cases via e-mail or means other than a call
p.49	<b>15. STUDY ADMINISTRATION</b> 15.1. Organization	<p><i>Revised Text:</i>            The <i>Steering Committee</i> is responsible for the design of the study and provides guidance to its execution. Members are the co-Chairs of the PERL Consortium (Drs. Mauer and Doria), the Directors of the Clinical Sites (Drs. Caramori, Goldfine, Maahs, Perkins, Pop-Busui, Molitch, Crandall, Rossing, Sigal, Senior, Umpierrez, <b>Hirsch</b>,</p>	Adding new site directors, a change in central lab director, and correction to a name spelling error

#	Location	Change	Rationale
		<p><b>Lingvay, Tuttle, Aronson and Elliott</b>, the Directors of the Data Coordinating Center (Drs. Galecki and Spino), and the Director of the Central Laboratory (Dr. <b>Karger</b>), the NIH program officers (Drs. Jones and Flessner), and the JDRF program officer (Dr. <b>Pragnell</b>)</p> <p><i>Deleted Text:</i> Hirsh, Eckfeldt and Pragnell</p>	
p.49	<p><b>15. STUDY ADMINISTRATION</b> 15.1. Organization</p>	<p><i>Revised Text:</i> The <i>Drug Monitoring Committee</i> is responsible for the oversight of the study drug administration as well as the RAS blocking and antihypertensive therapy during the trial. Members are Dr. Doria, Dr. Mauer, the PIs of the clinical sites, the Project Manager, the Lead Clinical Coordinator, and a research pharmacist. The participation of one of the PIs and 5 of the <b>16</b> Center Directors will be sufficient for making decisions.</p> <p><i>Deleted Text:</i> 8</p>	Correcting current number of sites
p.49	<p><b>15. STUDY ADMINISTRATION</b> 15.1. Organization</p>	<p><i>Revised Text:</i> The <i>Clinical Sites</i> are located at the Joslin Diabetes Center, the University of Minnesota, the University of Colorado (Barbara Davis Center for Childhood Diabetes), the University of Michigan, Northwestern University, Albert Einstein College of Medicine, Washington University (St. Louis), the University of Toronto, the Steno Diabetes Center (Denmark), the University of Calgary (Calgary, Alberta, Canada), <b>University of Alberta</b> (Edmonton, Alberta, Canada), Emory University, the University of Washington (<b>Seattle</b>), <b>University of Texas Southwestern, Providence Medical Research, and BC Diabetes</b> are responsible for recruiting study participants and implementing the protocol.</p>	Addition of new centers, and clarifying the name of an existing center for consistency
p.49	<p><b>15. STUDY ADMINISTRATION</b> 15.1. Organization</p>	<p><i>Revised Text:</i> The <i>Central Laboratory</i>, located at the University of Minnesota, is directed by Dr. <b>Karger</b>, and is responsible for all blood and urine tests.</p> <p><i>Deleted Text:</i> Eckfeldt</p>	Adding new central laboratory director
<b>List of Changes in PERL Protocol Version 10.0 March 6, 2018</b>			
p.1	Title page	Change version and date <b>Version 10.0 Approved by the DSMB March 6, 2018</b>	Version date changed to date of DSMB approval

#	Location	Change	Rationale
p.2-4	Table of Contents	Page numbers 8-49 changed	Page numbers shifted.
p.5	<b>GENERAL INFORMATION</b> Program Officers	<i>Revised Text:</i> <b>Afshin Parsa, MD, MPH</b> Program Director, Clinical Kidney Genomics, PKD and CKD NIDDK/NIH 6707 Democracy Blvd. Room 6139 Bethesda, MD 20892-5460 Tel: (301) 827-1375 <a href="mailto:afshin.parsa@nih.gov">afshin.parsa@nih.gov</a>	New Program Officer
		<i>Deleted Text:</i> <b>Michael Flessner, MD, PhD</b> Division of Kidney, Urologic, & Hematologic Diseases NIDDK, National Institutes of Health Building 2DEM, Room 641 6707 Democracy Blvd. Bethesda, MD 20817 Phone: (301) 451-4901 <a href="mailto:flessnermf@mail.nih.gov">flessnermf@mail.nih.gov</a>	
p.6	<b>GENERAL INFORMATION</b> Directors of Clinical Sites	<i>Added Text:</i> <b>S. Michael Mauer, MD</b> University of Minnesota 2450 Riverside Ave. East Building, MB681 Minneapolis, MN 55454 <b>Mobile: (612) 703-5884</b>	Added mobile phone number
p.6-7	<b>GENERAL INFORMATION</b> Directors of Clinical Sites	<i>New Text:</i> <b>Sylvia Rosas, MD</b> Joslin Diabetes Center and Harvard Medical School One Joslin Place Boston, MA 02215 Phone: (617) 309-2477 Fax: (617) 309-3403 <a href="mailto:Sylvia.Rosas@joslin.harvard.edu">Sylvia.Rosas@joslin.harvard.edu</a> <i>Deleted Text:</i> Allison Goldfine, MD Joslin Diabetes Center and Harvard Medical School One Joslin Place Boston, MA 02215 Phone: (617) 309-2643 Fax: (617) 309-3403 <a href="mailto:allison.goldfine@joslin.harvard.edu">allison.goldfine@joslin.harvard.edu</a>	Change in Director of Clinical Site
p. 8	<b>GENERAL INFORMATION</b> Directors of Clinical Sites	<i>New Text:</i> <b>Ian de Boer, MD, MS</b> University of Washington Medical Center Box 359606 325 9th Avenue, Room 3NJ357 Seattle, WA 98104	New Director of clinical Site

#	Location	Change	Rationale
		Phone: (206) 616-5403 Fax: (206) 685-9399IDeBoer@Nephrology.washington.edu <i>Deleted Text:</i> <b>Irl B. Hirsch, MD</b> University of Washington Medical Center – Roosevelt 4245 Roosevelt Way, NE, 3 <sup>rd</sup> floor Box 354691 Seattle, WA 98105 Phone: (206) 598-4884 Fax: (202) 598-4976 ihirsch@uw.edu	
p. 12	Protocol Summary	<i>Revised Text:</i> February 22, 2018	
p. 21	<b>Discontinuation of study drug</b> Reasons for Discontinuation	<i>New Text:</i> Develops end-stage renal disease (confirmed eGFR $\leq$ 15 ml/min/1.73 m <sup>2</sup> in the absence of acute kidney injury [AKI], institution of chronic dialysis treatment or kidney transplantation) or iGFR decreases by 50% from one measurement to the next or serum creatinine levels double over any 12 month interval in the post-randomization period. If any of these renal function changes prove to be temporary, the study medication could be resumed as described above with the consensus of the drug monitoring committee.  <i>Revised Text:</i> Develops end-stage renal disease (eGFR $\leq$ 15 ml/min/1.73 m <sup>2</sup> , institution of chronic dialysis treatment or kidney transplantation) or iGFR decreases by 50% from one measurement to the next or serum creatinine levels double over any 12-month interval in the post-randomization period. If these renal function changes prove to be temporary, the study medication could be resumed as described above with the consensus of the drug monitoring Committee.	Clarification of wording for developing ESRD and drug discontinuation.
p. 22	<b>Discontinuation of study drug</b> Handling of study drug discontinuation	<i>New/Revised Text:</i> Unless a participant withdraws consent all participants that are permanently discontinued from study drug or who discontinue study medication on their own will be followed for the full study period (i.e., 164 weeks, including the washout period) and all data will be collected as scheduled.  <i>Deleted Text:</i> Or develop ESRD as defined above under 5.5.1	Wording clarification.

#	Location	Change	Rationale
p. 22	<b>Discontinuation of study drug</b> Handling of study drug discontinuation	<p><i>New Text:</i>            If a participant reaches ESRD as defined above under 5.5.1, he/she will be permanently discontinued from the study and invited to participate in a study close-out call or visit to be held within three months from the occurrence of ESRD. Data collection at this call or visit will be limited to standard adverse event reporting. In addition, sites should continue to contact participants who have reached end-stage renal disease to determine their final status until 3 years and 2 months after randomization. Major attempts will be made to schedule an end-of-study assessment for all participants who are lost to follow-up during the course of the study.</p>	New text was added to clarify handling of study drug discontinuation and data collection for specific scenario in which participant reaches ESRD.
p. 35	<b>Statistical Analysis</b> Analysis Population	<p><i>Revised Text:</i>            Selected secondary efficacy analyses will be performed using a per-protocol analytical approach. In this case, the analysis population will consist of the ITT population excluding data points which 1. Had cumulative exposure to the study medication from randomization that was less than 80% of the theoretical full exposure; or 2. during major protocol deviations (e.g., treatment with prohibited medications), which could affect primary outcome.</p> <p><i>Deleted Text:</i>            all iGFR measurements/ did incur</p>	Grammar clarification.
p. 49	<b>Study Administration</b>	<p><i>Revised Text:</i>            The Steering Committee is responsible for the design of the study and provides guidance to its execution. Members are the co-Chairs of the PERL Consortium (Drs. Maurer and Doria), the Directors of the Clinical Sites (Drs. Caramori, <b>Rosas, Polsky</b>, Perkins, Pop-Busui, Molitch, Crandall, Rossing, Sigal, Senior, Umpierrez, <b>De Boer</b>, Lingway, Tuttle, Aronson and Elliott), the Directors of the Data Coordinating Center (Drs. Galecki and Spino), and the Director of the Central Laboratory (Dr. Karger), the NIH program officers (Drs. Jones and <b>Parsa</b>), and the JDRF program officer (Dr. Pragnell).</p> <p><i>Deleted Text:</i>            Goldfine, Maaha, Hirsch, Flessner</p>	



**PERL**  
PREVENTING EARLY RENAL LOSS  
IN DIABETES

**A multicenter clinical trial of allopurinol to prevent GFR loss in  
type 1 diabetes**

**Statistical Analysis Plan  
Prepared by PERL DCC**

**February 22, 2017**



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## 1. Overview

### DESIGN:

- Multicenter, double-blind, placebo-controlled, parallel-group randomized clinical trial.
- N=530 total number of subjects

### STUDY POPULATION:

- Type 1 Diabetes
- Inclusion/Exclusion Criteria listed in the Study Protocol

### STUDY TREATMENTS:

- Oral allopurinol or placebo administered for 3 years followed by a 2-month washout

### PRIMARY OUTCOME MEASURE:

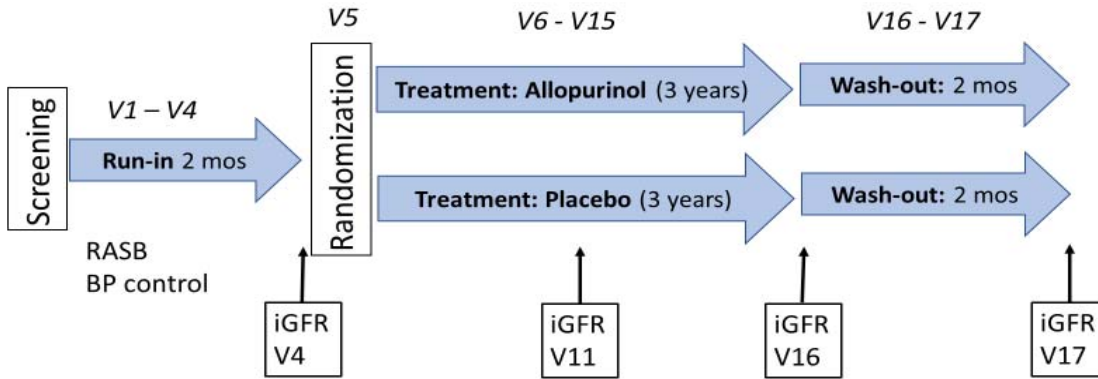
- iGFR at the end of the 2-month wash-out period following the 3-year intervention

### STATISTICAL ANALYSIS PLAN:

- This plan will be finalized prior to the database lock and unblinding of treatment groups

## 2. Schema

Figure 2.1. PERL Study Schema



### **3. Rationale for Adjustments of Statistical Analysis Plan as Compared to Protocol (Version 9, approved by DSMB on August 16<sup>th</sup>, 2016)**

Changes from the protocol-specified definitions of aims, outcomes, and statistical analytical approaches are outlined below. These changes reflect internal discussions since the design of the study that have not been incorporated yet as protocol amendments, but were discussed during the preparation of the Statistical Analysis Plan. These changes and the rationale for their implementation are documented herein and represent changes made prior to the database lock and unblinding of the study.

#### **3.1. Use of modified Intention-to-Treat (ITT) Analysis Population**

##### RATIONALE:

Given that some of the randomized subjects did not receive *any* study medication, data analysis will be based on the concept of *modified* ITT (mITT) population rather than ITT population.

##### PROTOCOL:

We define the ITT analysis population as "... all randomized study participants considered in their original randomization group, regardless of treatment discontinuation or loss to follow-up."

##### SAP:

We define the mITT analysis population as "... all randomized study participants considered in their original randomization group, regardless of treatment discontinuation or loss to follow-up who received *at least one dose* of study medication."

#### **3.2. Simplified model for the primary efficacy analysis**

##### RATIONALE:

The primary efficacy analysis presented in Section 11.2 of the study protocol was based on a linear model for correlated errors using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable. The iGFR at baseline was included among the dependent variables to effectively adjust the treatment effect for baseline iGFR in the presence of a considerable number of missing values. Since iGFR values at baseline are missing for only two randomized subjects, which can be imputed in the analyses, we have decided to adjust for baseline iGFR in a standard way by including it as a covariate. Please note that both modeling approaches/specifications are *equivalent* if there are no missing iGFR values at baseline.

##### PROTOCOL:

We specify the model as follows "... perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable. By conditioning on the baseline iGFR measure we will also effectively use this variable as a covariate. Treatment group, study center, stratifying variables, albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did

qualified by eGFR slope and were normoalbuminuric at baseline), baseline AER, time, and time by treatment interaction will also be included as covariates in the model.”

SAP:

We specify the model as follows “... perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available post-randomization iGFR measures (including 80, 156, and 164 weeks, respectively) as the dependent variable. At any given visit, iGFR in this model depends on treatment group, study center, stratifying variables, iGFR at baseline, albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did not qualify by eGFR slope and were normoalbuminuric at baseline), and baseline AER.”

#### **4. Study Aim**

The study aim is to determine whether lowering serum UA by means of oral allopurinol is effective in preventing or slowing decline of renal function in T1D patients with history and/or presence of microalbuminuria or moderate macroalbuminuria, or with ongoing GFR loss regardless of history or presence of albuminuria, who have only mildly or moderately impaired kidney function.

## 5. Study Endpoints and Other Outcomes

This section describes the primary and secondary efficacy outcomes, as well as safety and other outcomes, that will be included in the primary manuscript. Derivation of the endpoints and other outcomes from the data collected in the Case Report Forms will be described in detail in the Derived Dataset Requirements document.

### 5.1. Primary Endpoint

The primary endpoint is iohexol-plasma disappearance GFR (iGFR) at the end of the 2-month wash-out period (Visit 17 at Week 164) following the 3-year intervention. The rationale of measuring the primary outcome at the end of the wash-out period is to test allopurinol for *permanent* effects on the natural history of kidney disease, independent from any transient, hemodynamic effect that the medication may have on GFR. iGFR is calculated from blood samples drawn at baseline and 120, 150, 180, 210, and 240 minutes after an i.v. bolus of iohexol, adjusting for body surface area. If there are fewer than five measures or the other quality criteria described in the protocol are not met, the iGFR value is not used in the analysis.

### 5.2. Secondary Outcome Measures

#### 5.2.1. Secondary endpoint: iGFR at the end of the 3-year treatment period (Visit 16, before the washout)

iGFR calculated at the end of the 3-year intervention (at Visit 16, last visit before washout) as measured by the plasma disappearance of non-radioactive iohexol.

#### 5.2.2. Secondary endpoint: iGFR time trajectory estimated from repeated iGFR measurements

Repeated measures of iGFR at Visits 11, 16, 17.

#### 5.2.3. Secondary endpoint: Estimated (eGFR) at 4 months after randomization (Visit 7)

eGFR at 4 months after randomization as estimated from serum creatinine and cystatin C using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations (Fan et al, 2015). This endpoint is employed to measure a transient, hemodynamic effect that the study medication may have on GFR.

#### 5.2.4. Secondary endpoint: Estimated GFR (eGFR) time trajectory

Repeated eGFR measures at all post-randomization visits (Visit 6 through 17) as estimated from repeated serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations.

#### 5.2.5. Secondary endpoint: Time to doubling of serum creatinine or end-stage renal disease (ESRD)



This secondary endpoint is defined as a composite of two events: (1) ESRD, defined as eGFR  $\leq$  15 ml/min/1.73 m<sup>2</sup>, institution of chronic dialysis treatment or kidney transplantation, or (2) Doubling of serum creatinine levels as compared to baseline levels. Time to doubling of serum creatinine or ESRD is defined as the time from randomization to the first event (doubling of serum creatinine or ESRD) or censoring (lost-to-follow-up, withdrawal, and study completion without experiencing the event).

**5.2.6. Secondary endpoint: Urinary AER at the end of the two-month wash-out period (Visit 17)**

Geometric mean of two urinary AER measures obtained at Visit 17.

**5.2.7. Secondary endpoint: Urinary AER during the last three months of the treatment period (Visits 15 and 16)**

Geometric mean of urinary AER measures at Visit 15 and Visit 16.

**5.2.8. Secondary endpoint: Time to fatal or non-fatal cardiovascular events**

This secondary endpoint is defined as a composite of multiple events: (1) Cardiovascular disease (CVD) death (ICD-10 code I10 to I74.9), (2) Myocardial infarction, (3) Stroke (ischemic or hemorrhagic), (4) Coronary artery bypass grafting, or (5) Percutaneous coronary intervention. Time to fatal or non-fatal cardiovascular events is defined as the time from randomization to the first event (one of the events defined above) or censoring (lost-to-follow-up, withdrawal, non-CVD death, and study completion without experiencing the event).

**5.3. Safety measures**

Safety measures are assessed during three periods of the study: run-in (Visits 1-5), treatment (after Visit 5 through Visit 16), and off-treatment washout (after Visit 16 through Visit 17). Safety will be summarized overall (treatment and off-treatment combined) and by period, depending on the safety outcome of interest during that period.

- Percentage of subjects with SAEs, number of SAEs, time to first SAEs during on-treatment period.
- Percentage of subjects with and number of permanent discontinuations of study medication because of adverse effects.
- Percentage of subjects with and number of AEs, overall and by severity and by relatedness to study medication.
- Percentage of subjects with skin rash during on-treatment period.

**5.4. Other measures**

In addition to primary, secondary, and safety measures, the following additional outcomes will be analyzed to help with the interpretation of study results.

- Body weight, blood pressure, serum creatinine, HbA1c, and serum uric acid at each post-baseline visit and their changes from baseline
- Percentage of subjects receiving adequate study medication exposure (i.e., allopurinol or placebo) independent of adverse events. This is defined as the actual total dose during the 156-week dosing period, as determined from the dispensed dosage and pill counts, divided by the expected total dose defined by the eGFR-adjusted protocol-described dosing regimen, without consideration for temporary or permanent discontinuations or reductions owing to adverse events. The proportion of subjects receiving the adequate intended study medication exposure is defined as the number of subjects who had at least 80% and no more than 120% of the intended study medications during the entire dosing period, independent of adverse events, among all randomized subjects.

## 6. Analytical Strategy

In the initial analysis of the primary outcome we will present iGFR univariate statistics by Treatment Groups at each study visits (V4, V11, V16 and V17).

No formal interim analyses of the primary endpoint will be conducted, therefore the nominal  $\alpha$  level to be used at the final analysis will be 0.05 for the primary endpoint. All other secondary outcomes will also be tested at the 5% level, with no adjustment for multiplicity. Many of the models used in efficacy analyses include baseline covariates, such as stratifying variables (serum uric acid (sUA), HbA1C, clinical site), iGFR, albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who qualified by eGFR slope and were normoalbuminuric at baseline), AER, and time, and time by treatment interaction. If there are problems with fitting these models, due, for example, to lack of convergence to optimal values, covariates will be eliminated from the models in the following order: baseline AER, albuminuria status, clinical site, serum uric acid (sUA), and HbA1c. More detailed information about these covariates is included in Section 6.4.

### 6.1. Study populations

Two study populations will be defined for the purpose of data analysis:

- **Modified Intention to Treat (mITT):** The mITT analysis set consists of all subjects enrolled in PERL, randomized to study medication, and receiving at least one dose of study medication.
- **Per Protocol:** The per protocol analysis set will consist of a subset of mITT subjects. The per protocol population will exclude subjects with major protocol deviations (defined as receiving the wrong study medication) as well as data points for which the cumulative exposure to the study medication from randomization to that time point was less than 80% of the theoretical full exposure (see Section 11.1 in the protocol).

To account for missing values in any specific analysis, all subjects meeting the study population definitions will be included in the analysis using (1) appropriate analytical approaches that allow for missing values under plausible missing data mechanisms, or (2) analytical methods (defined within specific analyses) that allows the imputation of missing outcomes.

Since the mITT approach can result in the need to analyze data with missing values of the outcomes (or covariates), we will follow four strategies proposed by I.R. White et al (2011):

1. Attempt to follow up all randomized participants, even if they withdraw from allocated treatment.
2. Perform a main analysis of all observed data that are valid under a plausible assumption about missing data.
3. Perform sensitivity analyses to explore the effect of departures from the assumptions made in the main analysis.
4. Account for all mITT study population participants, at least in the sensitivity analyses.

Note that this approach is tailored to mITT population and deviates slightly from the “all randomized participants” suggested by White.

## 6.2. Blinded Data Review

Prior to unmasking the study and starting any formal analysis, data will be reviewed in a blinded fashion by computing summary statistics for primary and secondary outcomes, and baseline covariates. This will allow the identification of unusual values and/or patterns of missing values for key variables that need to be queried. In addition, such blinded data review will allow the writing committee to assess the format of data presentation. Note that the blinded data review incorporates real data but *random* treatment assignment (i.e., investigators do not receive data summarized by actual treatment group, rather they review data on two randomly formed groups). All decisions will be made and documented in this SAP document prior to database lock and unblinding.

## 6.3. Visit Windows

To provide scheduling flexibility to study sites and participants, visits were required to occur within a protocol-defined window rather than on a specific date. The protocol-defined visit windows are summarized in the table below. For analytic purposes, the visit windows defined in the protocol will be expanded in order to eliminate gaps between them. This will ensure that all observations, including those that may have occurred outside a protocol-specified time window, will be associated with the most appropriate visit and therefore properly included in the analysis. If multiple observations occur within a window, the one closest to the visit target date will be utilized. If two observations are equi-distant from the target date, the first one will be utilized.

**Table 6.3.1. PERL windows for post-randomization visits.**

Visit	Lower Boundary of Window (Week, Excluding First day)	Per protocol Target Date window in weeks	Upper Boundary of Window (Week, Including last day)
Visit Windows Relative to Visit 5 Date			
Visit 6	0	<u>4 [3-5]</u>	10
Visit 7	10	<u>16 [14-20]</u>	24
Visit 8	24	<u>32 [30-34]</u>	40
Visit 9	40	<u>48 [46-50]</u>	56
Visit 10	56	<u>64 [62-66]</u>	72
Visit 11	72	<u>80 [78-84]</u>	88
Visit 12	88	<u>96 [94-98]</u>	104
Visit 13	104	<u>112 [110 -114]</u>	120
Visit 14	120	<u>128 [126 -130]</u>	135
Visit 15	135	<u>142 [140 -146]</u>	149

Visit	Lower Boundary of Window (Week, Excluding First day)	Per protocol Target Date window in weeks	Upper Boundary of Window (Week, Including last day)
Visit 16	149	156 [154 -160]	160
Visit Window relative to V16			
Visit 17	0	8 [6-12]	16

All intervals (target dates and lower/upper window boundaries) for visits 6 through 16 are calculated relative to the Visit 5 date. The interval for Visit 17 is calculated relative to Visit 16. Lower and upper boundaries are based on the mid-points between target dates. Most post-randomization visits are 16 weeks apart, with the exception of Visits 6 and 7 and Visits 16 to visit 17.

#### 6.4. Covariates

The following is a description of the covariates that will be used in the various analyses outlined in the remainder of Section 6.

- Stratifying variables
  - serum uric acid (sUA) at baseline with 2 levels ( $\leq 6.0$  and  $> 6.0$  mg/dl)
  - glycated hemoglobin (HbA1c) at baseline with two levels ( $\leq 7.8$  and  $> 7.8\%$ )
  - clinical site/study center with 16 levels (based on main sites with satellite sites collapsed into main sites)
- Baseline iGFR measured at Visit 4
- Baseline eGFR measured at Visit 4
- Treatment group with two levels (Allopurinol, Placebo)
- Albuminuria status with 2 levels (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who qualified by eGFR slope and were normoalbuminuric at baseline)
- Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale

At the time of writing, we have complete data available for serum uric acid, treatment group, and study center. The number of missing values for the other baseline covariates is 2 for iGFR, 6 for HbA1c, and 2 for albuminuria status. Given the small number of missing values for baseline covariates, we will employ single-value stochastic regression model imputation (Van Buuren, 2012).

#### 6.5. Analysis of the Primary Endpoint

##### 6.5.1. Primary Analysis of the Primary Endpoint

The goal of the primary analysis will be to test the null hypothesis of the difference between treatment arms in the primary endpoint (iGFR at the end of the 2-month wash-out period [Visit 17] following the 3-year intervention) being equal to zero. The analysis will be performed on the modified intention-to-treat (mITT) population and will employ a linear

model for correlated errors with general/unstructured covariance matrix (Molenberghs and Verbeke, 2005; Galecki and Burzykowski, 2013). For each time  $t$  ( $t = 1, 2, 3$ ) corresponding to post-randomization iGFR visits, i.e. visits V11 (80 weeks), V16 (156 weeks), and V17 (164 weeks after randomization) the model equation is specified as:

$$iGFR_{it} = \beta_{0t} + \beta_{1t} TRT_i + \mathbf{x}'_i \boldsymbol{\beta} + \epsilon_{it}, \quad (6.1)$$

where  $iGFR_{it}$  is the value of iGFR at time  $t$  for subject  $i$  ( $i = 1, \dots, 530$ ). Fixed effects  $\beta_{0t}, \beta_{1t}$  for  $t = 1, 2, 3$  denote visit-specific intercepts and treatment effects.  $TRT_i$  is treatment group (equal to 1 for the allopurinol and 0 for placebo). Stratifying variables (serum uric acid, HbA1c, study center), and baseline covariates: albuminuria status, AER, iGFR for subject  $i$  are included in a vector  $\mathbf{x}_i$  of  $p$  covariates ( $x_1, \dots, x_p$ ) and associated fixed effects are stored in vector  $\boldsymbol{\beta} = (\beta_1, \dots, \beta_p)$ . We assume that residual errors  $\epsilon_{it}$  ( $t = 1, 2, 3$ ) for subject  $i$  are normally distributed with zero mean and 3x3 general/unstructured variance-covariance matrix. The model specified in (6.1) will yield the estimates of visit-specific treatment effects  $\beta_{11}, \beta_{12}, \beta_{13}$  for all three visits V11, V16 and V17. In the context of the primary analysis of the primary endpoint, we are interested in parameter  $\beta_{13}$ , representing treatment effect at Visit 17 adjusted for stratifying variables and baseline covariates.

<b>Analysis</b>	Primary Analysis of the Primary Endpoint: iGFR at the end of the 2-month wash-out period (Visit 17)
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Linear model for repeated measures with correlated errors
<b>Dependent Variable</b>	iGFR measured at Visits V11 (80 weeks), V16 (156 weeks) and V17 (164 weeks after randomization)
<b>Model</b>	Fixed effects: <ul style="list-style-type: none"> <li>• Visit-specific intercepts corresponding to V11, V16, V17</li> <li>• Visit-specific treatment effects corresponding to V11, V16, V17</li> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Predicted iGFR means <i>at Visit 17</i> for an exemplary subject by treatment group.</li> <li>• Estimate of treatment effect <i>at Visit 17</i> adjusted for baseline covariates</li> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>

### 6.5.2. Secondary Analysis of the Primary Endpoint

The primary analysis of the primary endpoint will be performed under the missing at random (MAR) assumption, i.e. the probability that the iGFR is missing depends on *observed* rather than *unobserved* values of the dependent variable. Although we consider the MAR assumption to be sensible for our study, the following sensitivity analyses will be performed to assess how alternative definitions of the primary endpoint (as defined above) and alternative approaches for handling missing data may affect the conclusions of the analysis:

1. Analysis of covariance using iGFR values at Visit 17 as the dependent variable and treatment effect as a covariate of primary interest. The same baseline covariates, as in the primary analysis of the primary endpoint, stored in vector  $x_i$  (see Equation. 6.1) will be used in the model.
2. Performing an analysis identical to the primary one (same endpoint and model) using the per-protocol analysis set rather than the mITT analysis set.

## 6.6. Analyses of Secondary Endpoints

### 6.6.1. iGFR at the end of the 3-year treatment period (Visit 16, before the washout)

The predicted means at Visit 16, estimate of treatment effect *at Visit 16* adjusted for baseline covariates, their 95% confidence interval and P-value will be obtained as part of the primary analysis of the primary endpoint (Equation (6.1) in section 6.1.1). In the context of this secondary endpoint, we are interested in fixed effect  $\beta_{12}$ , which represents treatment effect at Visit 16 adjusted for stratifying variables and baseline covariates.

### 6.6.2. iGFR time trajectory estimated from repeated iGFR measurements

<b>Analysis</b>	Analysis of the Secondary Endpoint: iGFR time trajectory estimated from repeated iGFR measurements
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Linear mixed-effects model for longitudinal iGFR measures
<b>Dependent Variable</b>	iGFR measured at Visits V11 (80 weeks) , V16 (156 weeks) and V17 (164 weeks after randomization)
<b>Model</b>	<p>Fixed effects associated with:</p> <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Time since randomization in days</li> <li>• Time by treatment group interaction</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul> <p>Subject-specific random effects</p> <ul style="list-style-type: none"> <li>• Random intercept for iGFR</li> <li>• Random slope for iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• iGFR slope estimates and 95% CIs by treatment group</li> <li>• Estimate of a treatment effect measured as a difference between average slopes of iGFR versus time for allopurinol and placebo groups adjusted for stratifying variables and baseline covariates</li> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>

### 6.6.3. eGFR at 4 months after randomization (Visit 7)

<b>Analysis</b>	Analysis of the Secondary Endpoint: eGFR at 4 months after randomization (Visit 7)
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<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Linear model
<b>Dependent Variable</b>	eGFR measured at Visit V7 (16 weeks after randomization)
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline eGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Predicted eGFR means <i>at Visit 7</i> for an exemplary subject by treatment group.</li> <li>• Estimate of treatment effect <i>at Visit 7</i> adjusted for stratifying variables and baseline covariates</li> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>

#### 6.6.4. eGFR time trajectory

<b>Analysis</b>	Analysis of the Secondary Endpoint: eGFR time trajectory estimated from repeated eGFR measurements
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Linear mixed-effects model for longitudinal eGFR measures
<b>Dependent Variable</b>	Post-randomization eGFR measured from Visits V6 through V17
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Time since randomization in days</li> <li>• Time by treatment group interaction</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline eGFR</li> </ul> Subject-specific random effects <ul style="list-style-type: none"> <li>• Random intercept for eGFR</li> <li>• Random slope for eGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• eGFR slope estimates and 95% CIs by treatment group</li> <li>• Estimate of a treatment effect measured as a difference between average eGFR versus time slopes for allopurinol and placebo groups adjusted for stratifying variables and baseline covariates</li> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>

#### 6.6.5. Time to serum creatinine doubling or ESRD

<b>Analysis</b>	Analysis of the Secondary Endpoint: Time to composite endpoint of serum creatinine doubling or ESRD
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Cox proportional hazards model



<b>Dependent Variable</b>	Time to composite endpoint of serum creatinine doubling or ESRD
<b>Cox Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• N(%) of subjects with doubled serum creatinine or ESRD during the course of the study</li> <li>• Hazard ratio of allopurinol to placebo</li> <li>• 95% confidence interval for hazard ratio</li> <li>• P-value for treatment effect</li> </ul>

#### 6.6.6. Urinary AER at the end of the wash-out period

<b>Analysis</b>	Analysis of the Secondary Endpoint: AER at the end of the wash-out period Visit 17
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Linear model
<b>Dependent Variable</b>	Two AER measures obtained at Visit 17 and summarized using the geometric mean expressed on logarithm base to 10 scale
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Predicted urinary AERs <i>at Visit 17</i> for an exemplary subject by treatment group</li> <li>• Estimate of treatment effect <i>at Visit 17</i> expressed on percent change scale using antilog transformation.</li> <li>• 95% confidence interval for treatment effect expressed on percent change scale using antilog transformation.</li> <li>• P-value for treatment effect</li> </ul>

#### 6.6.7. Urinary AER during the last three months of the treatment period (Visits 15 and 16)

<b>Analysis</b>	Analysis of the Secondary Endpoint: AER at the end of the treatment period (Visits V15 and V16)
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Linear model
<b>Dependent Variable</b>	Two AER measures obtained at Visit 15 and 16 are summarized using the geometric mean expressed on logarithmic scale
<b>Model</b>	Fixed effects associated with:

	<ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Predicted AERs at the end of treatment period for an exemplary subject by treatment group.</li> <li>• Estimate of treatment effect at the end of treatment period adjusted for baseline covariates expressed on percent change scale using antilog transformation.</li> <li>• 95% confidence interval for treatment effect expressed on percent change using antilog transformation.</li> <li>• P-value for treatment effect</li> </ul>

### 6.6.8. Time to fatal or non-fatal cardiovascular events

<b>Analysis</b>	Analysis of the Secondary Endpoint: Time to fatal or non-fatal cardiovascular events
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Cox proportional hazards model
<b>Dependent Variable</b>	Time to composite endpoint: fatal or non-fatal cardiovascular events
<b>Cox Model</b>	Fixed effects: <ul style="list-style-type: none"> <li>• Fixed effects associated with:</li> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• N(%) of subjects with fatal or non-fatal cardiovascular events during the course of the study</li> <li>• Hazard ratio of allopurinol to placebo</li> <li>• 95% confidence interval for hazard ratio</li> <li>• P-value for treatment effect</li> </ul>

### 6.7. Subgroup Analyses

To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses (based on the primary efficacy analysis described in subsection 6.6.1, with the inclusion of appropriate interaction terms with the subgroup variable) will be performed by age groups ( $\leq 40$  and  $> 40$  yrs), gender, racial/ethnic group, HbA1c ( $\leq 7.8$  and  $> 7.8\%$ ), serum uric acid ( $\leq 6.0$  and  $> 6.0$  mg/dl), baseline iGFR ( $\leq 70$  ml/min and  $> 70$ ). ml/min/1.73m<sup>2</sup>), AER at baseline ( $\leq 300$  and  $> 300$  mg/24 hr), and albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did not qualify by eGFR slope and were normoalbuminuric at baseline).

An example of such subgroup analysis for age groups ( $\leq 40$  and  $>40$  yrs) is provided below. Similar to Equation (6.1) for each time  $t$  ( $t = 1, 2, 3$ ), corresponding to visits V11, V16, V17, we specify the model:

$$iGFR_{it} = \beta_{0t} + \beta_{1t} TRT_i + \beta_{2t} AGE_i + \beta_{3t} AGE_i \times TRT_i + \mathbf{x}'_i \boldsymbol{\beta} + \epsilon_{it}, \quad (6.2)$$

where  $iGFR_{it}$  is the value of iGFR at time  $t$  for subject  $i$  ( $i = 1, \dots, 530$ ). Fixed effects  $\beta_{0t}, \beta_{1t}, \beta_{2t}, \beta_{3t}$  for  $t = 1, 2, 3$  denote visit-specific intercepts, treatment effects, age effects and age by treatment interactions, respectively.  $TRT_i$  is treatment group (equal to 1 for the allopurinol and 0 for placebo).  $AGE_i$  indicates age group ( $\leq 40$  and  $>40$  yrs). Stratifying variables, and baseline covariates albuminuria status, AER, iGFR for subject  $i$  are included in a vector of covariates  $\mathbf{x}_i$  and associated fixed effects are stored in vector  $\boldsymbol{\beta}$ . We assume that residual errors  $\epsilon_{it}$  ( $t = 1, 2, 3$ ) for subject  $i$  are normally distributed with zero mean and  $3 \times 3$  general/unstructured variance-covariance matrix. The model specified in (6.2) will yield the estimates of visit-specific treatment by age interaction effects  $\beta_{31}, \beta_{32}, \beta_{33}$  for all three visits V11, V16 and V17. In the context of subgroup analysis, we are interested in  $\beta_{33}$ , which represents treatment by age interaction at Visit 17 adjusted for baseline covariates.

<b>Analysis</b>	Subgroup Analysis of the Primary Endpoint: iGFR at the end of the 2-month wash-out period (Visit 17) by Age group
<b>Analysis Set</b>	mITT Population
<b>Methods</b>	Linear model for repeated measures with correlated errors
<b>Dependent Variable</b>	iGFR measured at Visits V11 (80 weeks), V16 (156 weeks) and V17 (164 weeks after randomization)
<b>Model</b>	Fixed effects: <ul style="list-style-type: none"> <li>• Visit-specific intercepts, age effects, treatment effects and age by treatment interaction effects</li> <li>• Stratifying variables: sUA, HbA1c, Study center</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Estimate of age by treatment interaction <i>at Visit 17</i> adjusted for baseline covariates</li> <li>• 95% confidence interval for age by treatment effect interaction <i>at Visit 17</i></li> <li>• P-value for treatment effect</li> </ul>

## 6.8. Analyses of Safety Outcomes

For dichotomous safety outcomes, the proportion of subjects experiencing adverse outcomes (AEs, SAEs) will be summarized by treatment group and compared by means of Fisher's exact tests. Poisson regression models will be used for safety outcomes (e.g., SAEs and AEs) with multiple recurrences per patient, with logarithm of the period of observation from the time of study medication used as the offset. Time to first SAE will be analyzed using Kaplan-Meier methods to estimate the SAE-free distributions for each treatment group. This analysis will employ the mITT analysis set.

## **6.9. Model assumptions and alternative analyses**

Model assumptions will be thoroughly checked for individual and systematic departures, using informal, e.g. inspection of residuals, and formal methods such as methods based on likelihood displacement. If individual outliers are detected, their influence will be evaluated using influence diagnostics methods based on comparing estimates from models fitted to data with and without outlying values. Whenever we are not successful in fitting the parametric model (linear or non-linear), then non-parametric analyses and/or transformation of the variables involved in the analysis will be considered.

## 7. Table, Listing and Figure Shells

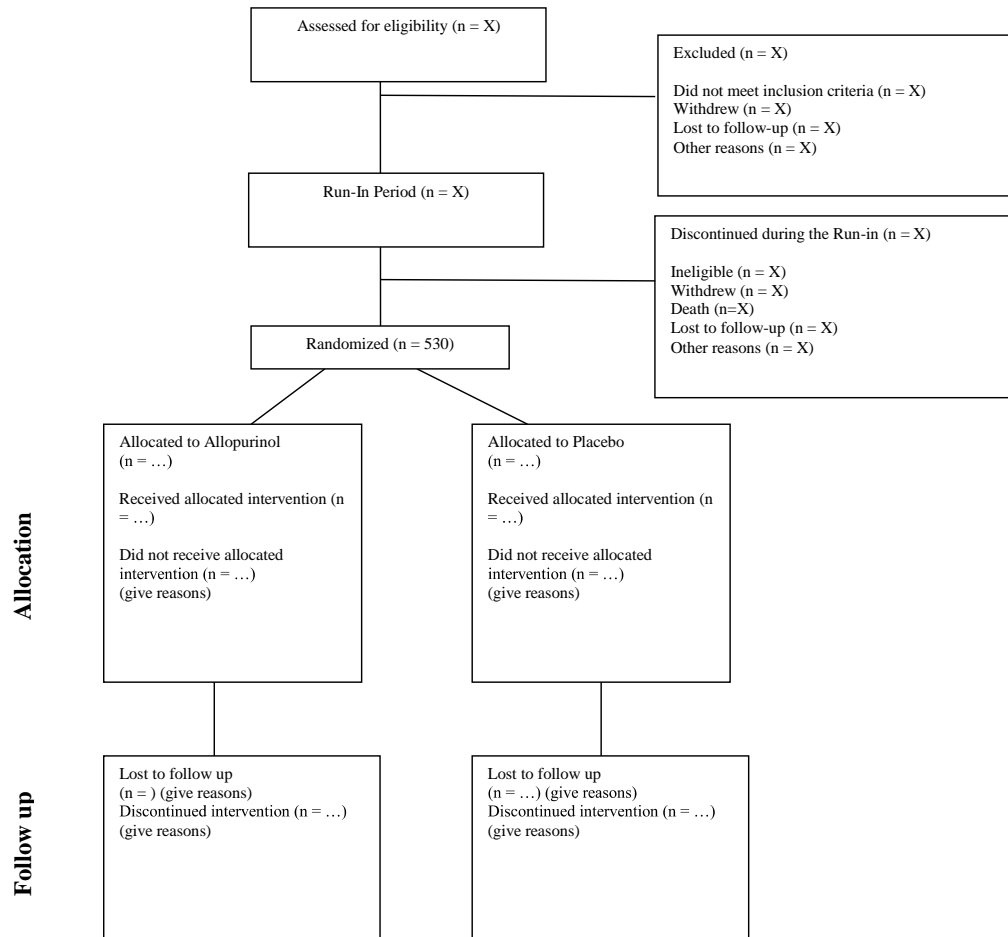
**Figure 7.1. Time from Randomization to End of Study by Treatment Group**

Kaplan-Meier plot of time from randomization to End of Study (death, withdrawal or lost-to-follow-up) in months

Y axis label = % of Subjects (100%, 90%, ... , 10%, 0%)

X axis label = Time (months) post-randomization (0, 6, 12, 18, 24, 36 months)

**Figure 7.2. Consort diagram describing the trial.**



**Table 7.1. Patients disposition by Treatment Group. All Subjects**

	N (%)	
<b>SCREENING PERIOD (at Visit 1)</b>	N	
Discontinuations after Visit 1 and before Visit 2	xx.x%	
Screen Failure (Ineligible for Run-in)	xx.x%	
Withdrawal	xx.x%	
Lost to Follow-up	xx.x%	
Other	xx.x%	
Eligible for Run-in	xx.x%	
<b>RUN-IN PERIOD (at Visit 2 through Visit 4)</b>	N	
Discontinued during Run-in (at Visit 2 through Visit 4)	N	
Ineligible	xx.x%	
Death	xx.x%	
Lost to Follow-up	xx.x%	
Withdrawal	xx.x%	
Other	xx.x%	
RANDOMIZED (Visit 5)	xx.x%	
<b>POST-RANDOMIZATION PERIOD (Visits 6-17)</b>	<b>Treatment Group</b>	
	<b>Allopurinol N=</b>	<b>Placebo N=</b>
Randomized	n	n
Post-Randomization Discontinuations	n	n
Death	n	n
Lost to Follow-up	n	n
Withdrew Consent	n	n
Other	n	n
Post-Randomization Discontinuations /# Randomized (%)	xx.x%	xx.x%
Completed Study	n	n
Completed Study (%)	xx.x%	xx.x%

**Table 7.2. Patient Follow-Up by Treatment Group. All Subjects.**

Time Point, n (%)	Treatment Group		Total
	Allopurinol N=	Placebo N=	
Screened (Visit 1)			N
Randomization			N/N_S (xx%)
Visit 6	N_Visit 6/N* (xx%)	N_Visit 6/N* (xx%)	N_Visit 6/N* (xx%)
Visit 7	N_Visit 7/N* (xx%)	N_Visit 7/N* (xx%)	N_Visit 7/N* (xx%)
Visit 8	N_Visit 8/N* (xx%)	N_Visit 8/N* (xx%)	N_Visit 8/N* (xx%)
Visit 9	N_Visit 9/N* (xx%)	N_Visit 9/N* (xx%)	N_Visit 9/N* (xx%)
Visit 10	N_Visit 10/N* (xx%)	N_Visit 10/N* (xx%)	N_Visit 10/N* (xx%)
Visit 11	N_Visit 11/N* (xx%)	N_Visit 11/N* (xx%)	N_Visit 11/N* (xx%)
Visit 12	N_Visit 12/N (xx%)	N_Visit 12/N (xx%)	N_Visit 12/N (xx%)
Visit 13	N_Visit 13/N* (xx%)	N_Visit 13/N* (xx%)	N_Visit 13/N* (xx%)
Visit 14	N_Visit 14/N* (xx%)	N_Visit 14/N* (xx%)	N_Visit 14/N* (xx%)
Visit 15	N_Visit 15/N* (xx%)	N_Visit 15/N* (xx%)	N_Visit 15/N* (xx%)
Visit 16	N_Visit 16/N* (xx%)	N_Visit 16/N* (xx%)	N_Visit 16/N* (xx%)
Visit 17	N_Visit 17/N* (xx%)	N_Visit 17/N* (xx%)	N_Visit 17/N* (xx%)
Completed Across All Visits	$\Sigma N_i$	$\Sigma N_i$	$\Sigma N_i$
% Completed of Total	$\Sigma N_i / n = xx\%$	$\Sigma N_i / n = xx\%$	$\Sigma N_i / n = xx\%$
Analysis Sets for Primary Endpoint:			
mITT Analysis Set <sup>1</sup>	N	N	n
Per Protocol Analysis Set <sup>2</sup>	N	N	n

<sup>1</sup> modified intention to treat (mITT) analysis set will consist of all subjects enrolled in PERL, randomized to study medication who received *at least one dose* of study medication.

<sup>2</sup> per-protocol analysis set will consist of a subset of mITT subjects. The per-protocol analysis set will exclude data points which 1. had cumulative exposure to the study medication from randomization less than 80% of the theoretical full exposure; or 2. with major protocol deviations (e.g., treatment with prohibited medications).

Note: N\* = number assessable at this point, i.e., denominator reflects loss from WD, LFU & deaths;

**Table 7.3. Demographics and Baseline Characteristics by Treatment Group. ITT Analysis Set.**

Variable Statistic or Category	Treatment Group	
	Allopurinol N=	Placebo N=
<b>Age (years)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>BMI (kg/m<sup>2</sup>) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>Diabetes duration (years)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>SBP (mm Hg) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>DBP (mm Hg) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>BP (Visit 4), n (%)</b>		
SBP > 140 or DBP > 90 mm Hg	xx (xx%)	xx (xx%)



SBP ≤140 and DBP ≤90 mm Hg	xx (xx%)	xx (xx%)
Missing	xx (xx%)	xx (xx%)
<b>On RASB at Visit 2</b>	xx (xx%)	xx (xx%)

**Table 7.3. Demographics and Baseline Characteristics by Treatment Group. ITT Analysis Set. (continued)**

Variable Statistic or Category	Treatment Group	
	Allopurinol N=	Placebo N=
<b>Gender, n (%)</b>		
Male	xx (xx%)	xx (xx%)
Female	xx (xx%)	xx (xx%)
<b>Ethnicity, n (%)</b>		
Hispanic or Latino	xx (xx%)	xx (xx%)
Not Hispanic or Latino	xx (xx%)	xx (xx%)
Unknown	xx (xx%)	xx (xx%)
<b>Race, n (%)</b>		
American Indian or Alaska Native	xx (xx%)	xx (xx%)
Asian	xx (xx%)	xx (xx%)
Black or African American	xx (xx%)	xx (xx%)
Native Hawaiian or Other Pacific Islander	xx (xx%)	xx (xx%)
White	xx (xx%)	xx (xx%)
Multi-Race	xx (xx%)	xx (xx%)
Other, Unknown, or not reported	xx (xx%)	xx (xx%)

**Table 7.4. Baseline Laboratory Values by Treatment Group. ITT Analysis Set.**

Variable Statistic or Category	Treatment Group	
	Allopurinol N=	Placebo N=
<b>HbA1c (Visit 1) (%)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>HbA1C (Visit 1), n (%)</b>		
≤7.8%	xx (xx%)	xx (xx%)
>7.8%	xx (xx%)	xx (xx%)
Missing	xx (xx%)	xx (xx%)
<b>Serum Uric Acid (mg/dL) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>Serum Uric Acid (Visit 4), n (%)</b>		
≤6 mg/dL	xx (xx%)	xx (xx%)
> 6 mg/dL	xx (xx%)	xx (xx%)
Missing	xx (xx%)	xx (xx%)
<b>eGFR (ml/min/1.73 m<sup>2</sup>) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>iGFR (ml/min/1.73 m<sup>2</sup>) (Visit 4)</b>		
Mean (SD)	xx (xx%)	xx (xx%)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx (xx%)	xx (xx%)
N missing	xx (xx%)	xx (xx%)
<b>AER (ug/min) (Geometric mean for Visits 3 and 4)</b>		
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x

<b>AER, n (%) (Geometric mean for Visits 3 and 4)</b>		
<20 ug/min	xx (xx%)	xx (xx%)
20-199 ug/min	xx (xx%)	xx (xx%)
≥200 ug/min	xx (xx%)	xx (xx%)
Missing	xx (xx%)	xx (xx%)

**Table 7.4. Baseline Laboratory Values by Treatment Group. ITT Analysis Set. (continued)**

Variable Statistic or Category	Treatment Group	
	Allopurinol N=	Placebo N=
<b>Potassium (mmol/L) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>Hemoglobin (gm/dl) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>Platelets (mmol) (Visit 4)</b>		
Mean (SD)	xx.x (xx.xx)	xx.x (xx.xx)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx, xx	xx, xx
N missing	x	x
<b>White Blood Cell (cells/mcL) (Visit 4)</b>		
Mean (SD)	xx (xx%)	xx (xx%)
Median (Q1, Q3)	xx.x (xx.x, xx.x)	xx.x (xx.x, xx.x)
Min, Max	xx (xx%)	xx (xx%)
N missing	xx (xx%)	xx (xx%)

## 7.1. Analyses of Primary and Secondary Outcomes

### P1. Primary Analysis of the Primary Endpoint: iGFR at the end of the 2-month wash-out period (Visit 17). mITT Analysis Set<sup>1</sup>.

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
iGFR Predicted Means at the end of the 2-month wash-out period (Visit 17) (95% CI) <sup>2</sup>	x.xx (x.xx, x.xx)	x.xx (x.xx, x.xx)
Treatment Effect at Visit 17 (95% CI)	x.xx (x.xx, x.xx)	
p-value	0.xxxx	

<sup>1</sup> Results are obtained from a linear model with correlated errors. The dependent variable is iGFR measured at visits V11, V16 and V17. Treatment effect at V17 was adjusted for stratifying variables (serum uric acid, HbA1c, study center), albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, baseline iGFR.

<sup>2</sup> Predicted means are calculated for an exemplary subject assuming that he/she is albuminuric from the Joslin site, with uric acid posited at 6 mg/dL, HbA1c at 8%, baseline AER geometric mean at 80 µg/min and baseline iGFR at 70 ml/min/1.73 m<sup>2</sup>. Covariate values for this exemplary subject were set close to their median values for continuous variables and most frequent categories for categorical variables

### S\_1. iGFR at the end of the 3-year treatment period (Visit 16, before the washout). mITT Analysis Set.<sup>1</sup>

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
iGFR Predicted Means at Visit 16, before the wash-out period (95% CI) <sup>2</sup>	x.xx (x.xx, x.xx)	x.xx (x.xx, x.xx)
Treatment Effect at Visit 16 (95% CI)	x.xx (x.xx, x.xx)	
p-value	0.xxxx	

<sup>1</sup> Results are obtained from a linear model with correlated errors employed for the Primary Analysis of the Primary Endpoint.

<sup>2</sup> Predicted means are calculated for an exemplary subject assuming that he/she is albuminuric from the Joslin site, with serum uric acid posited at 6 mg/dL, HbA1c at 8%, baseline AER geometric mean at 80 mg/min and baseline iGFR at 70 ml/min/1.73 m<sup>2</sup>. Covariate values for exemplary subject were set close to their median values for continuous and most frequent categories for categorical variables.

**S\_2. iGFR time trajectory estimated from repeated iGFR measurements. mITT Analysis Set.<sup>1</sup>**

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
<b>iGFR slope (95% CI)</b>	<b>x.xx (x.xx, x.xx)</b>	<b>x.xx (x.xx, x.xx)</b>
<b>Treatment Effect (difference between Allopurinol versus Placebo iGFR slopes) (95% CI)</b>	<b>x.xx (x.xx, x.xx)</b>	
<b>p-value</b>	<b>0.xxxx</b>	

<sup>1</sup>iGFR slope estimates and 95% CIs are obtained from a linear mixed-effects model for repeated iGFR measures. Fixed effects included stratifying variables: serum uric acid, HbA1c, study center, time since randomization in days, time by treatment group interaction, albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, baseline iGFR. Random effects included subject-specific intercepts and slopes for iGFR

**S\_3. eGFR at 4 months after randomization (Visit 7). mITT Analysis Set<sup>1</sup>.**

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
<b>eGFR Predicted Means at Visit 7 (95% CI)<sup>2</sup></b>	<b>x.xx (x.xx, x.xx)</b>	<b>x.xx (x.xx, x.xx)</b>
<b>Treatment Effect at Visit 7 (95% CI)</b>	<b>x.xx (x.xx, x.xx)</b>	
<b>p-value</b>	<b>0.xxxx</b>	

<sup>1</sup>Results are obtained using a linear model with independent residual errors. Fixed effects, included stratifying variables (serum uric acid, HbA1c, study center), treatment group, albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, baseline eGFR.

<sup>2</sup>Predicted means are calculated for an exemplary subject assuming that he/she is albuminuric from the Joslin site, with serum uric acid posited at 6 mg/dL, HbA1c at 8%, baseline AER geometric mean at 80 mg/min and baseline eGFR at 70 ml/min/1.73 m<sup>2</sup>. Covariate values for this exemplary subject were set close to their median values for continuous variables and most frequent categories for categorical variables

**S\_4. eGFR time trajectory. mITT Analysis Set.<sup>1</sup>**

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
eGFR slope (95% CI)	x.xx (x.xx, x.xx)	x.xx (x.xx, x.xx)
Treatment Effect (difference between Allopurinol versus Placebo eGFR slopes) (95% CI)	x.xx (x.xx, x.xx)	
p-value	0.xxxx	

<sup>1</sup>eGFR slope estimates and 95% CIs are obtained from a linear mixed-effects model for repeated eGFR measures. Fixed effects included stratifying variables: serum uric acid, HbA1c, study center, time since randomization in days, time by treatment group interaction, albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, baseline eGFR. Random effects included subject-specific intercepts and slopes for eGFR.

**S\_5. Time to serum creatinine doubling or ESRD. mITT Analysis Set.**

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
N (%) of subjects with doubled serum creatinine or ESRD during the course of the study	Xx (xx.x%)	Xx( xx.x%)
Adjusted Hazard Ratio (95% CI) <sup>1</sup>	x.xx (x.xx, x.xx)	
p-value <sup>1</sup>	0.xxxx	

<sup>1</sup>based on Cox Proportional Hazards Model. Fixed effects, included stratifying variables (serum uric acid, HbA1c, study center), treatment group, albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, and baseline iGFR.

**S\_6. Urinary AER at the end of the wash-out period. mITT Analysis Set.<sup>1</sup>**

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
Predicted Urinary AER at the end of the wash-out period (95% CI) <sup>2</sup>	x.xx (x.xx, x.xx)	x.xx (x.xx, x.xx)
Treatment Effect at Visit 17 expressed as % difference (95% CI)	xx.x% (xx.x%, xx.x%)	
p-value	0.xxxx	

<sup>1</sup>Results are obtained using a linear model with independent residual errors. The dependent variable is geometric mean of two AER measures obtained at Visit 17 expressed on log base to 10 scale. Fixed



effects, included stratifying variables (serum uric acid, HbA1c, study center), treatment group, albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, and baseline iGFR.

<sup>2</sup>Predicted urinary AER values are calculated using antilog transformation for an exemplary subject assuming that he/she is albuminuric from the Joslin site, with serum uric acid posited at 6 mg/dL, HbA1c at 8%, baseline AER geometric mean at 80 mg/min and baseline iGFR at 70 ml/min/1.73 m<sup>2</sup>. Covariate values for exemplary subject were set close to their median values for continuous variables and most frequent categories for categorical variables

### S\_7. Urinary AER during the last three months of the treatment period (Visits 15 and 16). mITT Analysis Set.<sup>1</sup>

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
Predicted Urinary AER during the last three months of the treatment period (95% CI) <sup>2</sup>	x.xx (x.xx, x.xx)	x.xx (x.xx, x.xx)
Treatment Effect expressed as % difference (95% CI) between treatment groups	xx.x% (xx.x%, xx.x%)	
p-value	0.xxxx	

<sup>1</sup>Results are obtained using a linear model with independent residual errors. The dependent variable is geometric mean of two AER measures obtained at Visits 15 and 16 expressed on log base to 10 scale. Fixed effects, included stratifying variables (serum uric acid, HbA1c, study center), treatment group, albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, and baseline iGFR.

<sup>2</sup>Predicted urinary AER values are calculated using antilog transformation for an exemplary subject assuming that he/she is albuminuric from the Joslin site, with uric acid posited at 6 mg/dL, glycated hemoglobin at 8%, baseline AER geometric mean at 80 mg/min and baseline iGFR at 70 ml/min/1.73 m<sup>2</sup>. Covariate values for this exemplary subject were set close to their median values for continuous variables and most frequent categories for categorical variables

### S\_8. Time to fatal or non-fatal cardiovascular events. mITT Analysis Set.

Variable	Treatment Group	
	Allopurinol N=	Placebo N=
N (%) of Subjects with fatal or non-fatal CVD during the course of the study	xx (xx.x%)	xx (xx.x%)
Adjusted Hazard Ratio (95% CI) <sup>1</sup>	x.xx (x.xx, x.xx)	
p-value <sup>1</sup>	0.xxxx	

<sup>1</sup>based on Cox Proportional Hazards Model. Fixed effects, included stratifying variables (serum uric acid, HbA1c, study center), treatment group, albuminuria status, baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale, and baseline iGFR.

## 7.2. Analyses of Safety Outcomes.

**Table S1: Subjects Discontinuing Study Medication because of Severe Adverse Events by Treatment Group. mITT Subjects.**

	Treatment Group		Total
	Allopurinol N=	Placebo N=	
# of subjects d/c treatment	X	X	X
# of Subjects	X	X	X
% of subjects d/c treatment	x.x%	x.x%	x.x%
p-value*	x.xxxx		

\*p-value from Fisher's exact test comparing percentage of subjects with discontinuing treatment by treatment group

**Table S2: SAEs by Treatment Group, Regardless of Relatedness to Intervention. mITT Subjects.**

	Treatment Group		Total
	Allopurinol N=	Placebo N=	
# of SAE's	X	X	X
# of subjects with SAE's	X	X	X
# of Subjects	X	X	X
SAE's per subject	x.xx	x.xx	x.xx
% of subjects with SAE's	x.x%	x.x%	x.x%
p-value*	x.xxxx		

\*p-value from Fisher's exact test comparing percentage of subjects with SAEs by treatment group

**Table S3: Summary of Number of SAEs per Subject in the Pre- and Post-Randomization Periods for Non-Randomized and Randomized Subjects. mITT Subjects.**

Number of SAEs per Subject	Allopurinol N=	Placebo N=	Total N=
1	N (xx.x%)	N (xx.x%)	N
2	N (xx.x%)	N (xx.x%)	N
3	N (xx.x%)	N (xx.x%)	N
≥4	N (xx.x%)	N (xx.x%)	N
Relative Risk (95% CI) <sup>1</sup>	x.xx (x.xx, x.xx)		

<b>Number of SAEs per Subject</b>	Allopurinol N=	Placebo N=	Total N=
p-value <sup>1</sup>	0.xxxx		

<sup>1</sup>Based on Poisson regression model with treatment as a covariate and follow-up time as an offset

**Table S4: SAEs by Treatment Group, Regardless of Relatedness to Intervention. mITT Subjects.**

<b>BODY SYSTEM</b>	Treatment Group		<b>TOTAL</b>
	Allopurinol N=	Placebo N=	
Congenital	N	N	N
Gastrointestinal	N	N	N
Hepatic	N	N	N
Immunological	N	N	N
Infectious	N	N	N
Metabolic	N	N	N
Miscellaneous	N	N	N
Neoplastic	N	N	N
Neurological	N	N	N
Nutritional	N	N	N
Orthopedic	N	N	N
Pulmonary	N	N	N
Surgical	N	N	N
Total SAEs	N	N	N
Total Subjects with SAEs	N	N	N
Total Subjects Randomized	N	N	N
% with SAEs	xx.x%	xx.x%	xx.x%
p-value*	0.xxxx		

\*p-value from Fisher's exact test comparing percentage of subjects with SAEs by treatment group

**Table S5: Subjects Discontinuing Study Medication because of Adverse Events by Treatment Group. mITT Subjects.**

	Treatment Group		<b>Total</b>
	Allopurinol N=	Placebo N=	
<b># of subjects d/c treatment</b>	X	X	X
<b># of Subjects</b>	X	X	X
<b>% of subjects d/c treatment</b>	x.x%	x.x%	x.x%
<b>p-value*</b>	x.xxxx		

\*p-value from Fisher's exact test comparing percentage of subjects with discontinuing treatment by treatment group

**Table S6: AEs by Treatment Group, Regardless of Relatedness to Intervention. mITT Subjects.**

	Treatment Group		<b>Total</b>
	Allopurinol N=	Placebo N=	
<b># of AE's</b>	X	X	X
<b># of subjects with AE's</b>	X	X	X
<b># of Subjects</b>	X	X	X
<b>AE's per subject</b>	x.xx	x.xx	x.xx
<b>% of subjects with AE's</b>	x.x%	x.x%	x.x%
<b>p-value*</b>	x.xxxx		

\*p-value from Fisher's exact test comparing percentage of subjects with AEs by treatment group

**Table S7: Summary of Number of AEs per Subject in the Pre- and Post-Randomization Periods for Non-Randomized and Randomized Subjects. mITT Subjects.**

<b>Number of AEs per Subject</b>	Allopurinol N=	Placebo N=	Total N=
1	N (xx.x%)	N (xx.x%)	N
2	N (xx.x%)	N (xx.x%)	N
3	N (xx.x%)	N (xx.x%)	N
≥4	N (xx.x%)	N (xx.x%)	N
<b>Relative Risk (95% CI)<sup>1</sup></b>	x.xx (x.xx, x.xx)		
<b>p-value<sup>1</sup></b>	0.xxxx		

<sup>1</sup>Based on Poisson regression model with treatment as a covariate and follow-up time as an offset

**Table S8: AEs by Treatment Group, Regardless of Relatedness to Intervention. mITT Subjects.**

BODY SYSTEM	Treatment Group		TOTAL
	Allopurinol N=	Placebo N=	
Congenital	N	N	N
Gastrointestinal	N	N	N
Hepatic	N	N	N
Immunological	N	N	N
Infectious	N	N	N
Metabolic	N	N	N
Miscellaneous	N	N	N
Neoplastic	N	N	N
Neurological	N	N	N
Nutritional	N	N	N
Orthopedic	N	N	N
Pulmonary	N	N	N
Surgical	N	N	N
Total AEs	N	N	N
Total Subjects with AEs	N	N	N
Total Subjects Randomized	N	N	N
% with AEs	xx.x%	xx.x%	xx.x%
p-value*	0.xxxx		

\*p-value from Fisher's exact test comparing percentage of subjects with AEs by treatment group

**Table S9: Skin reaction Adverse Event by Treatment Group. mITT Subjects.**

	Treatment Group		TOTAL
	Allopurinol N=	Placebo N=	
Stevens-Johnson Syndrome (SJS)	x/x (xx%)	x/x (xx%)	x/x (xx%)
Skin rash	x/x (xx%)	x/x (xx%)	x/x (xx%)
Subjects with SJS or skin rash	X	x	N
Total Subjects with Skin reaction Assessed	x	x	N
% with skin reaction AEs	xx.x%	xx.x%	xx.x%
p-value*	0.xxxx		

\*p-value from Fisher's exact test comparing percentage of subjects with expected AEs by treatment group

**Figure S1a. Time to first SAE during On-Study Drug Period, with Log-Rank Test to Compare Treatment Groups. mITT Subjects.**

Kaplan Meier curve of time from randomization to first SAE by treatment group; subjects censored at earliest of death, withdrawal, lost-to-follow-up, or end of study medication provided subject didn't have an SAE.

Y axis label = % of Subjects (100%, 90%, ... , 10%, 0%) without SAE

X axis label = Time (days) post-randomization (0, 6, 12, 18, 24, 30, 36 months)

**Figure S1b. Time to first SAE during Off-Study Drug Period, with Log-Rank Test to Compare Treatment Groups. mITT Subjects.**

Kaplan Meier curve of time from end of study medication to first SAE by treatment group; subjects censored at earliest of death or completion of study (withdrawal, lost-to-follow-up or end of study).

Y axis label = % of Subjects (100%, 90%, ... , 10%, 0%) without SAE

X axis label = Time (months) post-treatment (0, 6, 12, 18, 24, 30, 36 months)

**Additional Descriptive Statistics by Treatment Group**

**Table A1a. Comparison of iGFR by Treatment Groups. mITT Analysis Set.**

Visit	Treatment Group		LSMean Treatment Difference (95% CI)	p-value
	Allopurinol N=	Placebo N=		
iGFR at Visit 4				
N	xx	xx		
Mean (SD)	xx.x (x.xx)	xx.x (x.xx)		
Min, Max	xx.x, xx.x	xx.x, xx.x		
N missing	xx	xx		
iGFR at Visit 11				0.xxxx
N	xx	xx		
Mean (SD)	xx.x (x.xx)	xx.x (x.xx)	x.x	
Min, Max	xx.x, xx.x	xx.x, xx.x	(x.x, x.x)	
N missing	xx	xx		
iGFR at Visit 16				0.xxxx
N	xx	xx		
Mean (SD)	xx.x (x.xx)	xx.x (x.xx)	x.x	
Min, Max	xx.x, xx.x	xx.x, xx.x	(x.x, x.x)	
N missing	xx	xx		
iGFR Visit 17				0.xxxx
N	xx	xx		
Mean (SD)	xx.x (x.xx)	xx.x (x.xx)	x.x	
Min, Max	xx.x, xx.x	xx.x, xx.x	(x.x, x.x)	
N missing	xx	xx		

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## **APPENDIX I. Study Objective, Study Design, Outcomes & Statistical Analysis and Data Management Sections from Protocol**

In this appendix, selected sections (from protocol, version 9, approved by DSMB on August 16th, 2016) are included for reference. The following sections/figures from the study protocol are included:

- 2. Study Objective
- 3. Study design
- 7.1. Primary outcomes
- 7.2. Secondary outcomes
- Schedule of events (original figure on p. 27 in the study protocol)
- 9. Safety assessments
- 10. Adverse Event Reporting
- 11. Statistical Analysis



## **2. STUDY OBJECTIVE**

To determine whether lowering serum UA by means of oral allopurinol is effective in preventing or slowing decline of renal function in T1D patients with microalbuminuria or moderate macroalbuminuria who still have only mildly or moderately impaired kidney function.

### **3. STUDY DESIGN**

The study will be a multi-center, double-blind, placebo-controlled, parallel-group randomized clinical trial including a total of 480 patients with type 1 diabetes (T1D) who are at high risk for GFR loss because of increased albuminuria and a relatively high serum UA ( $\geq 4.5$  mg/dl), but have only mildly or moderately decreased renal function.

## 7. STUDY OUTCOMES

### 7.1. Primary outcome

The primary outcome will be the iGFR at the end of the 2-month wash-out period following the 3-year treatment period, measured by the plasma clearance of non-radioactive iohexol (iGFR) and adjusted for the iGFR at baseline. The rationale of measuring the primary outcome at the end of the wash-out period is to test allopurinol for permanent effects of on the natural history of kidney disease, independent from any transient, hemodynamic effect that the medication may have on GFR. Plasma iohexol clearance has been shown to provide accurate and reproducible GFR measurements.<sup>30,31</sup> It is highly correlated with inulin clearance (the gold standard to measuring GFR)<sup>32</sup> and is a safe, cost-effective method to test hundreds of patients enrolled in multicenter clinical trials.<sup>33</sup> The method consists of injecting a 5 mL bolus of Iohexol (Omnipaque, 300 mg iodine/mL) and drawing blood samples at baseline and 120, 150, 180, 210, and 240 minutes after the injection. Plasma concentrations of iohexol at different time points are measured by HPLC and used to calculate the plasma clearance of iohexol ( $Cl = \text{Dose}/\text{AUC}$ , where AUC is the area under the plasma concentration time curve), which is taken after appropriate body surface area corrections as a measure of GFR.<sup>30,31</sup>

### 7.2. Secondary outcomes

1. Iohexol-clearance GFR at the end of the 3-year treatment period (before the washout).
2. Iohexol-clearance GFR time trajectory estimated from periodical iohexol-GFR measurements.
3. Estimated (eGFR) at 4 months estimated from serum creatinine and cystatin C and adjusted for the eGFR at baseline.
4. Estimated GFR (eGFR) time trajectory estimated from quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations.<sup>34,35</sup>
5. Time to doubling of baseline serum creatinine value or ESRD (eGFR  $\leq 15$  ml/min/1.73 m<sup>2</sup>, institution of dialysis, kidney transplantation).
6. Geometric mean of two AER measurements at the end of the 2-month wash-out period following the 3-year treatment period, adjusted for the mean urinary AER at baseline. Urinary AER will be determined in timed overnight urine collections brought by study participants to regular clinic visits, and expressed in  $\mu\text{g}/\text{minute}$  and as urinary albumin/creatinine ratios.
7. Geometric mean of urinary AER during the last three months of the treatment period (Visits 15 and 16), adjusted for the mean urinary AER at baseline.
8. Time to fatal or non-fatal cardiovascular events, defined as the composite of CVD death (ICD-10 code I10 to I74.9), myocardial infarction, stroke (ischemic or hemorrhagic), coronary artery bypass grafting, or percutaneous coronary intervention.

**Figure 1. Schedule of Events**

Year	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	3	3	3
Week	-12	-9	-7	-3		0	4	16	32	48	64	80	96	112	128	142	156	164
Visit #	1	2	3	4	4a*	5	6	7	8	9	10	11	12	13	14	15	16	17
*Type of Visit: In-Person Visit Required (V); Phone Call (C); Other Visit (In-Person or Remote Visit, O)	O	V	O	V	V	C	O	O	O	O	O	V	O	O	O	O	V	V
EVENT	Screen	Run-in				Allopurinol or placebo												Wash-out
						RANDU 100 mg	200-400 mg											
Informed Consent	x	x																
Demographics	x																	
Initial Medical Hx		x																
Interval Medical Hx and BP Control			x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Concomitant Meds	x	x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Blood Pressure and Measurements	x	x	(x)	x	(x)		(x)	(x)	(x)	(x)	(x)	x	(x)	(x)	(x)	(x)	(x)	x
EKG Report		x		x	(x)													
Physical Exam		x		(x)	(x)													
Skin Assessment				x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Eligibility	x			x	(x)	x												
Randomization					(x)	x												
Family History				x	(x)													
RAS and BP Med Log		x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
IGFR Procedure				x	(x)													
PERL Study Drug Prescription						x	x	x	x	x	x	x	x	x	x	x	x	x
Study Drug Compliance							x	x	x	x	x	x	x	x	x	x	x	x
<b>CENTRAL LAB</b>																		
Serum uric acid, serum creat, cystatin C	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Urine ACR/AER	x		x	x	(x)			x	x	x	x	x	x	x	x	x	x	x
HbA1c	x			x	(x)			x	x	x	x	x	x	x	x	x	x	x
HLA B*58:01				x	(x)													
IGFR				x	(x)								x					x
NIDDK Repository: serum, plasma, urine				x									x					x
<b>LOCAL LAB</b>																		
Pregnancy test serum HCG	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Pregnancy test urine dipstick		x			(x)													
ALT, K, CBC, serum creatinine, urine	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Protocol Deviation		x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Adverse Events		x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x

\*If normal blood pressure control is not achieved at Visit 4, the run-in period may be extended for two more weeks after which participants will be examined as in Visit 4 (Visit 4A). In this event, the GFR measurement scheduled for Visit 4 will be conducted at Visit 4A.

^ Study visits will be generally conducted at the Study Sites or their Satellites. "In-Person Visits" (V) are required for Visit 2 and all visits requiring iohexol-GFR measurements. If a participant lives far from a study site or satellite, or travel impediments are present, other (O) visits may be conducted remotely or in-person. For any given study visit to be conducted remotely, a Phone Visit and a Remote Biospecimen Collection will be both required; a Phone Visit is performed by the study coordinator using the telephone or other media such as Skype to collect results of study procedures that do not require physical interactions (e.g., collection of medical history), and a Remote Biospecimen Collection is performed at a clinical laboratory close to where participants live.

Note: (x) indicates an optional assessment; For "BP and Measurements", (x) indicates an optional assessment only if the patient is NOT seen in-person.

## **9. SAFETY ASSESSMENTS**

### **9.1. Demographic Data/Medical History**

After collecting a detailed medical history at Visit 1, this information will be updated at each visit through a structured interview, with a special emphasis on skin symptoms and signs such as rash, itching and exfoliation and on pregnancy in females. Participants will be instructed to communicate any change in their health status and intervening hospitalizations to the study coordinator in-between visits. In particular, they will be instructed to discontinue study medication and immediately contact the study coordinator if they develop a suspicious skin rash, swelling of the lips or mouth, arthralgias, and/or jaundice, which may indicate a hypersensitivity reaction to allopurinol. Fever and chills should also be reported but would not require cessation of medication prior to discussion with study personnel.

### **9.2 Skin exam**

The skin of study participants will be examined for the presence of any kind of rash at each in-person visit. Participants will be instructed to carry-out periodical skin self-exams. If skin abnormalities are reported to the study personnel during the phone visits or on any other occasion, participants will be asked to immediately report to the study site, their PCP's office, or other local healthcare facilities for an in-person skin exam. Suspicion of drug allergy or Stevens-Johnson Syndrome SJS would require immediate discontinuation of study medication and dermatologic consultation.

### **9.3. Vital Signs**

Blood pressure and heart rate will be recorded at each in-person visit. BP readings at home will be reviewed during each phone visits; if abnormal values are reported, participants will be asked to visit the study site, their PCP's office, or other local healthcare facilities to have their BP measured.

### **9.4. Clinical Laboratory Tests**

Serum ALT, creatinine and K<sup>+</sup>, and CBC will be monitored and a pregnancy test, if a female of child bearing potential, performed at each visit. Participants who are started for the first time on RAS blockers as part of this study will have their serum K<sup>+</sup> and creatinine measured at a local laboratory after 2 weeks of full dose RASB treatment (i.e., after Visit 3). HbA1c will be measured at Visits 1, 4, and 7-17. An ECG will be performed at Visits 2, 4, 11, and 16.

### **9.5. Management of Uric Acid Levels**

Study participants and study personnel, other than the DCC and the study pharmacists, will be masked as to the uric acid levels obtained during the study. The patients' physicians will receive written requests to refrain from measuring uric acid levels during the time of the patients' participation in the study, except as is mandatory for the patient's wellbeing, e.g., in the treatment of malignancy or diagnosis of a clinical syndrome highly likely to represent gout. If gout is diagnosed, open-label treatment with allopurinol will become indicated. In such case, the study drug will be discontinued but the patient will remain in the study and will continue to be followed as if he/she was taking the study medication. If uric acid lowering for malignancy treatment is required, the patient will receive open-label treatment until such time as return to study drug is deemed clinically reasonable by their physician.



## **10. ADVERSE EVENT REPORTING**

### **10.1. Definitions**

An Adverse Event (AE) is any untoward medical occurrence in a study participant regardless of its relationship to study treatment. A treatment-emergent AE is an adverse event occurring during the period between the first dose and 30 days after the final dose of the study medication. A Serious Adverse Event (SAE) is any untoward medical occurrence that results in death, is life-threatening, requires hospitalization or prolongation of an existing hospitalization, results in persistent or significant disability, or is a congenital anomaly/birth defect. Important medical events that do not fall into the above categories may also be considered an SAE when, based on medical judgment, such events may jeopardize the patient's safety and require medical/surgical intervention to prevent one of the outcomes listed in the SAE definition. The term SAE is not intended as a measure of severity or intensity. All AE's/SAE's that occur after the time of informed consent will be reported.

A Suspected Adverse Reaction is any adverse event for which there is a reasonable possibility that the drug caused the adverse event. For the purposes of IND safety reporting, "reasonable possibility" means there is evidence to suggest a causal relationship between the drug and the adverse event. Suspected adverse reaction implies a lesser degree of certainty about causality than adverse reaction, which means any adverse event caused by a drug. An Unexpected Adverse Event or Unexpected Suspected Adverse Reaction is an adverse event or suspected adverse reaction that is not listed in the investigator brochure or is not listed at the specificity or severity that has been observed; or, if an investigator brochure is not required or available, is not consistent with the risk information described in the general investigational plan or elsewhere in the current application, as amended. For example, under this definition, hepatic necrosis would be unexpected (by virtue of greater severity) if the investigator brochure referred only to elevated hepatic enzymes or hepatitis. Similarly, cerebral thromboembolism and cerebral vasculitis would be unexpected (by virtue of greater specificity) if the investigator brochure listed only cerebral vascular accidents. "Unexpected", as used in this definition, also refers to adverse events or suspected adverse reactions that are mentioned in the investigator brochure as occurring with a class of drugs or as anticipated from the pharmacological properties of the drug, but are not specifically mentioned as occurring with the particular drug under investigation. An Expected Adverse Event or Expected Adverse Reaction is any adverse experience that has been identified in nature or severity in the current investigator brochure and/or protocol.

### **10.2. Adverse Events Reporting**

All AEs will be reported on the Adverse Events form that will be completed by the study staff, who are masked as to study treatment assignment, at each regular follow-up visits. This will insure that AEs are ascertained in an unbiased manner using the same standardized methodology for participants in both treatment arms. Forms will include standardized questions relating to specific events of import in diabetic patients on either of the study treatment arms as well as any significantly abnormal physical finding identified on examination and any significantly abnormal laboratory results obtained on the patient between visits or at the time of the visit. AEs reported or ascertained between clinic visits will be captured and reported at the time of the next schedule visit. Pre-existing conditions (that is, any condition that was known to be present prior to the signing of informed consent or was identified during the screening procedures at Visit 1) will not be considered or recorded as AEs unless the condition worsens in intensity or frequency after Visit 1. Likewise, continuing AEs will not be reported as AEs at

subsequent visits unless they increase in severity or frequency between visits, they result in criteria for a SAE, and/or they resolve between visits. Each site will be responsible for reporting all AE's to their IRB according to its AE reporting policy and procedures.

### **10.3. Assessment of Causality and Severity**

The seriousness of adverse events will be ascertained by the study staff according to the criteria listed in 10.1 and the need for further evaluation, follow-up, or referral. The relationship between study participation and AEs will be determined according to the following criteria:

**A. Not related** – temporal relationship of the onset of the event, relative to study participation, is not reasonable or another cause can by itself explain the occurrence of the event.

**B. Possibly related** – temporal relationship of the onset of the event, relative to study participation, is reasonable but the event could have been due to another, equally likely cause.

**C. Probably related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and the event is more likely explained by the study treatment than by another cause.

**D. Definitely related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and there is no other cause to explain the event.

### **10.4. Serious Adverse Events Reporting**

See Section 15 – Data and Safety Monitoring Plan.



## **11. STATISTICAL ANALYSIS**

This section presents a summary of the planned statistical analyses. A statistical analysis plan (SAP) will be written for the study that contains detailed descriptions of the analyses to be performed. The SAP will be written prior to database lock.

### **11.1. Analysis Population**

For most of the analyses, including the primary efficacy analysis described in section 11.3, an intention to treat (ITT) analytical approach will be employed. Accordingly, the population for statistical analysis will consist of all randomized study participants considered in their original randomization group, regardless of treatment discontinuation or loss to follow-up.

Selected secondary efficacy analyses will be performed using a per-protocol analytical approach. In this case, the analysis population will consist of the ITT population excluding data points which 1. had cumulative exposure to the study medication from randomization that was less than 80% of the theoretical full exposure; or 2. during major protocol deviations (e.g., treatment with prohibited medications), which could affect primary outcome.

### **11.2. Initial Data Analysis**

The initial data analysis will be performed to detect any differences in distributions of characteristics measured at baseline, 4, 20, 36, and 38 months (0, 16, 80, 156, and 164 weeks, respectively) between study groups. The number of patients screened, enrolled, and completing the study will be summarized within and across study centers. Measures of central tendency (means, medians) and variability (standard deviations, ranges) will be estimated from the data for continuous variables. Frequency distributions will be provided for categorical data. This preliminary analysis step will provide us with insight into data, distributions of the variables considered, and will allow us to find additional invalid values not detected earlier during data validation.

### **11.3. Primary Efficacy Analysis**

For the primary endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention), we will follow the recommendations by Carpenter et al<sup>38,39</sup> and perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable. By conditioning on the baseline iGFR measure we will also effectively use this variable as a covariate. Treatment group, study center, stratifying variables, albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did not qualify by eGFR slope and were normoalbuminuric at baseline), baseline AER, time, and time by treatment interaction will also be included as covariates in the model. Three features make this analytical approach especially attractive:

1. If there is no dropout (a very unlikely case), the estimate of the treatment effect at the end of the 2-month wash-out period following the 3-year intervention and its precision obtained using this approach will be exactly the same as those based on a classical approach employing an analysis of covariance (ANCOVA) model with treatment group, study center, iGFR and AER/ACR measured at baseline included as covariates.
2. If the iGFR measure at the end of the wash-out period is missing, we will be able to efficiently use the information contained in the intermediate iGFR measurements

obtained at 80 and 156 weeks, by virtue of them being correlated with the GFR measurement at washout. Estimate of the treatment effect obtained this way is valid under the missing at random (MAR) assumption. This is in contrast to the ANCOVA approach, which would lead to the loss of this information and would require a more stringent assumption about the mechanism of data missingness, i.e. a missing completely at random (MCAR) mechanism.

3. The underlying analytical framework allows the use of all post-randomization data and is well suited to investigate the reason for withdrawal, for example to study whether participants having low iGFR values are more likely to withdraw.

Calculations will be performed using SAS PROC/MIXED. Results of the analysis will be expressed in terms of point estimate and its corresponding 95% confidence interval for the treatment effect at the end of the 2-month wash-out period following the 3-year treatment and will be accompanied by the corresponding p value.

#### **11.4. Secondary Efficacy Analyses**

1. The effect of treatment on the iGFR at the end of the 3-year treatment period (before the washout) will be evaluated using the same analytical approach employed for the primary outcome.
2. The effect of treatment on the eGFR at 4 months after randomization will be evaluated using the same analytical approach employed for the primary outcome.
3. The iGFR and eGFR time trajectories, estimated from periodical iGFR measures and quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations<sup>34,35</sup>, respectively, will be analyzed using linear mixed-effects models.<sup>40-42</sup> The main objective of the analysis will be to construct confidence interval for the effect of the intervention over three years of observation (treatment main effect) and investigate whether the effect of the intervention changes with time (time by treatment interaction).
4. Time to serum creatinine doubling or ESRD in the two treatment groups is subject to censoring due to dropouts or reaching the end of study before the participant experiences the event. Survival time will be defined as the time from randomization to the event (the first of serum creatinine doubling from baseline or occurrence of ESRD, defined as  $eGFR \leq 15$  ml/min/1.73 m<sup>2</sup>, hemodialysis, or kidney transplant) or, for participants who did not experienced an event, to the last study visit. Data will be summarized by means of Kaplan-Meier survival curves and by providing the proportions of participants surviving without events at 1, 2, 3 years, and at the end of the wash-out period along with their 95% CIs. Given the potentially small number of events, differences between study groups will be tested by means of the log rank test or by means of simple Cox regression models including a limited number of predictors in addition to treatment group.
5. The effect of treatment on the AER at the end of the wash-out period, based on the geometric mean of two AER measured at this time point and adjusted for the geometric mean of AER at baseline (Visit 3 and 4), will be investigated in a linear regression model framework as in the case of the primary outcome.
6. The effect of treatment on the AER at the end of the treatment period, based on the geometric mean of the AER measures at visit 15 and 16 adjusted for the geometric mean of AER at baseline (Visit 3 and 4) will be investigated as in #5.

7. Time to fatal or non-fatal cardiovascular events will be analyzed as proposed for time to serum creatinine doubling or ESRD.
8. We will perform a per-protocol analysis (as defined in 11.1) for the primary efficacy endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention).

### **11.5. Incomplete Data**

Missing values represent a potential source of bias. Efforts will be made to keep all participants in the study. If this is not feasible, at least some information regarding the status at the end of the trial will be obtained. For randomized patients, the number of completing and dropouts will be summarized. This procedure will help to compare characteristics of the participants' groups who drop out from the study with those who completed the study by treatment group, within and across study centers. The models considered in the proposal allow for a missing at random (MAR) mechanism. MAR means that the missing values mechanism can be explained by observed data and does not depend on the *unobserved* values of outcome measures. The differences in distributions between characteristics of the groups may indicate potential sources of bias due to missing values. For instance, some patients may dropout from the study due to *unobserved* factors related to the intervention itself. If we suspect such bias is present, the methods discussed in this section, assuming (MAR), are not applicable. We will incorporate plausible missing values mechanism into the model as discussed in Little<sup>43</sup> and investigate how such mechanism may affect the estimates of treatment effect. To this end, sensitivity analyses will be conducted involving selection and/or pattern-mixture models<sup>44</sup> with an appropriate submodel used to describe dropout.

### **11.6. Pilot participants**

All pilot participants who were already randomized to allopurinol or placebo during the pilot will be included in the final analysis of the pivotal trial. Those who do not consent to the pivotal trial will be treated as having dropped from the study at a time corresponding to their last pilot visit. Sensitivity analyses will be performed to investigate whether results may be potentially affected by the roll-over of pilot subjects in the pivotal trial.

### **11.7. Model assumptions and alternative analyses**

Model assumptions will be thoroughly checked for individual and systematic departures, using informal, e.g. inspection of residuals, and formal methods such as score test for extra parameter or methods based on likelihood displacement. If individual outliers are detected, their influence will be evaluated using influence diagnostics methods based on comparing estimates from models fitted to data with and without outlying values. Whenever we are not successful in fitting the parametric model (linear or non-linear), then non-parametric analyses and/or transformation of the variables involved in the analysis will be considered. To investigate the potential hemodynamic influence of allopurinol on treatment effect, in addition to the aforementioned analyses, we will consider models including the post-randomization measure of GFR at 4 months as an additional covariate. To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses (based on the primary efficacy analysis described in section 11.3, with the inclusion of an interaction term of the treatment group by the subgroup variable) will be performed by age groups ( $\leq 40$  and  $> 40$  yrs), gender, racial/ethnic group, HbA1c ( $\leq 7.8$  and  $> 7.8\%$ ), serum uric acid ( $\leq 6.0$  and  $> 6.0$  mg/dl), baseline iGFR ( $\leq 70$  ml/min and  $> 70$  ml/min/1.73m<sup>2</sup>), AER at baseline ( $\leq 300$  and  $> 300$  mg/24 hr), and albuminuria status (subjects who qualified by ACR or AER or were albuminuric at

baseline vs. subjects who did qualify by eGFR slope and were normoalbuminuric at baseline). To investigate possible influence of using selected covariates on the treatment effect estimate in the models considered in Section 11, we will perform appropriate sensitivity analyses. These additional analyses will be considered as strictly exploratory.

### 11.8. Safety Analyses

Adverse events will be independently reviewed by an independent data safety monitoring board (DSMB, see Sections 15 and 16). All safety data will be available in data listing in the clinical protocol report. Data will be described in terms of descriptive statistics and presented by treatment group. Presentation will include graphs (scatterplots, boxplots, histograms), measures of central tendency (mean, median) and variability (confidence intervals) for continuous variables and frequency tables for categorical variables.

### 11.9. Interim Analysis

No formal interim analyses of efficacy to stop for benefit or futility are planned, given the timing of the primary endpoint.

### 11.10. Sample Size

Since a variance-covariance matrix for the iGFR measures is not available and this matrix is essential in order to perform formal power calculations for a model with correlated errors, we performed alternative power calculations based on an intent-to-treat analysis within an ANCOVA framework. Specifically, we assumed that the primary hypothesis is tested in the following model:

$$M1: \text{iGFR at washout} = \text{iGFR at baseline} + \text{treatment group}$$

Compared to the model that will be used in the primary analysis, model M1 is simplified in two aspects. First, it does not use information from iGFR values measured at intermediate time points. Second, it does not include covariates such as the stratifying variables (HbA1c and UA) or other GFR predictors such as baseline AER. Both of these aspects may lead to loss of precision of the treatment effect estimate. Consequently, our sample size calculations should be considered as conservative.

The hypothesis being tested, i.e. the effect of treatment on iGFR at washout, corresponds to testing whether the treatment group factor in Model M1 is significant. The choice of the ANCOVA model for the purpose of power calculations is sensible, as residuals from a univariate model involving baseline iGFR as covariate fitted to data from RASS study conform to normal distribution. Sample size calculations were performed based on Cohen<sup>45</sup> and making the following assumptions:

1. Postulated effect on iGFR at washout ( $\bullet$ ) = 3 ml/min/1.73 m<sup>2</sup>. We deem this effect to be clinically meaningful and attainable. It is clinically meaningful because it would translate on average into a 10-year delay in the progression to ESRD. It is attainable because it is smaller than the difference in 3-year GFR that we observed in the JKS between subjects with serum UA  $\geq$  4.5 mg/dl compared to those with levels below this value. The postulated effect was based on the following changes in GFR levels in the two treatment groups:
  - a. Untreated group = 3 ml/min/1.73 m<sup>2</sup> per year. This estimate is based on data from the Joslin Kidney Study (JKS), in which the median GFR loss among 43 subjects meeting the above criteria was 3.1 ml/min/1.73 m<sup>2</sup> per year, with 70% of subjects having a GFR loss  $>$ 1.5 ml/min/1.73 m<sup>2</sup> per year. Also, among 116

subjects from Steno who met the albuminuria and GFR criteria, but for whom serum uric acid values were not available, the median GFR loss was 3.3 ml/min/1.73 m<sup>2</sup> per year, with 71% of subjects having a GFR loss >1.5 ml/min/1.73 m<sup>2</sup> per year.

b. Treated group = 2 ml/min/1.73 m<sup>2</sup> per year. The average GFR loss in the JKS subjects with serum UA <4.5 mg/dl was 1.5 ml/min per year. On this basis, we conservatively assumed that the allopurinol treatment, if effective, would decrease the GFR loss to 2 ml/min per year (a 33% decrease compared to the untreated group).

2. Standard deviation (SD) of residual error = 10.1 ml/min/1.73 m<sup>2</sup>. This was estimated based on the root-mean-squared error from a regression model with eGFR at 3 yrs as the dependent variable and baseline eGFR as the independent variable fitted to data concerning T1D patients from the Joslin Kidney Study meeting the PERL inclusion criteria.

Assuming a two-sided alpha error equal to 0.05, the effective sample size needed to detect the pre-specified treatment effect ( $\delta = 3 \text{ ml/min/1.73 m}^2$ ) at washout adjusted for baseline iGFR with 80% power is equal to n=180 per group. To take into account the anticipated overall dropout rate (up to 5%/yr or 15% over the entire duration of the study) and drug discontinuation or non-compliance in the treatment group (up to 2%/yr or 6% over the entire duration of the study), and to maintain the desired

power of at least 80%, it will be necessary to recruit n=240 subjects per group. In Table 1, we show the power of the proposed sample size for Model M1 under different dropout and non-compliance scenarios. We also provide the corresponding power for a model (Model M2) including the two stratifying variables (Hb1Ac and UA) and baseline AER as covariates to illustrate the effect of adding these variables to Model M1. In this analysis, we assumed that adding these covariates reduces the residual variance by 10%, which corresponds to these covariates explaining merely 4% of the total iGFR variation over and above the variability explained by iGFR at baseline. As shown in Table 1, once these covariates are accounted for, power is expected to exceed the conservative estimates provided by Model M1 and reach almost 90% for 15% dropout and 6% non-compliance rates.

**Table 1. Power to detect treatment effect for two ANCOVA models under different drop-out and non-compliance scenarios.**

Overall Dropout (%)	Non-compliance (%)	Model	
		M1	M2
9	0	.87	.92
12	0	.86	.91
15	0	.85	.90
9	6	.83	.89
12	6	.82	.88
15	6	.80	.87



**PERL**  
PREVENTING EARLY RENAL LOSS  
IN DIABETES

**A Multicenter Clinical Trial of Allopurinol to Prevent GFR Loss in  
Type 1 Diabetes**

**Statistical Analysis Plan  
Prepared by PERL DCC**

**August 3, 2019**

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## 1. Overview

### DESIGN:

- Multicenter, double-blind, placebo-controlled, parallel-group randomized clinical trial.
- N=530 total number of subjects

### STUDY POPULATION:

- Type 1 Diabetes
- Inclusion/Exclusion Criteria listed in the Study Protocol

### STUDY TREATMENTS:

- Oral allopurinol or placebo administered for 3 years followed by a 2-month drug washout

### PRIMARY OUTCOME MEASURE:

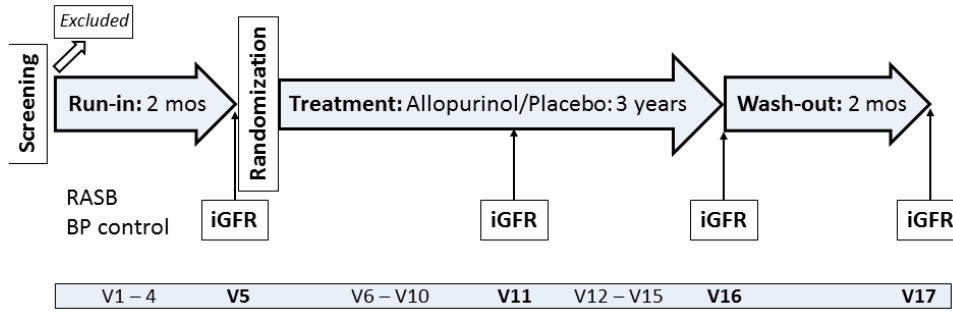
- iGFR at the end of the 2-month wash-out period following the 3-year intervention

### STATISTICAL ANALYSIS PLAN:

- This plan will be finalized prior to the database lock and unblinding of treatment groups

## 2. Schema

Figure 2.1. PERL Study Schema



### **3. Rationale for Adjustments of Statistical Analysis Plan as Compared to Protocol (Version 10, approved by DSMB on March 6, 2018)**

Changes from the protocol-specified definitions of aims, outcomes, and statistical analytical approaches are outlined below. These changes reflect internal discussions since the initiation of the study that have not been incorporated as protocol amendments, but were discussed during the preparation of the Statistical Analysis Plan. These changes and the rationale for their implementation are documented herein and represent changes made prior to the database lock and unblinding of the study.

#### **3.1. Specifying primary and secondary estimands**

##### RATIONALE:

In the study protocol, we describe the analysis populations (section 11.1) and methods to deal with incomplete data (section 11.5); however, we do not explicitly specify estimands of interest. To meet recently proposed guidelines in the “ICH E9 (R1) addendum on estimands and sensitivity analysis in clinical trials” (August 30, 2017) and to elucidate the target of our research questions, we formally define estimands that have led us to our decisions in terms of conducting the study and selecting analytical approaches.

#### **3.2. Simplified model for the primary efficacy analysis using a multiple imputation approach**

##### RATIONALE:

The primary efficacy analysis presented in Section 11.3 of the study protocol was based on a linear model for correlated errors using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable.

To effectively address missing values in baseline covariates and the need to consider iGFR values that were not measured after end stage renal disease (ESRD) as an unfavorable outcome, direct likelihood based methods are difficult to implement. For this reason, we have decided to perform the primary efficacy analysis using a *multiple imputation* (MI) approach. To perform the MI analysis, we define both *imputation* and *substantive* models. We note that in the substantive model, iGFR at baseline is no longer included as a dependent variable.

##### PROTOCOL:

We specify the model as follows “... perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable.”

##### SAP:

We specify the model as follows “... perform the analysis using a *multiple imputation* approach with a substantive model defined by means of a linear model for correlated errors with general/unstructured covariance matrix using all post-baseline iGFR measures (including those at 80, 156, and 164 weeks, respectively) as the dependent variable.

### 3.3. Revised cut-points for variables used in subgroup analyses

#### RATIONALE:

The protocol specified cut-points for subgroup analyses based on educated guesses about the distributions of variables. After investigating baseline distributions of age and AER in pooled analyses, we changed the cut-points for these variables to achieve better balance in subgroup sample size: (1) for age from 40 to 50 years (median age 52 years), (2) for iGFR from 70 to 60 ml/min/1.73 m<sup>2</sup> and (3) for AER from 300 to 30 mg/24h (median AER 42 mg/24h).

#### PROTOCOL:

To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses (based on the primary efficacy analysis described in section 11.3, with the inclusion of an interaction term of the treatment group by the subgroup variable) will be performed by age groups ( $\leq 40$  and  $>40$  yrs.), ..., baseline iGFR ( $\leq 70$  and  $>70$  ml/ min/1.73m<sup>2</sup>) ... AER at baseline ( $\leq 300$  and  $>300$  mg/24 hr.), and ....

#### SAP:

To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses (based on the primary efficacy analysis described in section 11.3, with the inclusion of an interaction term of the treatment group by the subgroup variable) will be performed by age groups ( $\leq 50$  and  $>50$  yrs.), ..., baseline iGFR ( $\leq 60$  and  $>60$  ml/ min/1.73m<sup>2</sup>) AER at baseline ( $\leq 30$  and  $> 30$  mg/24 hr.), and ....

### 3.4. Calculations of visit windows for the analytical dataset

#### RATIONALE:

Per-protocol windows for scheduling Visits 6-16 are calculated relative to the Visit 5 date. In early versions of the Study Protocol (versions 5.0 and 6.0), randomization was performed at a study visit (Visit 5) and consequently the visit date and randomization date were equivalent. Starting with Study Protocol, version 7.0, Visit 5 became a phone call visit and randomization did not necessarily occur on the date of the phone call. For analytical purposes (see SAP Section 6.3), visit windows will be calculated relative to randomization date.

#### PROTOCOL:

Visit 1 will be considered as Time 0 for scheduling Visits 2-5, Visit 5 will be considered as Time 0 for scheduling Visit 6-16, Visit 16 as Time 0 for scheduling Visit 17.

#### SAP:

Visit 1 will be considered as Time 0 for scheduling Visits 2-5, Visit 5 will be considered as Time 0 for scheduling Visit 6-16, Visit 16 as Time 0 for scheduling Visit 17. For analytical purposes, the randomization date will be considered as Time 0 for calculating windows for Visits 6-16.

### **3.5. Additional analysis assessing an effect of post-randomization serum uric acid changes on iGFR values at Visit 17**

RATIONALE:

Following internal discussion on the importance of the relationship between serum uric acid (sUA) and iGFR measures, we added this analysis.

PROTOCOL:

Not applicable

SAP:

Details are provided in SAP Section 6.8.4.

### **3.6. Additional analysis assessing treatment effect on time to 40% eGFR decrease**

RATIONALE:

Following internal discussion on the importance of the recently proposed measure of kidney decline, namely 40% eGFR decrease, we added this analysis.

PROTOCOL:

Not applicable

SAP:

Details are provided in SAP Section 6.8.5.

### **3.7. Additional analysis assessing time to doubling of serum creatinine, end-stage renal disease (ESRD), or cardiovascular/renal death**

RATIONALE:

Following internal discussion on the importance of the recently proposed measure of kidney decline, namely using cardiovascular/renal death as part of the composite endpoint definition, we added this analysis.

PROTOCOL:

Not applicable

SAP:

Details are provided in SAP Section 6.8.6.

### **3.8. Modifying definition of per-protocol analysis set**

RATIONALE:

Following internal discussion we modified the per-protocol definition as follows.

PROTOCOL:

- **Per Protocol:** ... The per protocol population will exclude subjects ... as well as data points for which the cumulative exposure to the study medication from

randomization to that time point was less than 80% of the theoretical full exposure (see Section 11.1 in the protocol).

SAP:

- **Per Protocol:** ... The per protocol population will exclude subjects ... for whom the average drug exposure was less than 80% (see Section 11.1 in the protocol).

#### 4. Study Aim

The study aim is to determine whether lowering serum UA by means of oral allopurinol is effective in preventing or slowing decline of renal function in T1D patients with history and/or presence of microalbuminuria or moderate macroalbuminuria, or with ongoing GFR loss regardless of history or presence of albuminuria, who have only mildly or moderately impaired kidney function.

#### 5. Study Estimands

This section describes the primary and secondary estimands for corresponding endpoints and variables of interest. We follow ICH-E9 (R1) recommendations and specify estimands in terms of four attributes defining the treatment effect of interest:

- A1. The target population
- A2. The variable (or endpoint) to be obtained for each patient that is required to address scientific question of interest
- A3. Strategies for addressing intercurrent events
- A4. The population summary for the variable (endpoint), that provides a basis for a comparison between treatment conditions.

In Table 5.1. we include various intercurrent events that occurred in the PERL study and divide them into three groups, based on their implications for subsequent data collection of the endpoint of interest.

Table 5.1. Groups of intercurrent (IC) events in PERL study

Group of IC events	IC event	Implications for Post-IC data
Group A	Non-adherence to study drug schedule	Post-IC data are collected, but their interpretation may be affected depending on the estimand of interest
	Permanent discontinuation of study drug	
	Use of prohibited medication	
	Missed scheduled visit	
Group B	ESRD treatment (hemodialysis or transplant for ESRD subjects)	Post-IC data do not contain any relevant information about estimands of interest and for this reason they are not collected
Group C	Early discontinuation from the study	Post-IC data cannot be collected
	Terminal event, i.e. death	

## 5.1. Primary estimand for iGFR at Visit 17 endpoint

This is the de-facto (effectiveness) estimand of the primary endpoint – iGFR at Visit 17 – that quantifies a treatment effect due to the initially randomized treatments as actually taken, i.e., the treatment of allopurinol versus placebo without a confounding effect of treatment for ESRD subjects. The four attributes of this estimand are as follows:

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. The variable (or endpoint): The primary endpoint is the measured glomerular filtration rate (GFR) based on plasma disappearance of non-radioactive iohexol (iGFR) at the end of the 2-month wash-out period (Visit 17 at Week 164) following the 3-year intervention. The rationale of measuring the primary outcome at the end of the wash-out period is to test allopurinol for *lasting* effects on the natural history of kidney disease, independent from any transient, hemodynamic effect that the medication may have on GFR. iGFR is calculated from blood samples drawn at baseline and 120, 150, 180, 210, and 240 minutes after an i.v. bolus of iohexol, adjusting for body surface area.

A3. Strategies for different groups of IC events: Depending on IC event group membership (see Table 5.1), the variable of interest, in this case iGFR values, collected after an IC event in

- Group A will be considered as directly interpretable. Effectively, IC events in this group are ignored, which is consistent with the ITT principle.
- Group B are assumed to follow a hypothetical scenario, in which variable of interest after developing ESRD takes on biologically plausible values that are not confounded by IC event i.e. by ESRD treatment.
- Group C are assumed to conform to a hypothetical scenario in which post-IC values of the variable of interest (or endpoint) have a similar distribution to other non-ESRD subjects

A4. Population summary to compare treatments: Population-average treatment effect on iGFR at V17.

## 5.2. Secondary estimands

### 5.2.1. Estimand for iGFR at the end of the 3-year treatment period (Visit 16, before the washout) a secondary endpoint

This is de-facto (effectiveness) estimand for the iGFR at Visit 16 endpoint with the following attributes:

A1. Target population: T1D (inclusion/exclusion criteria specified in the Study Protocol)

A2. Variable of interest (endpoint): iGFR calculated at the end of the 3-year intervention (at Visit 16, last visit before washout)

A3. Strategies for different groups of IC events: The same as those used for primary estimand (see Section 5.1)

A4. Population summary to compare treatments: Population-average treatment effect on iGFR at V16

#### **5.2.2. Estimand for iGFR time trajectory estimated from repeated iGFR measurements**

This is de-facto (effectiveness) estimand for repeated iGFR measures with the following attributes:

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. Variables of interest: Repeated measures of iGFR at Visits 11, 16, 17.

A3. Strategies for different groups of IC events: The same as those used for primary estimand (see Section 5.1)

A4. Population summary to compare treatments: Population-average treatment effect on the slope of iGFR trajectory

#### **5.2.3. Estimand for estimated Glomerular Filtration Rate (eGFR) at 4 months after randomization (Visit 7)**

This is de-facto (effectiveness) estimand for eGFR at 4 months after randomization with the following attributes:

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. Variable of interest (endpoint): eGFR at 4 months after randomization as estimated from serum creatinine and cystatin C using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations (Inker et al, 2012, Fan et al, 2015). This endpoint is employed to measure a transient, hemodynamic effect that the study medication may have on GFR.

A3. Strategies for different groups of IC events: The same as those used for primary estimand (see Section 5.1)

A4. Population summary to compare treatments: Population-average treatment effect on eGFR at 4 months after randomization.

#### **5.2.4. Estimand for estimated GFR (eGFR) time trajectory**

This is de-facto (effectiveness) estimand for eGFR trajectory with the following attributes:

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. Variable of interest (endpoint): Repeated eGFR measures at all post-randomization visits (Visit 6 through 17) as estimated from repeated serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations.



A3. Strategies for different groups of IC events: The same as those used for primary estimand (see Section 5.1)

A4. Population summary to compare treatments: Population-average treatment effect on the slope of post-randomization eGFR trajectory

#### **5.2.5. Estimand for time to doubling of serum creatinine or end-stage renal disease (ESRD)**

This is de-facto (effectiveness) estimand for doubling of serum creatinine or developing ESRD with the following attributes.

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. Variable of interest (endpoint): This secondary endpoint is defined as a composite of two events: (1) doubling to serum creatinine, and (2) ESRD. Time to event is defined as time from randomization to the first event (one of the events defined above) or censoring (lost-to-follow-up, withdrawal, death, and study completion without experiencing the event).

A3. Strategies for different groups of IC events: Depending on IC event group membership (see Table 5.1) variable of interest/endpoint values, collected after IC event in

- Group A will be considered as directly interpretable. Effectively IC events in this group are ignored, which is consistent with the ITT principle.
- Groups B and C are assumed to conform a hypothetical scenario in which the variable of interest/endpoint values have a similar distribution to subjects not experiencing the IC event.

A4. Population summary to compare treatments: Hazard ratio for allopurinol versus placebo.

#### **5.2.6. Estimand for urinary AER at the end of the two-month wash-out period (Visit 17)**

This is de-facto (effectiveness) estimand for AER at V17 with the following attributes.

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. Variable of interest (endpoint): Geometric mean of two urinary AER measures obtained at Visit 17.

A3. Strategies for different groups of IC events: Depending on IC event group membership (see Table 5.1) variable of interest/endpoint values, collected after IC event in

- Group A will be considered as directly interpretable. Effectively IC events in this group are ignored, which is consistent with the ITT principle.
- Groups B and C are assumed to conform a hypothetical scenario in which the variable of interest/endpoint values have a similar distribution to subjects not experiencing the IC event.

A4. Population summary to compare treatments: Population-average treatment effect on AER at V17 expressed as a ratio of geometric means.

#### **5.2.7. Estimand for urinary AER during the last three months of the treatment period (Visits 15 and 16)**

This is de-facto (effectiveness) estimand for AER at Visit 15 and 16 with the following attributes:

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. Variable of interest (endpoint): Geometric mean of urinary AER measures at Visit 15 and Visit 16.

A3. Strategies for different groups of IC events: Depending on IC event group membership (see Table 5.1) variable of interest/endpoint values, collected after IC event in

- Group A will be considered as directly interpretable. Effectively IC events in this group are ignored, which is consistent with ITT principle.
- Groups B and C are assumed to conform a hypothetical scenario in which the variable of interest/endpoint values have a similar distribution to subjects not experiencing the IC event.

A4. Population summary to compare treatments: Population-average treatment effect on AER during last three months of the treatment expressed as a ratio of geometric means.

#### **5.2.8. Estimand for the time to fatal or non-fatal cardiovascular events endpoint**

This is de-facto (effectiveness) estimand for fatal and non-fatal cardio-vascular events with the following attributes:

A1. Target population: Individuals with T1D meeting the inclusion/exclusion criteria specified in the Study Protocol.

A2. Variable of interest (endpoint): This secondary endpoint is defined as a composite of multiple events: (1) Cardiovascular disease (CVD) death (ICD-10 code I10 to I74.9), (2) Myocardial infarction, (3) Stroke (ischemic or hemorrhagic), (4) Coronary artery bypass grafting, or (5) Percutaneous coronary intervention. Time to fatal or non-fatal cardiovascular events is defined as the time from randomization to the first event (one of the events defined above) or censoring (lost-to-follow-up, withdrawal, non-CVD death, and study completion without experiencing the event).

A3. Strategies for different groups of IC events: Depending on IC event group membership (see Table 5.1) variable of interest/endpoint values, collected after IC event in

- Group A will be considered as directly interpretable. Effectively IC events in this group are ignored, which is consistent with ITT principle.
- Groups B and C (except CVD death) are assumed to conform a hypothetical scenario in which variable of interest/endpoint values have similar distribution to subjects not experiencing IC event.

A4. Population summary to compare treatments: Hazard ratio for allopurinol versus placebo

## 6. Analytical Strategy

In the initial analysis of the primary outcome we will present iGFR univariate statistics by Treatment Groups at each study visit (V4, V11, V16 and V17).

No formal interim analyses of the primary endpoint will be conducted, therefore the nominal  $\alpha$  level to be used at the final analysis will be 0.05 for the primary endpoint. All other secondary outcomes will also be tested at the 0.05 level, with no adjustment for multiplicity. Many of the models used in the analyses include baseline covariates, such as stratifying variables (serum uric acid (sUA), HbA1c, clinical site), iGFR, albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who qualified by eGFR slope and were normoalbuminuric at baseline), AER, and time, and time by treatment interaction. If there are problems with fitting these models, due, for example, to lack of convergence to optimal values, covariates will be eliminated from the models in the following order: baseline AER, albuminuria status, serum uric acid (sUA), HbA1c, and clinical site. More detailed information about these covariates is included in Section 6.4.

### 6.1. Study populations

Two study populations will be defined for the purpose of data analysis:

- **Intention to Treat (ITT):** The ITT analysis set consists of all subjects enrolled in PERL, randomized to study medication.
- **Per Protocol:** The per protocol analysis set will consist of a subset of ITT subjects. The per protocol population will exclude subjects with major protocol deviations (defined as receiving the wrong study medication) as well as subjects for whom the average drug exposure is less than 80% (see Section 11.1 in the protocol).

To account for missing values in any specific analysis, all subjects meeting the study population definitions will be included and analyzed using (1) multiple imputation techniques (see Section 6.4), or (2) appropriate analytical approaches that allow for missing values under plausible missing data mechanisms, such as linear mixed-effects models that allow values of the dependent variable to be missing under missing at random (MAR) mechanism.

Long study follow-up results in missing values for the outcomes and precludes strict adhering to ITT principle. To mitigate this issue we will follow four strategies proposed by I.R. White et al (2011):

1. Attempt to follow up all randomized participants, even if they withdraw from allocated treatment.
2. Perform a main analysis of all observed data that are valid under a plausible assumption about missing data.
3. Perform sensitivity analyses to explore the effect of departures from the assumptions made in the main analysis.
4. Account for all ITT study population participants, at least in the sensitivity analyses.

## 6.2. Blinded data review

Prior to unmasking the study and starting any formal analysis, data will be reviewed in a blinded fashion by computing summary statistics for primary and secondary outcomes, and baseline covariates. This will allow the identification of unusual values and/or patterns of missing values for key variables that need to be queried. In addition, such blinded data review will allow the writing committee to assess the format of data presentation. Note that the blinded data review incorporates real data but *random* treatment assignment (i.e., investigators do not receive data summarized by actual treatment group, rather they review data on two randomly formed groups). All decisions will be made and documented in this SAP document prior to database lock and unblinding.

## 6.3. Visit windows

To provide scheduling flexibility to study sites and participants, visits were required to occur within a protocol-defined window rather than on a specific date. The protocol-defined visit windows are summarized in the tables 6.3.1 and 6.3.2 below. For analytic purposes, the visit windows defined in the protocol will be expanded in order to eliminate gaps between them. This will ensure that all observations, including those that may have occurred outside a protocol-specified time window, will be associated with the most appropriate visit and therefore properly included in the analysis. If multiple observations occur within a window, the one closest to the visit target date will be utilized. If two observations are equi-distant from the target date, the first one will be utilized.

As iGFR is the primary and key secondary endpoint, the protocol allowed for repeats of the iGFR procedure in order to achieve qualified iGFR values. Also, the procedure required a longer visit, so it was more difficult to schedule. Thus, we allowed wider windows for iGFR visits (V11, V16 and V17) to ensure that all qualified iGFRs are analyzed. In addition to avoid over-writing iGFR visits with a non-iGFR (V6-V10, V12-V15) visit, and vice-versa the aforementioned procedure will be performed separately for non-iGFR (Table 6.3.1) and iGFR visits (Table 6.3.2).

**Table 6.3.1. PERL windows for post-randomization non-IGFR visits.**

Visit	Lower Boundary of Window (Week, Excluding First day)	Per protocol Target Date window in weeks	Upper Boundary of Window (Week, Including last day)	Time since randomization attributed to visit window (in weeks)
Visit Windows relative to Randomization Date				
Visit 6	0	<u>4 [3-5]</u>	10	5
Visit 7	10	<u>16 [14-20]</u>	24	17
Visit 8	24	<u>32 [30-34]</u>	40	32
Visit 9	40	<u>48 [46-50]</u>	56	48
Visit 10	56	<u>64 [62-66]</u>	72	64
Visit 12	88	<u>96 [94-98]</u>	104	96
Visit 13	104	<u>112 [110-114]</u>	120	112
Visit 14	120	<u>128 [126-130]</u>	135	128
Visit 15	135	<u>142 [140-146]</u>	150	142

All intervals (target dates and lower/upper window boundaries) for visits 6 through 16 are calculated relative to the randomization date. The interval for Visit 17 is calculated relative to Visit 16. Most post-randomization visits are 16 weeks apart, with the exception of Visits 6 and 7 and Visits 16 to visit 17. For the purpose of selected analyses (sections 6.7.2 and 6.7.4) involving multiple imputations, we included in the last column of Tables 6.3.1 and 6.3.2 a time since randomization associated with a corresponding visit window. Entries in this column are based approximately on the mid-points between target dates.

**Table 6.3.2. PERL windows for post-randomization iGFR visits.**

Visit	Lower Boundary of Window (Week, Excluding First day)	Per protocol Target Date window in weeks	Upper Boundary of Window (Week, Including last day)	Time since randomization associated with visit window (in weeks)
Visit Windows relative to Randomization Date				
Visit 11 (iGFR)	53	80 [78-84]	97	80
Visit 16 (iGFR)	149	156 [154-160]	178	164
Visit Window relative to V16				
Visit 17 (iGFR)	0	8 [6-12]	20	174

## 6.4. Baseline covariates

The following is a description of the baseline covariates that will be used in the various analyses outlined in the remainder of Section 6.

- Stratifying variables
  - serum uric acid (sUA) at baseline with 2 levels ( $\leq 6.0$  and  $> 6.0$  mg/dl)
  - glycated hemoglobin (HbA1c) at baseline with two levels ( $\leq 7.8$  and  $> 7.8\%$ )
  - clinical site/study center with 16 levels (based on main sites with satellite sites collapsed into main sites)
- Baseline iGFR measured at Visit 4
- Baseline eGFR measured at Visit 4
- Treatment group with two levels (Allopurinol, Placebo)
- Albuminuria status with 2 levels (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who qualified by eGFR slope and were normoalbuminuric at baseline)
- Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale

As of Dec. 17, 2018, we have complete data available for baseline serum uric acid, treatment group, and study center. The number of missing values for the other baseline covariates is 1 for iGFR, 2 for HbA1c, and 17 for albuminuria status. Missing baseline data will be imputed according to the approach described in Section 6.5. When creating covariates for analytical and imputation purposes, we will aggregate clinical sites with a small number of randomized subjects, such as Edmonton (site #11, n=3) and Vancouver (site #16, n=11) will be combined with Calgary (site #10, n=20) in the same geographic region. Similarly, Spokane (site #15, n=5) will be combined with Seattle (site #13, n=35).

## 6.5. Missing values

Missing values both for baseline characteristics and for outcomes/endpoints of interest are inevitable, especially in studies with longer follow-up. To effectively address missing values that occurred for baseline covariates and for post-randomization variables of interest, and the necessity to consider post-ESRD iGFR values as an unfavorable outcome, models involving direct likelihood methods are difficult to implement. For this reason, we will perform the analyses using a *multiple imputation* (MI) approach consisting of three steps:

Step 1. Using an *imputation* model, create multiple datasets with missing values imputed

Step 2. Fit *substantive models* described in Sections 6.6 and 6.7 using imputed datasets created in Step 1

Step 3. For each substantive model, combine the results obtained in Step 2 for the inference using Rubin's rule (Rubin, 1987).

To create imputed datasets in Step 1, we will employ multivariate imputation by means of fully conditional specification (FCS) method introduced by van Buuren et al, 2006. This method is especially attractive in our case because it handles non-monotone patterns of missingness, and arbitrary types of imputed variables, i.e. both continuous and categorical. The imputation model will include baseline covariates listed in Section 6.4. In addition, to make imputation model more general than substantive models, we will include HbA1c at Visit 1 and geometric mean of AER at Visit 3 and 4 expressed on logarithmic base to 10 scale predictive of other baseline covariates.

We will also include eGFR at all post-randomization visits, i.e., Visit 6-17, iGFR at Visits 11, 16 and 17, AER at Visits 15, 16 and 17 expressed on logarithmic base to 10 scale. Imputation of baseline variables will be performed starting with variables having the lowest number of missing values. Variables measured longitudinally, i.e., eGFR and iGFR, will also be modeled sequentially in order determined by visit number. To preserve different response patterns in the study treatment groups (i.e., treatment group by study visit interaction) imputations will be performed separately in each group. Resulting data will consist of 25 imputed datasets. We note that the FCS method imputes data under the missing at random (MAR) assumption, e.g., the probability that the iGFR/eGFR value is missing depends on *observed* rather than *unobserved* values of the variable. Although we consider the MAR assumption to be sensible for our study, it does not apply for post-ESRD iGFR/eGFR values. To model *post-ESRD eGFR* measures as a deviation from the MAR assumption, we will impute these values using a controlled imputation technique, specifically the delta-adjustment approach (O’Kelly, Ratitch, 2014). This technique will impute post-ESRD eGFR values on average at 7 ml/min/1.73m<sup>2</sup>, with a small variation around it, that is consistent with: (1) attributing to missing post-ESRD eGFR values the value representing ‘the worst case scenario’, (2) assigning a biologically acceptable value, and (3) including the ‘absorbing state’ feature of ESRD. We note that eGFR measures are taken at every visit and are used to determine time of developing ESRD. For this reason pre-ESRD eGFR values are highly predictive of post-ESRD iGFR values. In addition, we note that post-ESRD iGFR and eGFR values lie in a very narrow range and they are effectively interchangeable. For these reasons, we will impute post-ESRD iGFR values by using corresponding post-ESRD eGFR imputed values as a proxy. We note that the imputation of *eGFR* and *iGFR* values for subjects who *did not develop* ESRD and have low values to start with may lead to imputed values lower than 15 ml/min/1.73m<sup>2</sup>, which is biologically implausible. For this reason, imputed values of *eGFR* and *iGFR* for subjects who *did not develop* ESRD will be truncated at 15 ml/min/1.73m<sup>2</sup>. Similarly, log(AER) base 10 imputed values for these subjects will be truncated at a lower limit of detection of -0.60206 (= log<sub>10</sub>(0.25)).

## 6.6. Analysis for the primary estimand

In this Section we describe primary and secondary analyses aligned with the primary estimand defined in Section 5.1. These models will be employed as substantive models (see Step 2 of multiple imputation approach described in Section 6.5).

### 6.6.1. Primary analysis for the primary estimand

The goal of the primary analysis for the primary estimand is to test the null hypothesis of the difference in means between treatment arms in the primary endpoint (iGFR at the end of the 2-month wash-out period [Visit 17] following the 3-year intervention) being equal to zero. The analysis will be performed in a multiple imputation framework on the intention-to-treat (ITT) population and will employ a linear model for correlated errors with general/unstructured covariance matrix (Molenberghs and Verbeke, 2005; Galecki and Burzykowski, 2013) as a substantive model. For each time  $t$  ( $t = 1, 2, 3$ ) corresponding to post-randomization iGFR visits, i.e. visits V11 (80 weeks), V16 (156 weeks), and V17 (164 weeks after randomization) the model equation is specified as:

$$iGFR_{it} = \beta_{0t} + \beta_{1t} TRT_i + \mathbf{x}'_i \boldsymbol{\beta} + \epsilon_{it}, \quad (6.1)$$

where  $iGFR_{it}$  is the value of iGFR at time  $t$  for subject  $i$  ( $i = 1, \dots, 530$ ). Fixed effects  $\beta_{0t}$ ,  $\beta_{1t}$  for  $t = 1, 2, 3$  denote visit-specific intercepts and treatment effects.  $TRT_i$  is

treatment group (equal to 1 for the allopurinol and 0 for placebo). Stratifying variables (serum uric acid, HbA1c, study center), and baseline covariates: albuminuria status, AER, iGFR for subject  $i$  are included in a vector  $\mathbf{x}_i$  of  $p$  covariates  $(x_1, \dots, x_p)$  and associated fixed effects are stored in vector  $\boldsymbol{\beta} = (\beta_1, \dots, \beta_p)$ . We assume that residual errors  $\epsilon_{it}$  ( $t = 1, 2, 3$ ) for subject  $i$  are normally distributed with zero mean and  $3 \times 3$  general/unstructured variance-covariance matrix. The model specified in (6.1) will yield the estimates of visit-specific treatment effects  $\beta_{11}, \beta_{12}, \beta_{13}$  for all three visits V11, V16 and V17. In the context of the primary analysis of the primary endpoint, we are interested in parameter  $\beta_{13}$ , representing treatment effect at Visit 17 adjusted for stratifying variables and baseline covariates. The Kenward-Roger approximation will be used to estimate denominator degrees of freedom.

<b>Estimand</b>	Primary estimand defined in Section 5.2.1
<b>Analysis</b>	Primary Analysis of the Primary Endpoint: iGFR at the end of the 2-month wash-out period (Visit 17)
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear model for repeated measures with correlated errors using multiple imputation technique.
<b>Dependent Variable</b>	iGFR measured at Visits V11 (80 weeks) , V16 (156 weeks) and V17 (164 weeks after randomization)
<b>Model</b>	Fixed effects: <ul style="list-style-type: none"> <li>• Visit-specific intercepts corresponding to V11, V16, V17</li> <li>• Visit-specific treatment effects corresponding to V11, V16, V17</li> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Least square iGFR means at Visit 17 by treatment group.</li> <li>• Estimate of treatment effect at Visit 17 adjusted for baseline covariates</li> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>

We will assess the impact of deviation from the MAR assumption on the robustness of the results through a *sensitivity analysis*. For the primary estimand, it will be performed within the same multiple imputation framework; however we will employ marginal delta-adjusted method and apply it to Visit 17 with adjustments in allopurinol arm increasing by one unit of iGFR value until the MAR results are overturned, that is, we will use so called tipping point approach (O’Kelly, Ratitch, 2014).

### 6.6.2. Secondary analysis for the primary estimand

The following secondary analyses will be performed to assess how alternative assumptions of the primary endpoint (as defined above) and alternative approaches for handling missing data may affect the conclusions of the analysis:

1. Analysis of covariance using iGFR values at Visit 17 as the dependent variable and treatment effect as a covariate of primary interest. The same baseline covariates, as in the primary analysis of the primary endpoint, stored in vector  $\mathbf{x}_i$  (see Equation. 6.1) will be used in the model.



2. Performing an analysis identical to the primary one (same endpoint and substantive model) using the per-protocol analysis set rather than the ITT analysis set.

## 6.7. Analyses for secondary estimands

In this section we present analyses aligned with secondary estimands defined in section 5.2. Analyses will be performed using the multiple imputation technique, except those involving time-to event endpoints (sections 6.7.5, 6.7.8).

### 6.7.1. iGFR at the end of the 3-year treatment period (Visit 16, before the washout)

In this section we describe the analysis (estimator) aligned with the estimand for iGFR at the end of the 3-year treatment period (Visit 16, before the washout) endpoint defined in Section 5.2.1.

The least square means at Visit 16, estimate of treatment effect *at Visit 16* adjusted for baseline covariates, their 95% confidence interval and P-value will be obtained as part of the primary analysis of the primary estimand (Equation (6.1) in section 6.1.1). In the context of this secondary endpoint, we are interested in the fixed effect  $\beta_{12}$ , which represents the treatment effect at Visit 16 adjusted for stratifying variables and baseline covariates. To assess the hemodynamic/transient effect of the allopurinol, we will estimate the contrast  $\beta_{12} - \beta_{13}$  between the treatment effect at Visit 16 (before washout) compared to that at Visit 17 (after washout).

### 6.7.2. iGFR time trajectory estimated from repeated iGFR measurements

This analysis is aligned with the estimand defined in Section 5.2.2

<b>Estimand</b>	See section. 5.2.2 for definition
<b>Analysis</b>	Analysis of the Secondary Endpoint: iGFR time trajectory estimated from repeated iGFR measurements.
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear mixed-effects model for longitudinal iGFR measures using multiple imputation technique.
<b>Dependent Variable</b>	iGFR measured at Visits V11 (80 weeks) , V16 (156 weeks) and V17 (164 weeks after randomization)
<b>Model</b>	<p>Fixed effects associated with:</p> <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Time since randomization (in years) associated visit windows defined in section 6.3</li> <li>• Time by treatment group interaction</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul> <p>Subject-specific random effects</p> <ul style="list-style-type: none"> <li>• Random intercept for iGFR</li> <li>• Random slope for iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• iGFR slope estimates and 95% CIs by treatment group</li> <li>• Estimate of a treatment effect measured as a difference between average slopes of iGFR versus time for allopurinol and placebo groups adjusted</li> </ul>

	for stratifying variables and baseline covariates <ul style="list-style-type: none"> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>
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### 6.7.3. eGFR at 4 months after randomization (Visit 7)

<b>Estimand</b>	See section 5.2.3 for definition
<b>Analysis</b>	Analysis of the Secondary Endpoint: eGFR at 4 months after randomization (Visit 7).
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear model using multiple imputation technique.
<b>Dependent Variable</b>	eGFR measured at Visit V7 (16 weeks after randomization)
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline eGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Least square eGFR means at Visit 7 by treatment group.</li> <li>• Estimate of treatment effect at Visit 7 adjusted for stratifying variables and baseline covariates</li> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>

### 6.7.4. eGFR time trajectory

<b>Estimand</b>	See section 5.2.4 for definition
<b>Analysis</b>	Analysis of the Secondary Endpoint: eGFR time trajectory estimated from repeated eGFR measurements using multiple imputation technique..
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear mixed-effects model for longitudinal eGFR measures using multiple imputation technique.
<b>Dependent Variable</b>	Post-randomization eGFR measured from Visits V6 through V17
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Time since randomization in (in years) associated visit windows defined in section 6.3</li> <li>• Time by treatment group interaction</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline eGFR</li> </ul> Subject-specific random effects <ul style="list-style-type: none"> <li>• Random intercept for eGFR</li> <li>• Random slope for eGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• eGFR slope estimates and 95% CIs by treatment group</li> <li>• Estimate of a treatment effect measured as a difference between average eGFR versus time slopes for allopurinol and placebo groups adjusted for stratifying variables and baseline covariates</li> <li>• 95% confidence interval for treatment effect</li> <li>• P-value for treatment effect</li> </ul>

### 6.7.5. Time to serum creatinine doubling or ESRD

<b>Estimand</b>	See section 5.2.5 for definition
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Proportional hazards model for interval censored data.
<b>Dependent Variable</b>	Time to composite endpoint of serum creatinine doubling or ESRD
<b>Proportional Hazards Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• N(%) of subjects with doubled serum creatinine or ESRD during the course of the study</li> <li>• Hazard ratio of allopurinol to placebo</li> <li>• 95% confidence interval for hazard ratio</li> <li>• P-value for treatment effect</li> </ul>

### 6.7.6. Urinary AER at the end of the wash-out period

<b>Estimand</b>	See section 5.2.6 for definition
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear model using multiple imputation technique.
<b>Dependent Variable</b>	Two AER measures obtained at Visit 17 and summarized using the geometric mean expressed on logarithm base to 10 scale
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Predicted urinary AERs at <i>Visit 17</i> by treatment group obtained by antilog transformation applied to corresponding least square means</li> <li>• Estimate of treatment effect at <i>Visit 17</i> expressed on percent change scale using antilog transformation.</li> <li>• 95% confidence interval for treatment effect expressed on percent change scale using antilog transformation.</li> <li>• P-value for treatment effect</li> </ul>

### 6.7.7. Urinary AER during the last three months of the treatment period (Visits 15 and 16)

<b>Estimand</b>	See Section 5.2.7 for definition
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear model using multiple imputation technique.
<b>Dependent Variable</b>	Two AER measures obtained at Visit 15 and 16 are summarized using the geometric mean expressed on logarithmic scale
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Predicted AERs at the end of treatment period by treatment group obtained by antilog transformation applied to corresponding least square means.</li> <li>• Estimate of treatment effect at the end of treatment period adjusted for baseline covariates expressed on percent change scale using antilog transformation.</li> <li>• 95% confidence interval for treatment effect expressed on percent change using antilog transformation.</li> <li>• P-value for treatment effect</li> </ul>

### 6.7.8. Time to fatal or non-fatal cardiovascular events

<b>Estimand</b>	See Section 5.2.8 for definition
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Cox proportional hazards model.
<b>Dependent Variable</b>	Time to composite endpoint: fatal or non-fatal cardiovascular events
<b>Cox Model</b>	Fixed effects: <ul style="list-style-type: none"> <li>• Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul> </li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• N(%) of subjects with fatal or non-fatal cardiovascular events during the course of the study</li> <li>• Hazard ratio of allopurinol to placebo</li> <li>• 95% confidence interval for hazard ratio</li> <li>• P-value for treatment effect</li> </ul>

## 6.8. Other analyses

### 6.8.1. Subgroup analyses

To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses (based on the primary efficacy analysis described in subsection 6.6.1, with the inclusion of appropriate interaction terms with the subgroup variable) will be performed by age groups ( $\leq 50$  and  $> 50$  yrs), gender, racial/ethnic group, HbA1c ( $\leq 7.8$  and  $> 7.8\%$ ), serum uric acid ( $\leq 6.0$  and  $> 6.0$  mg/dl), baseline iGFR ( $\leq 60$  ml/min and  $> 60$  ml/min/ $1.73\text{m}^2$ ), AER at baseline ( $\leq 50$  and  $> 50$  mg/24 hr), and albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did qualify by eGFR slope and were normoalbuminuric at baseline).

An example of such subgroup analysis for age groups ( $\leq 50$  and  $> 50$  yrs) is provided below. Similar to Equation (6.1) for each time  $t$  ( $t = 1, 2, 3$ ), corresponding to visits V11, V16, V17, we specify the model:

$$iGFR_{it} = \beta_{0t} + \beta_{1t} TRT_i + \beta_{2t} AGE_i + \beta_{3t} AGE_i \times TRT_i + \mathbf{x}'_i \boldsymbol{\beta} + \epsilon_{it}, \quad (6.2)$$

where  $iGFR_{it}$  is the value of iGFR at time  $t$  for subject  $i$  ( $i = 1, \dots, 530$ ). Fixed effects  $\beta_{0t}, \beta_{1t}, \beta_{2t}, \beta_{3t}$  for  $t = 1, 2, 3$  denote visit-specific intercepts, treatment effects, age effects and age by treatment interactions, respectively.  $TRT_i$  is treatment group (equal to 1 for the allopurinol and 0 for placebo).  $AGE_i$  indicates age group ( $\leq 50$  and  $> 50$  yrs). Stratifying variables, and baseline covariates albuminuria status, AER, iGFR for subject  $i$  are included in a vector of covariates  $\mathbf{x}_i$  and associated fixed effects are stored in vector  $\boldsymbol{\beta}$ . We assume that residual errors  $\epsilon_{it}$  ( $t = 1, 2, 3$ ) for subject  $i$  are normally distributed with zero mean and  $3 \times 3$  general/unstructured variance-covariance matrix. The model specified in (6.2) will yield the estimates of visit-specific treatment by age interaction effects  $\beta_{31}, \beta_{32}, \beta_{33}$  for all three visits V11, V16 and V17. In the context of subgroup analysis, we are interested in  $\beta_{33}$ , which represents treatment by age interaction at Visit 17 adjusted for baseline covariates.

<b>Analysis</b>	Subgroup Analysis of the Primary Endpoint: iGFR at the end of the 2-month wash-out period (Visit 17) by Age group
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear model for repeated measures with correlated errors
<b>Dependent Variable</b>	iGFR measured at Visits V11 (80 weeks), V16 (156 weeks) and V17 (164 weeks after randomization)
<b>Model</b>	Fixed effects: <ul style="list-style-type: none"> <li>• Visit-specific intercepts, age effects, treatment effects and age by treatment interaction effects</li> <li>• Stratifying variables: sUA, HbA1c, Study center</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log base to 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Estimate of age by treatment interaction <i>at Visit 17</i> adjusted for baseline covariates</li> <li>• 95% confidence interval for age by treatment effect interaction <i>at Visit 17</i></li> <li>• P-value for treatment effect</li> </ul>

### 6.8.2. Analyses of safety outcomes

Safety measures are assessed during three periods of the study: run-in (Visits 1-5), on-treatment (after Visit 5 through Visit 16), and off-treatment washout (after Visit 16 through Visit 17). Safety will be summarized overall (treatment and off-treatment combined) and by period, depending on the safety outcome of interest during that period.

- Percentage of subjects with and number of SAEs, time to first SAEs during on-treatment period and overall by MedDRA System Organ Class and by MedDRA Preferred Term Categories.
- Percentage of subjects with and number of permanent discontinuations of study medication because of adverse effects on-treatment period and overall.
- Percentage of subjects with and number of AEs, overall and by severity and by relatedness to study medication during on-treatment period and overall.
- Percentage and number of subjects with skin rash during on-treatment period and overall.

For dichotomous safety outcomes, the proportion of subjects experiencing adverse outcomes (AEs, SAEs) will be summarized by treatment group and compared by means of odds ratios and 95% CIs. Poisson regression models will be used for safety outcomes (e.g., SAEs and AEs) with multiple recurrences per patient, with the logarithm of the period of observation from the time of study medication used as the offset. Time to first SAE will be analyzed using Kaplan-Meier methods to estimate the SAE-free distributions for each treatment group. This analysis will employ the ITT analysis set. No imputation for missing data will be used.

### 6.8.3 Analyses of other measures

In addition to primary, secondary, and safety measures, the following additional outcomes will be analyzed to help with the interpretation of study results:

- Descriptive statistics for body weight, blood pressure, serum creatinine, HbA1c, and serum uric acid at each post-baseline visit and their changes from baseline, by treatment group in the ITT population. No imputation for missing data will be employed.
- Percentage of subjects receiving adequate study medication exposure (i.e., allopurinol or placebo) independent of adverse events. This is defined as the actual total dose during the 156-week dosing period, as determined from the dispensed dosage and pill counts, divided by the expected total dose defined by the eGFR-adjusted protocol-described dosing regimen, without consideration for temporary or permanent discontinuations or reductions owing to adverse events. The proportion of subjects receiving the presumed adequate study medication exposure is defined as the number of subjects who had at least 80% and no more than 120% of the intended study medications during the entire dosing period, independent of adverse events, among all randomized subjects. No imputation for missing data will be employed.

**6.8.4. Analysis of the effect of post-randomization sUA changes on iGFR value at Visit 17**

The analysis outlined below will be performed using linear model with correlated errors. In addition to fixed effects associated with baseline covariates, we will include fixed effects associated with visit-specific effects of another covariate, namely the average sUA change from baseline over the initial post-randomization period (Visits 6-10) on iGFR values. We note that this covariate is created based on sUA values that precede iGFR measures (our dependent variable) and in this way we attempt to mitigate the impact of the bidirectional relationship between concurrent measures of sUA and iGFR.

<b>Analysis</b>	Analysis of the effect of post-randomization average changes of sUA values (V6-V10) relative to sUA at baseline on iGFR value at Visit 17
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Linear model with correlated errors for longitudinal iGFR measures
<b>Dependent Variable</b>	iGFR measured at Visits V11 (80 weeks), V16 (156 weeks) and V17 (164 weeks after randomization)
<b>Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Visit-specific intercepts corresponding to V11, V16, V17</li> <li>• Visit-specific effects of sUA change on iGFR at V11, V16, V17</li> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Albuminuria status at baseline with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• Estimate of an effect of an average sUA changes from baseline on iGFR value at Visit 17, adjusted for stratifying variables and baseline covariates</li> <li>• 95% confidence interval for sUA changes effect</li> <li>• P-value for sUA changes effect</li> </ul>

### 6.8.5. Time to 40% eGFR decrease

<b>Analysis</b>	Analysis of time to 40% eGFR decrease from randomization
<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Proportional hazards model for interval censored data.
<b>Dependent Variable</b>	Time to endpoint of 40% eGFR decrease from randomization
<b>Proportional Hazards Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• N(%) of subjects with 40% eGFR decrease during the course of the study</li> <li>• Hazard ratio of allopurinol to placebo</li> <li>• 95% confidence interval for hazard ratio</li> <li>• P-value for treatment effect</li> </ul>

### 6.8.6. Time to doubling of serum creatinine, end-stage renal disease (ESRD), or cardiovascular/renal death

Variable of interest (endpoint) is defined as a composite of three events: (1) doubling to serum creatinine, (2) ESRD, or (3) cardiovascular/renal death. Time to event is defined as time from randomization to the first event (one of the events defined above) or censoring (lost-to-follow-up, withdrawal, death other than due to cardiovascular/renal cause, and study completion without experiencing the event).

<b>Analysis Set</b>	ITT Population
<b>Methods</b>	Proportional hazards model for interval censored data.
<b>Dependent Variable</b>	Time from randomization to composite endpoint of serum creatinine doubling, ESRD or cardiovascular/renal death
<b>Proportional Hazards Model</b>	Fixed effects associated with: <ul style="list-style-type: none"> <li>• Stratifying variables: sUA, HbA1c, study center</li> <li>• Treatment group</li> <li>• Albuminuria status with 2 levels</li> <li>• Baseline AER geometric mean (at Visits 3 and 4 combined) expressed on log to base 10 scale</li> <li>• Baseline iGFR</li> </ul>
<b>Results</b>	<ul style="list-style-type: none"> <li>• N(%) of subjects with doubled serum creatinine, ESRD or cardiovascular/renal death during the course of the study</li> <li>• Hazard ratio of allopurinol to placebo</li> <li>• 95% confidence interval for hazard ratio</li> <li>• P-value for treatment effect</li> </ul>



## **6.9. Model assumptions and alternative analyses**

Model assumptions will be thoroughly checked for individual and systematic departures, using informal, e.g. inspection of residuals, and formal methods such as methods based on likelihood displacement. If individual outliers are detected, their influence will be evaluated using influence diagnostics methods based on comparing estimates from models fitted to data with and without outlying values. Whenever we are not successful in fitting the parametric model (linear or non-linear), then non-parametric analyses and/or transformation of the variables involved in the analysis will be considered.

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## **APPENDIX I. Study Objective, Study Design, Outcomes & Statistical Analysis and Data Management Sections from Protocol**

In this appendix, selected sections (from protocol, version 10, approved by DSMB on March 6th, 2018) are included for reference. The following sections/figures from the study protocol are included:

- 2. Study objective
- 3. Study design
- 7.1. Primary outcomes
- 7.2. Secondary outcomes
- Schedule of events (original figure on p. 27 in the study protocol)
- 9. Safety assessments
- 10. Adverse event reporting
- 11. Statistical analysis

## 2. STUDY OBJECTIVE

To determine whether lowering serum UA by means of oral allopurinol is effective in preventing or slowing decline of renal function in T1D patients with microalbuminuria or moderate macroalbuminuria who still have only mildly or moderately impaired kidney function.

## 3. STUDY DESIGN

The study will be a multi-center, double-blind, placebo-controlled, parallel-group randomized clinical trial including a total of 480 patients with type 1 diabetes (T1D) who are at high risk for GFR loss because of increased albuminuria and a relatively high serum UA ( $\geq 4.5$  mg/dl), but have only mildly or moderately decreased renal function.

## 7. STUDY OUTCOMES

### 7.1. Primary outcome

The primary outcome will be the iGFR at the end of the 2-month wash-out period following the 3-year treatment period, measured by the plasma clearance of non-radioactive iohexol (iGFR) and adjusted for the iGFR at baseline. The rationale of measuring the primary outcome at the end of the wash-out period is to test allopurinol for permanent effects of on the natural history of kidney disease, independent from any transient, hemodynamic effect that the medication may have on GFR. Plasma iohexol clearance has been shown to provide accurate and reproducible GFR measurements.<sup>30,31</sup> It is highly correlated with inulin clearance (the gold standard to measuring GFR)<sup>32</sup> and is a safe, cost-effective method to test hundreds of patients enrolled in multicenter clinical trials.<sup>33</sup> The method consists of injecting a 5 mL bolus of Iohexol (Omnipaque, 300 mg iodine/mL) and drawing blood samples at baseline and 120, 150, 180, 210, and 240 minutes after the injection. Plasma concentrations of iohexol at different time points are measured by HPLC and used to calculate the plasma clearance of iohexol ( $Cl = \text{Dose}/\text{AUC}$ , where AUC is the area under the plasma concentration time curve), which is taken after appropriate body surface area corrections as a measure of GFR.<sup>30,31</sup>

### 7.2. Secondary outcomes

1. Iohexol-clearance GFR at the end of the 3-year treatment period (before the washout).
2. Iohexol-clearance GFR time trajectory estimated from periodical iohexol-GFR measurements.
3. Estimated (eGFR) at 4 months estimated from serum creatinine and cystatin C and adjusted for the eGFR at baseline.
4. Estimated GFR (eGFR) time trajectory estimated from quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations.<sup>34,35</sup>
5. Time to doubling of baseline serum creatinine value or ESRD (eGFR  $\leq 15$  ml/min/1.73 m<sup>2</sup>, institution of dialysis, kidney transplantation).
6. Geometric mean of two AER measurements at the end of the 2-month wash-out period following the 3-year treatment period, adjusted for the mean urinary AER at baseline. Urinary AER will be determined in timed overnight urine collections brought by study participants to regular clinic visits, and expressed in  $\mu\text{g}/\text{minute}$  and as urinary albumin/creatinine ratios.
7. Geometric mean of urinary AER during the last three months of the treatment period (Visits 15 and 16), adjusted for the mean urinary AER at baseline.
8. Time to fatal or non-fatal cardiovascular events, defined as the composite of CVD death (ICD-10 code I10 to I74.9), myocardial infarction, stroke (ischemic or hemorrhagic), coronary artery bypass grafting, or percutaneous coronary intervention.



**Figure 1. Schedule of events**

Year	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	3	3	3
Week	-12	-9	-7	-3		0	4	16	32	48	64	80	96	112	128	142	156	164
Visit #	1	2	3	4	4a*	5	6	7	8	9	10	11	12	13	14	15	16	17
*Type of Visit: In-Person Visit Required (V); Phone Call (C); Other Visit (In-Person or Remote Visit, O)	O	V	O	V	V	C	O	O	O	O	O	V	O	O	O	O	V	V
EVENT	Screen	Run-in				Allopurinol or placebo												Wash-out
						RANDU 100 mg	200-400 mg											
Informed Consent	x	x																
Demographics	x																	
Initial Medical Hx		x																
Interval Medical Hx and BP Control			x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Concomitant Meds	x	x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Blood Pressure and Measurements	x	x	(x)	x	(x)		(x)	(x)	(x)	(x)	(x)	x	(x)	(x)	(x)	(x)	(x)	x
EKG Report		x		x	(x)													
Physical Exam		x		(x)	(x)													
Skin Assessment				x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Eligibility	x			x	(x)	x												
Randomization						x												
Family History				x	(x)													
RAS and BP Med Log		x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
IGFR Procedure				x	(x)													
PERL Study Drug Prescription						x	x	x	x	x	x	x	x	x	x	x	x	x
Study Drug Compliance							x	x	x	x	x	x	x	x	x	x	x	x
<b>CENTRAL LAB</b>																		
Serum uric acid, serum creat, cystatin C	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Urine ACR/AER	x		x	x	(x)			x	x	x	x	x	x	x	x	x	x	x
HbA1c	x			x	(x)			x	x	x	x	x	x	x	x	x	x	x
HLA B*58:01				x	(x)													
IGFR				x	(x)								x					x
NIDDK Repository: serum, plasma, urine				x									x					x
<b>LOCAL LAB</b>																		
Pregnancy test serum HCG	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Pregnancy test urine dipstick		x			(x)													
ALT, K, CBC, serum creatinine, urine	x		x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Protocol Deviation		x	x	x	(x)		x	x	x	x	x	x	x	x	x	x	x	x
Adverse Events		x	x	x	(x)	x	x	x	x	x	x	x	x	x	x	x	x	x

\*If normal blood pressure control is not achieved at Visit 4, the run-in period may be extended for two more weeks after which participants will be examined as in Visit 4 (Visit 4A). In this event, the GFR measurement scheduled for Visit 4 will be conducted at Visit 4A.

^ Study visits will be generally conducted at the Study Sites or their Satellites. "In-Person Visits" (V) are required for Visit 2 and all visits requiring iohexol-GFR measurements. If a participant lives far from a study site or satellite, or travel impediments are present, other (O) visits may be conducted remotely or in-person. For any given study visit to be conducted remotely, a Phone Visit and a Remote Biospecimen Collection will be both required; a Phone Visit is performed by the study coordinator using the telephone or other media such as Skype to collect results of study procedures that do not require physical interactions (e.g., collection of medical history), and a Remote Biospecimen Collection is performed at a clinical laboratory close to where participants live.

Note: (x) indicates an optional assessment; For "BP and Measurements", (x) indicates an optional assessment only if the patient is NOT seen in-person.

## **9. SAFETY ASSESSMENTS**

### **9.1. Demographic data/medical history**

After collecting a detailed medical history at Visit 1, this information will be updated at each visit through a structured interview, with a special emphasis on skin symptoms and signs such as rash, itching and exfoliation and on pregnancy in females. Participants will be instructed to communicate any change in their health status and intervening hospitalizations to the study coordinator in-between visits. In particular, they will be instructed to discontinue study medication and immediately contact the study coordinator if they develop a suspicious skin rash, swelling of the lips or mouth, arthralgias, and/or jaundice, which may indicate a hypersensitivity reaction to allopurinol. Fever and chills should also be reported but would not require cessation of medication prior to discussion with study personnel.

### **9.2 Skin exam**

The skin of study participants will be examined for the presence of any kind of rash at each in-person visit. Participants will be instructed to carry-out periodical skin self-exams. If skin abnormalities are reported to the study personnel during the phone visits or on any other occasion, participants will be asked to immediately report to the study site, their PCP's office, or other local healthcare facilities for an in-person skin exam. Suspicion of drug allergy or Stevens-Johnson Syndrome SJS would require immediate discontinuation of study medication and dermatologic consultation.

### **9.3. Vital signs**

Blood pressure and heart rate will be recorded at each in-person visit. BP readings at home will be reviewed during each phone visits; if abnormal values are reported, participants will be asked to visit the study site, their PCP's office, or other local healthcare facilities to have their BP measured.

### **9.4. Clinical laboratory tests**

Serum ALT, creatinine and K<sup>+</sup>, and CBC will be monitored and a pregnancy test, if a female of child bearing potential, performed at each visit. Participants who are started for the first time on RAS blockers as part of this study will have their serum K<sup>+</sup> and creatinine measured at a local laboratory after 2 weeks of full dose RASB treatment (i.e., after Visit 3). HbA1c will be measured at Visits 1, 4, and 7-17. An ECG will be performed at Visits 2, 4, 11, and 16.

### **9.5. Management of uric acid levels**

Study participants and study personnel, other than the DCC and the study pharmacists, will be masked as to the uric acid levels obtained during the study. The patients' physicians will receive written requests to refrain from measuring uric acid levels during the time of the patients' participation in the study, except as is mandatory for the patient's wellbeing, e.g., in the treatment of malignancy or diagnosis of a clinical syndrome highly likely to represent gout. If gout is diagnosed, open-label treatment with allopurinol will become indicated. In such case, the study drug will be discontinued but the patient will remain in the study and will continue to be followed as if he/she was taking the study medication. If uric acid lowering for malignancy treatment is required, the patient will receive open-label treatment until such time as return to study drug is deemed clinically reasonable by their physician.



## **10. ADVERSE EVENT REPORTING**

### **10.1. Definitions**

An Adverse Event (AE) is any untoward medical occurrence in a study participant regardless of its relationship to study treatment. A treatment-emergent AE is an adverse event occurring during the period between the first dose and 30 days after the final dose of the study medication. A Serious Adverse Event (SAE) is any untoward medical occurrence that results in death, is life-threatening, requires hospitalization or prolongation of an existing hospitalization, results in persistent or significant disability, or is a congenital anomaly/birth defect. Important medical events that do not fall into the above categories may also be considered an SAE when, based on medical judgment, such events may jeopardize the patient's safety and require medical/surgical intervention to prevent one of the outcomes listed in the SAE definition. The term SAE is not intended as a measure of severity or intensity. All AE's/SAE's that occur after the time of informed consent will be reported.

A Suspected Adverse Reaction is any adverse event for which there is a reasonable possibility that the drug caused the adverse event. For the purposes of IND safety reporting, "reasonable possibility" means there is evidence to suggest a causal relationship between the drug and the adverse event. Suspected adverse reaction implies a lesser degree of certainty about causality than adverse reaction, which means any adverse event caused by a drug. An Unexpected Adverse Event or Unexpected Suspected Adverse Reaction is an adverse event or suspected adverse reaction that is not listed in the investigator brochure or is not listed at the specificity or severity that has been observed; or, if an investigator brochure is not required or available, is not consistent with the risk information described in the general investigational plan or elsewhere in the current application, as amended. For example, under this definition, hepatic necrosis would be unexpected (by virtue of greater severity) if the investigator brochure referred only to elevated hepatic enzymes or hepatitis. Similarly, cerebral thromboembolism and cerebral vasculitis would be unexpected (by virtue of greater specificity) if the investigator brochure listed only cerebral vascular accidents. "Unexpected", as used in this definition, also refers to adverse events or suspected adverse reactions that are mentioned in the investigator brochure as occurring with a class of drugs or as anticipated from the pharmacological properties of the drug, but are not specifically mentioned as occurring with the particular drug under investigation. An Expected Adverse Event or Expected Adverse Reaction is any adverse experience that has been identified in nature or severity in the current investigator brochure and/or protocol.

### **10.2. Adverse Events Reporting**

All AEs will be reported on the Adverse Events form that will be completed by the study staff, who are masked as to study treatment assignment, at each regular follow-up visits. This will insure that AEs are ascertained in an unbiased manner using the same standardized methodology for participants in both treatment arms. Forms will include standardized questions relating to specific events of interest in diabetic patients on either of the study treatment arms as well as any significantly abnormal physical finding identified on examination and any significantly abnormal laboratory results obtained on the patient between visits or at the time of the visit. AEs reported or ascertained between clinic visits will be captured and reported at the time of the next schedule visit. Pre-existing conditions (that is, any condition that was known to be present prior to the signing of informed consent or was identified during the screening procedures at Visit 1) will not be considered or recorded as AEs unless the condition worsens in

intensity or frequency after Visit 1. Likewise, continuing AEs will not be reported as AEs at subsequent visits unless they increase in severity or frequency between visits, they result in criteria for a SAE, and/or they resolve between visits. Each site will be responsible for reporting all AE's to their IRB according to its AE reporting policy and procedures.

### **10.3. Assessment of causality and severity**

The seriousness of adverse events will be ascertained by the study staff according to the criteria listed in 10.1 and the need for further evaluation, follow-up, or referral. The relationship between study participation and AEs will be determined according to the following criteria:

**A. Not related** – temporal relationship of the onset of the event, relative to study participation, is not reasonable or another cause can by itself explain the occurrence of the event.

**B. Possibly related** – temporal relationship of the onset of the event, relative to study participation, is reasonable but the event could have been due to another, equally likely cause.

**C. Probably related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and the event is more likely explained by the study treatment than by another cause.

**D. Definitely related** – temporal relationship of the onset of the event, relative to study participation, is reasonable and there is no other cause to explain the event.

### **10.4. Serious adverse events reporting**

See Section 15 – Data and Safety Monitoring Plan.

## **11. STATISTICAL ANALYSIS**

This section presents a summary of the planned statistical analyses. A statistical analysis plan (SAP) will be written for the study that contains detailed descriptions of the analyses to be performed. The SAP will be written prior to database lock.

### **11.1. Analysis population**

For most of the analyses, including the primary efficacy analysis described in section 11.3, an intention to treat (ITT) analytical approach will be employed. Accordingly, the population for statistical analysis will consist of all randomized study participants considered in their original randomization group, regardless of treatment discontinuation or loss to follow-up.

Selected secondary efficacy analyses will be performed using a per-protocol analytical approach. In this case, the analysis population will consist of the ITT population excluding data points which 1. had cumulative exposure to the study medication from randomization that was less than 80% of the theoretical full exposure; or 2. during major protocol deviations (e.g., treatment with prohibited medications), which could affect primary outcome.

### **11.2. Initial data analysis**

The initial data analysis will be performed to detect any differences in distributions of characteristics measured at baseline, 4, 20, 36, and 38 months (0, 16, 80, 156, and 164 weeks, respectively) between study groups. The number of patients screened, enrolled, and completing the study will be summarized within and across study centers. Measures of central tendency (means, medians) and variability (standard deviations, ranges) will be estimated from the data for continuous variables. Frequency distributions will be provided for categorical data. This preliminary analysis step will provide us with insight into data, distributions of the variables considered, and will allow us to find additional invalid values not detected earlier during data validation.

### **11.3. Primary efficacy analysis**

For the primary endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention), we will follow the recommendations by Carpenter et al<sup>38,39</sup> and perform the analysis by means of a linear model for correlated errors with general/unstructured covariance matrix using all available iGFR measures (including those at baseline, 80, 156, and 164 weeks, respectively) as the dependent variable. By conditioning on the baseline iGFR measure we will also effectively use this variable as a covariate. Treatment group, study center, stratifying variables, albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did not qualify by eGFR slope and were normoalbuminuric at baseline), baseline AER, time, and time by treatment interaction will also be included as covariates in the model. Three features make this analytical approach especially attractive:

1. If there is no dropout (a very unlikely case), the estimate of the treatment effect at the end of the 2-month wash-out period following the 3-year intervention and its precision obtained using this approach will be exactly the same as those based on a classical approach employing an analysis of covariance (ANCOVA) model with treatment group, study center, iGFR and AER/ACR measured at baseline included as covariates.
2. If the iGFR measure at the end of the wash-out period is missing, we will be able to efficiently use the information contained in the intermediate iGFR measurements obtained at 80 and 156 weeks, by virtue of them being correlated with the GFR measurement at washout. Estimate of the treatment effect obtained this way is valid under the missing at random (MAR) assumption. This is in contrast to the ANCOVA approach, which would lead to the loss of this information and would require a more stringent assumption about the mechanism of data missingness, i.e. a missing completely at random (MCAR) mechanism.
3. The underlying analytical framework allows the use of all post-randomization data and is well suited to investigate the reason for withdrawal, for example to study whether participants having low iGFR values are more likely to withdraw.

Calculations will be performed using SAS PROC/MIXED. Results of the analysis will be expressed in terms of point estimate and its corresponding 95% confidence interval for the treatment effect at the end of the 2-month wash-out period following the 3-year treatment and will be accompanied by the corresponding p value.

### **11.4. Secondary efficacy analyses**

1. The effect of treatment on the iGFR at the end of the 3-year treatment period (before the washout) will be evaluated using the same analytical approach employed for the primary outcome.

2. The effect of treatment on the eGFR at 4 months after randomization will be evaluated using the same analytical approach employed for the primary outcome.
3. The iGFR and eGFR time trajectories, estimated from periodical iGFR measures and quarterly serum creatinine and cystatin C measurements using the CKD-EPI SCr and the CKD-EPI SCr-SCysC equations<sup>34,35</sup>, respectively, will be analyzed using linear mixed-effects models.<sup>40-42</sup> The main objective of the analysis will be to construct confidence interval for the effect of the intervention over three years of observation (treatment main effect) and investigate whether the effect of the intervention changes with time (time by treatment interaction).
4. Time to serum creatinine doubling or ESRD in the two treatment groups is subject to censoring due to dropouts or reaching the end of study before the participant experiences the event. Survival time will be defined as the time from randomization to the event (the first of serum creatinine doubling from baseline or occurrence of ESRD, defined as  $eGFR \leq 15$  ml/min/1.73 m<sup>2</sup>, hemodialysis, or kidney transplant) or, for participants who did not experienced an event, to the last study visit. Data will be summarized by means of Kaplan-Meier survival curves and by providing the proportions of participants surviving without events at 1, 2, 3 years, and at the end of the wash-out period along with their 95% CIs. Given the potentially small number of events, differences between study groups will be tested by means of the log rank test or by means of simple Cox regression models including a limited number of predictors in addition to treatment group.
5. The effect of treatment on the AER at the end of the wash-out period, based on the geometric mean of two AER measured at this time point and adjusted for the geometric mean of AER at baseline (Visit 3 and 4), will be investigated in a linear regression model framework as in the case of the primary outcome.
6. The effect of treatment on the AER at the end of the treatment period, based on the geometric mean of the AER measures at visit 15 and 16 adjusted for the geometric mean of AER at baseline (Visit 3 and 4) will be investigated as in #5.
7. Time to fatal or non-fatal cardiovascular events will be analyzed as proposed for time to serum creatinine doubling or ESRD.
8. We will perform a per-protocol analysis (as defined in 11.1) for the primary efficacy endpoint (iGFR at the end of the 2-month wash-out period following the 3-year intervention).

#### **11.5. Incomplete data**

Missing values represent a potential source of bias. Efforts will be made to keep all participants in the study. If this is not feasible, at least some information regarding the status at the end of the trial will be obtained. For randomized patients, the number of completing and dropouts will be summarized. This procedure will help to compare characteristics of the participants' groups who drop out from the study with those who completed the study by treatment group, within and across study centers. The models considered in the proposal allow for a missing at random (MAR) mechanism. MAR means that the missing values mechanism can be explained by observed data and does not depend on the *unobserved* values of outcome measures. The differences in distributions between characteristics of the groups may indicate potential sources of bias due to missing values. For instance, some patients may dropout from

the study due to *unobserved* factors related to the intervention itself. If we suspect such bias is present, the methods discussed in this section, assuming (MAR), are not applicable. We will incorporate plausible missing values mechanism into the model as discussed in Little<sup>43</sup> and investigate how such mechanism may affect the estimates of treatment effect. To this end, sensitivity analyses will be conducted involving selection and/or pattern-mixture models<sup>44</sup> with an appropriate submodel used to describe dropout.

#### **11.6. Pilot participants**

All pilot participants who were already randomized to allopurinol or placebo during the pilot will be included in the final analysis of the pivotal trial. Those who do not consent to the pivotal trial will be treated as having dropped from the study at a time corresponding to their last pilot visit. Sensitivity analyses will be performed to investigate whether results may be potentially affected by the roll-over of pilot subjects in the pivotal trial.

#### **11.7. Model assumptions and alternative analyses**

Model assumptions will be thoroughly checked for individual and systematic departures, using informal, e.g. inspection of residuals, and formal methods such as score test for extra parameter or methods based on likelihood displacement. If individual outliers are detected, their influence will be evaluated using influence diagnostics methods based on comparing estimates from models fitted to data with and without outlying values. Whenever we are not successful in fitting the parametric model (linear or non-linear), then non-parametric analyses and/or transformation of the variables involved in the analysis will be considered. To investigate the potential hemodynamic influence of allopurinol on treatment effect, in addition to the aforementioned analyses, we will consider models including the post-randomization measure of GFR at 4 months as an additional covariate. To investigate the possible presence of heterogeneity in the response to allopurinol, subgroup analyses (based on the primary efficacy analysis described in section 11.3, with the inclusion of an interaction term of the treatment group by the subgroup variable) will be performed by age groups ( $\leq 40$  and  $> 40$  yrs), gender, racial/ethnic group, HbA1c ( $\leq 7.8$  and  $> 7.8\%$ ), serum uric acid ( $\leq 6.0$  and  $> 6.0$  mg/dl), baseline iGFR ( $\leq 70$  ml/min and  $> 70$  ml/min/ $1.73\text{m}^2$ ), AER at baseline ( $\leq 300$  and  $> 300$  mg/24 hr), and albuminuria status (subjects who qualified by ACR or AER or were albuminuric at baseline vs. subjects who did qualify by eGFR slope and were normoalbuminuric at baseline). To investigate possible influence of using selected covariates on the treatment effect estimate in the models considered in Section 11, we will perform appropriate sensitivity analyses. These additional analyses will be considered as strictly exploratory.

#### **11.8. Safety analyses**

Adverse events will be independently reviewed by an independent data safety monitoring board (DSMB, see Sections 15 and 16). All safety data will be available in data listing in the clinical protocol report. Data will be described in terms of descriptive statistics and presented by treatment group. Presentation will include graphs (scatterplots, boxplots, histograms), measures of central tendency (mean, median) and variability (confidence intervals) for continuous variables and frequency tables for categorical variables.

#### **11.9. Interim analysis**

No formal interim analyses of efficacy to stop for benefit or futility are planned, given the timing of the primary endpoint.

### 11.10. Sample size

Since a variance-covariance matrix for the iGFR measures is not available and this matrix is essential in order to perform formal power calculations for a model with correlated errors, we performed alternative power calculations based on an intent-to-treat analysis within an ANCOVA framework. Specifically, we assumed that the primary hypothesis is tested in the following model:

$$M1: \text{iGFR at washout} = \text{iGFR at baseline} + \text{treatment group}$$

Compared to the model that will be used in the primary analysis, model M1 is simplified in two aspects. First, it does not use information from iGFR values measured at intermediate time points. Second, it does not include covariates such as the stratifying variables (HbA1c and UA) or other GFR predictors such as baseline AER. Both of these aspects may lead to loss of precision of the treatment effect estimate. Consequently, our sample size calculations should be considered as conservative.

The hypothesis being tested, i.e. the effect of treatment on iGFR at washout, corresponds to testing whether the treatment group factor in Model M1 is significant. The choice of the ANCOVA model for the purpose of power calculations is sensible, as residuals from a univariate model involving baseline iGFR as covariate fitted to data from RASS study conform to normal distribution. Sample size calculations were performed based on Cohen<sup>45</sup> and making the following assumptions:

1. Postulated effect on iGFR at washout ( $\bullet$ ) = 3 ml/min/1.73 m<sup>2</sup>. We deem this effect to be clinically meaningful and attainable. It is clinically meaningful because it would translate on average into a 10-year delay in the progression to ESRD. It is attainable because it is smaller than the difference in 3-year GFR that we observed in the JKS between subjects with serum UA  $\geq$  4.5 mg/dl compared to those with levels below this value. The postulated effect was based on the following changes in GFR levels in the two treatment groups:
  - a. Untreated group = 3 ml/min/1.73 m<sup>2</sup> per year. This estimate is based on data from the Joslin Kidney Study (JKS), in which the median GFR loss among 43 subjects meeting the above criteria was 3.1 ml/min/1.73 m<sup>2</sup> per year, with 70% of subjects having a GFR loss  $>$ 1.5 ml/min/1.73 m<sup>2</sup> per year. Also, among 116 subjects from Steno who met the albuminuria and GFR criteria, but for whom serum uric acid values were not available, the median GFR loss was 3.3 ml/min/1.73 m<sup>2</sup> per year, with 71% of subjects having a GFR loss  $>$ 1.5 ml/min/1.73 m<sup>2</sup> per year.
  - b. Treated group = 2 ml/min/1.73 m<sup>2</sup> per year. The average GFR loss in the JKS subjects with serum UA  $<$ 4.5 mg/dl was 1.5 ml/min per year. On this basis, we conservatively assumed that the allopurinol treatment, if effective, would decrease the GFR loss to 2 ml/min per year (a 33% decrease compared to the untreated group).
2. Standard deviation (SD) of residual error = 10.1 ml/min/1.73 m<sup>2</sup>. This was estimated based on the root-mean-squared error from a regression model with eGFR at 3 yrs as the dependent variable and baseline eGFR as the independent variable fitted to data concerning T1D patients from the Joslin Kidney Study meeting the PERL inclusion criteria.

Assuming a two-sided alpha error equal to 0.05, the effective sample size needed to detect the pre-specified treatment effect ( $\Delta = 3 \text{ ml/min/1.73 m}^2$ ) at washout adjusted for baseline iGFR with 80% power is equal to  $n=180$  per group. To take into account the anticipated overall dropout rate (up to 5%/yr or 15% over the entire duration of the study) and drug discontinuation or non-compliance in the treatment group (up to 2%/yr or 6% over the entire duration of the study), and to maintain the desired

power of at least 80%, it will be necessary to recruit  $n=240$  subjects per group. In Table 1, we show the power of the proposed sample size for Model M1 under different dropout and non-compliance scenarios. We also provide the corresponding power for a model (Model M2) including the two stratifying variables (Hb1Ac and UA) and baseline AER as covariates to illustrate the effect of adding these variables to Model M1. In this analysis, we assumed that adding these covariates reduces the residual variance by 10%, which corresponds to these covariates explaining merely 4% of the total iGFR variation over and above the variability explained by iGFR at baseline. As shown in Table 1, once these covariates are accounted for, power is expected to exceed the conservative estimates provided by Model M1 and reach almost 90% for 15% dropout and 6% non-compliance rates.

**Table 1. Power to detect treatment effect for two ANCOVA models under different drop-out and non-compliance scenarios.**

Overall Dropout (%)	Non-compliance (%)	Model	
		M1	M2
9	0	.87	.92
12	0	.86	.91
15	0	.85	.90
9	6	.83	.89
12	6	.82	.88
15	6	.80	.87

## Summary of Changes to the Statistical Analysis Plan

The first statistical analysis plan for the PERL status (version dated February 22, 2017) was prepared for inclusion as an appendix to the NIH application for renewal of the grant supporting this clinical trial. This SAP was approved by the Steering Committee but was not reviewed by NIDDK or the PERL DSMB.

A subsequent SAP version (dated May 14, 2019) was prepared as the trial was approaching completion. This was reviewed and approved by the Steering Committee, NIDDK, and the DSMB. The main changes in this version as compared to the previous one (dated February 22, 2017 were):

- Section 3. Rationale for adjustments of the SAP as compared to Protocol
  - Addition and justification of several adjustments to the analyses detailed in the Protocol (v10, dated March 6, 2018).
- Section 5. Study estimands
  - Re-framing of data analysis in terms of “Study estimands”, in order to follow ICH-E9 (R1) recommendations.
- Section 6. Analytical strategy:
  - 6.1. Study population.
    - Revision of the definition of the ITT and per protocol populations.
    - Addition of details regarding the imputation methods.
  - 6.3. Visit Windows.
    - Specification of wider windows for V11, V16, and V17 to ensure that all iGFR are analyzed.
    - Addition of procedure to avoid temporal overlap of iGFR and non-iGFR visits.
    - Creation of separate tables for non-iGFR and iGFR visit windows (Tables 6.3.1 and 6.3.2).
    - Addition of “Time since randomization attributed to visit window” to tables with visit windows.
  - Section 6.4. Baseline Covariates
    - Addition of methods to impute missing values for baseline covariates.
  - Section 6.5 (New) Missing values
    - Description of methods to account for missing values.
  - Section 6.6. Analysis of the primary estimand
    - Miscellaneous changes to make the analysis consistent with the re-framing in terms of Study estimands and for the more detail description of imputation methods.
    - Addition of the tipping point sensitivity analysis to check the robustness of the MAR assumption.
  - Section 6.7. Analysis of secondary estimands
    - Miscellaneous changes to make analysis consistent with the re-framing in terms of Study estimands and for the more detail description of imputation methods.
  - Section 6.8. Other analyses
    - Section 6.8.1 changes in cut-offs for stratified analyses to make them consistent with changes made in Section 3.



- Section 6.8.2. Addition of details about the metrics used to evaluate safety.
- Section 6.8.3. Addition of details on the additional analyses introduced in Section 3 (effect of post-randomization serum urate changes on iGFR at V17, effect of allopurinol on time to 40% eGFR decrease, effect of allopurinol on composite of serum creatinine doubling, ESRD, and CVD/renal death).
- Section 7. Mock Tables and Figures – Deleted.

Minor revisions were made to the SAP on August 3, 2019, right before the lock of the study database. These changes included:

- Section 6. Analytical Strategy
  - Section 6.4. Baseline Covariates
    - Clarification about the aggregation of sites with small numbers of randomized individuals within the baseline covariate “Clinical site”.
  - Section 6.5. Missing values
    - Clarification about the imputation of eGFR and iGFR values in subjects who started with low GFR values and did not develop ESRD.
  - 6.6. Analysis of the primary estimand.
    - Addition of Kenward-Roger approximation to estimate degrees of freedom.